POLITICAL THEORY

Markets in Historical Contexts: Ideas and Politics in the Modern World. Edited by Mark Bevir and Frank Trentmann. Cambridge: Cambridge University Press, 2004. 268p. $70.00.

— Peter Lindsay, *Georgia State University*

Since the fall of the Soviet Bloc regimes, it has become accepted wisdom that the market system has triumphed—that relying on the free, independent exchange between economic actors is a far superior way of allocating resources than attempting to "plan" from above. At a general level, this accepted wisdom is hard to dispute. Yet, like all such propositions, its heuristic usefulness cuts two ways: Embracing it wholesale makes for an easy bet; it also, however, encourages one to overlook a more complex and ambiguous reality that lurks beneath the surface. We might agree, for example, that markets confer myriad economic and political advantages. What we might less readily agree on, however, is what, precisely, we mean by "markets." As the essays in *Markets in Historical Contexts* make clear, if we mean institutions that are "not encumbered by geography, weight, unequal access to information, government regulation, or particularistic agendas" (p. 226) (not to mention cultural context), then we will quickly find ourselves running afoul of social science's ultimate limit: reality.

The goal of this interdisciplinary effort (with contributions coming from political scientists, historians, and sociologists) is to "retrieve" markets from the simplistic dichotomies of the twentieth century, that is, to demonstrate that in juxtaposing them to the state or to culture, we forget the degree to which they *are* the state and they *are* the culture (and the state and culture are them). Rather than overturn accepted wisdom, then, this volume seeks to better understand it.

Contributions follow two complementary lines of thinking: analyses of the actual place and role of markets in the development of modern societies, and analyses of how thinkers of the eighteenth, nineteenth, and twentieth centuries have understood that role and place. In the former vein, Rob Jenkin's essay on political legitimation and economic reform in India offers perhaps the clearest illustration of the book's central tenet that market activity needs to be seen from within the confines of culture, tradition, and ongoing social forces. While market ideology has reemerged in the India of the 1990s, its continued growth, Jenkins argues, rests less on *institutional* than *ideological* adaptability. To be more specific, market success will be determined by how well it competes with class-based issues, Hindu nationalism, and issue-based social activism. In such competition, all these ideological tendencies must interact with India's *swadeshi* (roughly, its economic nationalism), and it is this interaction that will not only determine which are successful, but on what terms. In establishing its *swadeshi* credentials, then, the market does in India what it no doubt must do in all social milieus: reflect the particularity of its time and place.

Patricia Maclachlan and Frank Trentmann's essay reinforces this point by suggesting that the recent spate of consumer protests might be better understood if we look less to the globalizing pressures of capitalism and more to the ways these protests "reflect and contribute to the longevity of domestic political cultures and local markets" (p. 201). In examining Japan, Britain, and the United States, the authors demonstrate that national definitions of citizenship and the public good play a strong role in determining whether consumer movements will flourish. Moreover, it is not just market activity in particular that is linked to the local. Maclachlan and Trentmann extend their conclusion to all social movements, suggesting that their "political 'success' . . . rests on the ability of movements to frame their objectives in ways that complement or contribute to broader cultural norms and prevailing ideas about democracy and political economy" (p. 201).

The final empirical assaults are on our understanding of how markets developed historically and where they are headed now. James Livesey's fascinating look back at eighteenth-century peasant agricultural societies adroitly demonstrates that much of what Karl Polanyi referred to as our "market mentality" came *to* markets from outside cultural sources, rather than emanating from them. And lest we believe that twenty-first-century global capital markets have finally thrown off all cultural shackles, Saskia Sassen reveals the "non-market and non-digital dynamics, agendas, contents [and] powers" (p. 225) that still remain.

If these essays complicate the real world of markets, the remaining essays complicate the ways we have historically viewed that world. David Eastwood's analysis of "High Tory" (e.g., Coleridge, Southey, and Sadler) opposition to the political economy of Smith, Malthus, and Ricardo serves to remind modern conservatives that the progressive Left was hardly alone in its condemnation of market austerity and its commitment to an active state. Markets have historically presented societies with features offensive to *both* sides of the political spectrum. Moreover, as Heinz-Gerhard Haupt argues, we often fail to see just how complex was the opposition from quarters traditionally thought precapitalist in orientation. In his essay on guild theory and organization, Haupt argues that in the hands of Gierke, Durkheim, and others, guilds provided the necessary buffer between the state and the individual, a buffer that, far from "annulling the mechanisms of the market," improved its chances of survival by "diminishing and moralizing its effects" (p. 104). The implication to consider here is that if the opposition to markets did not have the retarding effect on their growth that we have commonly presumed, then perhaps the dynamics of the markets that emerged in the wake of that opposition has also not been entirely as we have presumed.

None of these essays suffers from a lack of scholarly insight, and for that reason alone, this book is well worth the effort of those with intrinsic interests in the history of political economy and political economic thought. If there is a criticism to be found, it is one that stems from the (perhaps unfair) vantage point of readers in need of a more instrumental incentive. For those prone to asking the dreaded "so what?" when confronted with history, these essays are for the most part missing the hook required to draw them in. This is not to say that the hook is not there; it is. In fact, the message beneath this historical scholarship is of great importance to readers with contemporary concerns. The problem is that, for the most part, these essays fall far short of explaining why that might be.

Essays by Donald Winch, Jose Harris, and Richard Whatmore provide the most obvious examples here. In Winch's case, a rigorous look at the proto-environmentalism of Mill, Ruskin, and, to a lesser degree, Marshall leaves readers to sort out for themselves whatever parallels may exist with contemporary eco-strategies (e.g., the tension between its Luddite-romantic and its progressive socialistic elements). By contrast, Harris's concern with analytic imprecision looks initially to be more promising. As a correction to our slippery use of language, she offers a look back at Tönnies's famous *Gemeinschaft/Gesellschaft* distinction, arguing that it illuminates the "clashes of interest and principle in many major areas of public life" in ways that the "now largely exhausted dichotomies of socialism versus capitalism [and] states versus markets" (p. 144) can only partially accomplish. The problem here is that while the focus is in a helpful direction, the argument is less than convincing, as it is difficult to see how Tönnies's distinction could really help us better weigh the costs and benefits associated with the advance of modern bureaucratic societies.

Finally, Whatmore's historically astute treatment of Rousseau, Constant, and Say convincingly "complicates the historical record for those who continue to espouse a 'black and white' approach to the intellectual history of markets" (p. 69). But, a reader might ask, "to what end?" While we might rightly agree with his closing statement—"Say's belief that modern republics actually deprive the people of political and economic agency is a recurring theme in contemporary political culture" (p. 69)—we might just as rightly wonder what exactly to do with that insight.

In contrast to such academic caution, Axel Schäfer's examination of the German antecedents to late-nineteenth- and early-twentieth-century American progressivism will appear downright (and refreshingly) shrill. After concluding that "while progressive thought helped shape the consumer economy, welfare policies remained tied to the normative code of nineteenth-century producer capitalism," Schäfer uses this observation to shed light on an odd and easily overlooked aspect of our current state: "[W]hile the poor are chided for becoming 'dependent' on welfare, equivalent behavior patterns among the middle classes are defined as conducive to the workings of the consumer economy" (p. 167). This refreshing appraisal of the duplicity with which America's consumerist public ethos condemns with one hand what it praises with another reminds us of a simple yet—in this volume—sometimes overlooked truth: History matters.

Healing Identities: Black Feminist Thought and the Politics of Groups. By Cynthia Burack. Ithaca, NY: Cornell University Press, 2004. 224p. $42.50 cloth, $19.95 paper.

— Jane Flax, *Howard University*

Are groups necessarily destructive? This question is the focus of Cynthia Burack's new book. To address it, she constructs a conversation among psychoanalytic political theorists, psychoanalytic group theorists, and black feminist theorists. As the structure of the book makes clear, Burack hopes to broaden her audience to readers unfamiliar with any or all of these discourses. She intends to convince students of politics and feminists (particularly black feminists) that psychoanalytic theory can contribute much to understanding the dynamics of that ubiquitous feature of political life, groups. Furthermore, she wants to bridge the disciplinary gap between those who study groups and those who engage in discourse analysis. Discourse analysts are presently primarily located in humanities and cultural studies, and when they employ psychoanalytic thinking, it tends to be the Lacanian strand. Burack argues for a different tack, psychoanalytic object relations theory, particularly as articulated by Wilfred Bion, D. W. Winnicott, and Melanie Klein.

While she intends to instigate a conversation among equals, the reader senses that the most important dialogue is between Burack and psychoanalytic political theorists. These theorists are the primary source cited for the proposition that groups are necessarily destructive. Theorists such as Fred Alford claim that groups are inherently destructive because they construct and maintain identities in part by projecting unwanted qualities on outsiders, creating hated enemies as the other that bounds the group, and demanding that their members sacrifice individuality to effect a homogeneous and regulatory unity. In contrast, while Burack agrees that such dynamics can occur within any group, constructive ones also exist. As evidence, she offers black feminist theory. Its practices, she claims, are "reparative." Although black feminist theory seeks to create solidarity among black women and to repair wounds caused by the interwoven effects of race and gender domination, it does not do so through projection, enemy creation, and repressive norms of unity and authenticity. Instead, black feminist theorists stress honoring diversity among group members, forming coalitions with outsiders, and constantly negotiating differences, both intra- and intergroup. Further, since she asserts that group life is an ineradicable feature of social and political life, rather than seeking to eradicate identity group politics, students of politics ought to adopt black feminist practices as a model for doing them differently.

Although Burack's ideas about "reparative" groups are suggestive, the force of her argument is undercut by slippage and ambiguity in her deployment of central terms, including group and black feminism. The author is quite clear that she intends to analyze group discourse, not group behavior. However, sometimes she posits an at least implicit equivalence (if not identity) between group behavior and group discourse. For example, she argues that group processes occur within discourse, and that such discourses may offer the "most accessible source of information about the interplay of the political and psychological in the identity groups to which people are committed in everyday social life" (p. 2). She also claims that in the case of black feminist theory, discourse is itself a form of action (p. 62). She discusses accounts of group processes that object relational psychoanalysts derive from observing and intervening in them. However, she argues that while this strand of psychoanalytic theory correctly identifies common group dynamics, its accounts must be complicated by close attention to the specific practices and contexts that cause variations in group behavior. Attention to such practices may necessitate revisions in these psychoanalytic theories and political ones as well. The corrective practices she cites are black feminist discourses.

Burack also conducts a mostly subtextual argument against theorists who not only focus on the potential dangers of identity groups but also contest their desirability as a basis for political change. Such theorists, for example, Wendy Brown and Paul Gilroy, mount cogent arguments against identity group politics. The argument that identity group discourse can do reparative work is not sufficient to overcome these critiques. It avoids central questions, such as those raised also by Hannah Arendt, about whether such work ought to be a central or organizing purpose of political action at all. An equally important issue is whether identity formations, such as race and gender that are so interwoven with relations of domination, ought to be repaired or resisted. Furthermore, it is not at all clear that just because groups are endemic to political life, identity groups must be. As Michel Foucault and others argue, "identity" itself is a concept with a specific genealogy and is only one of the many ways subjectivity has been practiced. Its emergence as a site of political action is fairly recent.

Burack's own discourse made me more sympathetic to the claims of Alford and others about the dynamics of such identity formations. I found myself growing increasingly uncomfortable with her use of black feminist theory. She seems to fall victim to attitudes she herself warns against (p. 81). Black feminist theory appears as an idealized all-good object, located solely on the positive side of a binary reparative/destructive split. Furthermore, I do not think she finesses the problematic use of discourse to counter arguments based on observations of group behavior. As any political activist can attest, a vast gap often exists between the discourse of a group and its dynamics and effects. Such a gap, for example, between the discourse of equality and the practice of gender subordination motivated many women in the civil rights movements to start feminist groups. This is not to say that one of her central propositions is faulty. I agree that whether one is interested in increasing their efficacy or analyzing them, psychoanalysis has much to contribute to our understanding of groups. Whatever their stated purpose, a diverse, constructive, and destructive assortment of psychodynamic processes are endemic to groups. It is inaccurate to treat them as simple aggregates of utility-maximizing individuals. Nor are groups or their members solely rational actors whose pursuit of their stated goals is their only agenda or sufficient explanation for their dynamics or appeal. However, Burack could have made a stronger case for her argument by showing that black feminist theory has some of the reparative effect she claims.

Critics of identity politics might be more persuaded of its constructive possibilities had she met their arguments more directly. As presented in *Healing Identities,* unfortunately, Burack introduces much potentially rich material but is unable to produce the effects she intends.

The Modern Self in the Labyrinth: Politics and the Entrapment Imagination. By Eyal Chowers. Cambridge, MA: Harvard University Press, 2004. 260p. $49.95.

— Harvey Goldman, *University of California, San Diego*

This is an extremely rich and provocative work, wide in learning, filled with thoughtful interpretations of Max Weber, Sigmund Freud, and Michel Foucault, among others, and containing many insights into the ways that "modernity" and its consequences for the self have been conceived in the last century. It is also a disturbing work, pointing to what the author argues are apparently inescapable dilemmas for "us" posed by both "our institutions" and "our notions of identity" (p. 197). And it will be a very contested work, first, because of what I think are a number of questionable assertions the author makes about the views of those he discusses, particularly Nietzsche and Weber; second, because of the completely unhistorical and uncontextual methodology the author employs to throw light on these thinkers so close to us in time, yet immersed in such different social and cultural milieus; and third, because of the larger thesis and framework of the interpretation, which convey great depth of concern and sincerity, but which, I think, are very problematical as an interpretation of these thinkers. To deal with the book adequately would require much more extended treatment than a short review can give it here.

By entrapment, Eyal Chowers means "the dehumanizing sameness that springs from duplication of the social—the menace of homogenized existence in a world conceived as self-made" (p. 2). The entrapment thinkers, Weber, Freud, and Foucault, are distinguished by contrast to optimistic "proto-entrapment" thinkers of the nineteenth century, like Marx and Nietzsche—who believed in the possibility of revolutionary action or personal transformation as solutions to the "malaise of modernity" (p. 4)—and to "pessimistic" twentieth-century versions of such thinkers, like Walter Benjamin and Hannah Arendt—who still believed somehow in "redemption and hope" and in "modes of intervention in historical time" (p. 181). The entrapment thinkers all see the self "as trapped in the life-orders of modernity," though the trap looks somewhat different to each of them. Indeed, to Chowers, "perhaps they convey as a group a truth that transcends diverse theoretical frameworks and disciplines; perhaps, indeed, they constitute a loose 'school' of their own," for they all "reject the belief that human beings are the authors of history," capable of "steering the future in desirable directions." And their ultimate lesson is that, therefore, "the best we can do is to cope with its dehumanizing effects, mostly through individual projects" (p. 181). Chowers sees them and their project in quite romantic terms, praising their "dignity" (p. 186), their "courage" (p. 187), and their renunciations of hope.

Yet it is very hard to believe that we have gone through all of this analysis of modernity, of rifts in the self, and of experiences of being stymied, blocked, and entrapped only to come up with the claim that the ultimate lesson of these thinkers is that we should just stop fooling ourselves with groundless dreams of what we would like to see, be, and have, stop aiming so high, and, instead, just "cope." This is advice most people could get from their parents or friends whenever they happen to be going through a crisis or having a hard time.

Naturally, one should not dismiss wisdom just because it appears in a simple form; perhaps that is the highest wisdom, but the evidence of Weber, Freud, and Foucault runs quite against this. Where is Weber's great project for mastery of the order of the spheres of the world, on behalf of which he calls for a revival of the Puritan "calling" to permit self-mastery, control, and leadership of the political, economic, and intellectual orders of the world, as all charismatic leaders have done before? Where is the Freud who believed that he was the successor of Copernicus and Darwin in disturbing the "sleep" of humankind and who dismissed the rejection of his "truths" about sexuality as simply "resistance"? Where is the Foucauldian conviction that we have outgrown the need for moral imperatives to govern our conduct and are actually, not potentially, much freer than before, so that we can now take up the project of becoming artists of the self, dismissing all of history, tradition, and the demands of others, to renounce what everything and everyone has imposed on us, owing explanations of ourselves to no one? These are not projects of "coping," however much they may diverge from what Bernard Yack (1992) has called the "longing for total revolution" so desired by the optimistic and pessimistic "proto-entrapment" thinkers. They are grandiose, world-challenging projects, and that is why we still read them.

Although much of the textual work of this book is extremely good, especially on Freud and Foucault, Chowers has too often chosen to emphasize the features of all of these thinkers that seem to confirm his picture of their intellectual "school." At other places, his interpretations stretch what can be found in the texts, although some of his claims about modernity might be very helpful if he realized that they were his, and not those of Weber and the others (not a surprising claim, perhaps, from a Weber scholar like myself). More troubling is how completely these thinkers are divorced from the real interlocutors, concrete problems, and audience of the world they actually lived in, a consideration of which might have provided a more tangible understanding of why these particular issues emerged for these particular upper-middle-class intellectuals and why they thought of them as they did, rather than seeing these intellectuals as a kind of rupture with all thought until theirs, engaged in critique of a "modernity" as abstract and divorced from real experience as a Platonic form.

The Modern Self in the Labyrinth should be read for its interesting interpretations of the thinkers it treats, as well as for the author's own views of modernity. His views of the dilemmas he observes deserve to be expanded on their own, rather than read through Weber, Freud, and Foucault, because I do not believe that the author has persuaded us that his views and their views are actually in agreement.

The Green State: Rethinking Democracy and Sovereignty. By Robyn Eckersley. Cambridge, MA: MIT Press, 2004. 344p. $62.00 cloth, $25.00 paper.

— John Martin Gillroy, *Lehigh University*

Some argue that market democracies do not engage in war with one another, and therefore that if one promotes markets, franchise, and elections, or democratic-capitalist states, this will lead to international peace and cooperation. This idea has informed both the theory of international law (e.g., a right to democratic governance) and the practice of American foreign policy (e.g., Bush Doctrine). A counterargument is built on the suspicion that institutional political/economic process is largely independent of the propensity of a state to cooperate in international relations, and that a focus on democracy and markets as a cure-all for international dispute settlement distracts both theorist and practitioner from the real problems that plague the international system. These skeptics call the focus on the creation of democratic states the "consoling myth."

In *The Green State,* Robyn Eckersley gives another dimension to this myth with her assumption that a focus on democracy will create more ecologically conscious states. She begins with the premise that "[t]he history of modern grassroots environmental activism and the broader green movement has been . . . a history of attempts to address the problems of risk generation and risk displacement by seeking to extend and deepen democracy" (p. 109) and concludes that "[t]he case for deeper ecological reform is thus dependent on extending representation and deepening democratic participation" (p. 247). However, in the same way markets and democracy may be independent of war, democracy and grassroots participation may be independent of ecological reform and, if so, by focusing on green democracy and assuming it will create green policy, she introduces a "green consoling myth" that may cloud the real issues of state sovereignty, international law, and the environment.

Specifically, Eckersley asserts that because of its focus on the territorial state, capitalist markets, and what she calls its "democratic deficits," the liberal state is unfit as a foundation for the creation of the "green state" and therefore must be transcended through critical political ecology to what she calls "ecological democracy." But before one accepts that we can transcend to an ecological democracy, one needs convincing that it is necessary to do so. Two propositions therefore need argument: first, that these three characteristics of the liberal state (sovereignty, markets, and democratic deficits) are all obstacles to greening and, second, that the existing liberal state does not contain within itself the capacity for change toward greener policy; that is, the ecological democracy as a transcendence of liberalism is necessary for the genesis of the green state.

Eckersley's argument for the first proposition begins well as she convincingly makes the case that nature deserves more respect and status in policy choice and international law. Chapter 2 points out the shortcomings of the "anarchy" model of the current world system and argues that "the ecological crisis has the potential to transform the rationale and structure of exclusive territorial rule, and the identities and interests of states" (p. 49). In Chapter 3, the author rejects the dominance of markets and argues effectively for "ecological modernization" that would replace market motivations with greener alternatives. She is persuasive here that "[e]conomic competitiveness, after all, is not an end in itself" (p. 83).

The argument on the first proposition is unsettled by the "myth" of its third component, a concern for democratic deficits (Chapters 4–7). If there is a possibility that the *greenness* of a state is independent of how *democratic* it is, then she needs to argue for how, exactly, democratic inclusiveness renders increased environmental awareness and protection of nature before she moves on to argue the merits of different models of democratic theory. This is especially important, first, as environmental risk needs anticipatory policy not triggered by democratic responsiveness and, second, because life is not a *Far Side* cartoon where animals and plants can directly participate if granted the opportunity.

The irony is that Eckersley does not need this "deficit" component to support her ultimate recommendations. In Chapter 8, she concludes that the genesis of the green state is in constitutional law reform, human rights, recognizing the intrinsic value of nature, and the need for anticipatory policy through the precautionary principle. But the connection, if any exists, between these conclusions and the existence of more inclusive democratic institutions or "deficits" in participation is vague. She recommends 11 rights and responsibilities that would encourage the green state (pp. 243–44), but of these, only two involve democratic franchise, "a right to participation in negotiation of environmental standards" and "holding of cross-border referenda and reciprocal representation in deliberative forums," and even these seem as dependent on green administrative structure or a green judiciary as a green electorate.

The second proposition, that the liberal state is inherently inadequate, is undermined by the fact that the author comes to the conclusion, in a number of places, that the liberal/territorial state is already on the move toward a greener reality and that her definition of ecological democracy might exist within a reformed liberalism. In order to make this central part of the book work, she accepts the green caricature of liberalism as confined to instrumental value, market consumerism, and strong *Vattel* sovereignty, when the possibilities are more varied. For example, I have argued (*Justice and Nature,* 2002), that Kant's liberalism supports anticipatory policy, precaution in the face of risk, and a reversal of the burden of proof, causing a transcendence of the market for an "ecological contract" and an ecocentric worldview. I am now encouraged to apply this paradigm to international law to see if greening will really require us to "totally dislodge the tight nexus between citizenship, democracy, territoriality and sovereignty that is central to the regulative ideals of the liberal state" (p. 247).

This is a thought-provoking book and valuable for both its critical argument on the failures of the current state system to properly account for the environment and for its conclusions on rights, intrinsic value, precaution, and the need for constitutional transformation toward the green state. But the "democratic" part of the argument and its painstaking analysis of discursive and ecological forms of participation leaves the reader worried that expanding the consoling myth to include the environment will distract many excellent theorists from the real dilemmas of international law, politics, and sustainability.

Citizens Without Shelter: Homelessness, Democracy, and Political Exclusion. By Leonard C. Feldman. Ithaca and London: Cornell University Press, 2004. 224p. $35.00.

— J. Donald Moon, *Wesleyan University*

American attitudes toward the homeless tend to shift back and forth between compassion and compassion fatigue, between supporting policies to provide resources to the homeless and supporting punitive policies to exclude them from public spaces. This ambivalence is often explained by invoking "economic functionality" and/or "a cultural logic" (p. 5), but neither of these approaches is adequate. Rather, Leonard Feldman argues, we should see the problem of homelessness as a "problem of sovereign state power" (p. 18). Citizenship, and political life generally, are defined in opposition to "bare life," or "mere physical existence": Citizenship as full membership is constituted as the exclusion of bare life" (p. 18). Feldman continues, "Home-dwelling citizen and homeless bare life are political statuses, not social statuses or elements of personal identity" (p. 20), and so an adequate response to homelessness must also be political. He calls for a move toward a "pluralized citizenship" (p. 21), in which we deconstruct "the rigid oppositions between . . . bare life and citizenship." Acknowledging a plurality of ways of dwelling, we can then recognize "that those displaced from 'house' and 'home' must dwell . . . and that public policy should be oriented toward enabling dwelling, not criminalizing it or reducing it to the stripped down client relationship of the shelter" (p. 147). More specifically, rather than repressing the habitats and networks that the homeless have themselves created, we should recognize these communities, including, in particular, "politicized homeless encampments" (p. 107) as participants in the political processes through which we formulate policy.

Viewing homelessness as a political, rather than a social or economic, problem and its solution in terms of the extension of citizenship and political participation to the homeless represents an original and provocative way of reframing this issue. It is not without its difficulties, however. To start with the idea that citizenship is defined in opposition to bare life: However adequate this may be as an account of the ancient world, it seems problematic in the modern world. "Bare life" today seems very much a concern of the political realm, as all contemporary states mount vast and costly programs to provide for health care and to maintain an income sufficient for subsistence. The state, of course, provides far more than (the resources necessary for) bare life, but in doing so, it does not exclude it but builds on it.

One might also be skeptical of the effectiveness of extending citizenship and including the homeless in political decision making. In spite of his deep sympathy with the homeless, and his hostility to the stereotyping and punitive responses to them that are so prevalent in our society, Feldman tends to treat them in an undifferentiated way, blurring distinctions between poor families who have some member(s) normally employed and single adults who are mentally ill or who face serious substance-abuse problems, or between those whose homelessness is temporary or transitional, and those whose homelessness is more or less permanent. By all means, we need to attend to the "agency of homeless persons" (p. 24), but their capacities for agency appear to be quite diverse, and in some cases it seems a mockery to call on them to become politically active on their own behalf.

Nonetheless, it is easy to see why Feldman is searching for a new conceptualization of the problem of homelessness. Much of the book wrestles with what we might think of as the central dilemma of the welfare state, arising from its commitment to welfare or positive rights. On the one hand, it is easy to see how access to certain resources and opportunities is necessary for equal citizenship, since the exercise of agency—and there is no citizenship without agency; at best there can be only subjection—is impossible without those resources. On the other hand, to utilize those resources or take advantage of those opportunities requires that one be capable of certain functionings. One must have certain skills (say, literacy) and capacities (say, some ability to control one's impulses). And so we find that the provision of welfare carries with it various normalizing measures. This dilemma appears in a particularly acute form in the context of homelessness, as the author shows with great clarity and power. The seemingly opposed policies of compassion and repression turn out to be complements, as the creation of shelters, to take one example, legitimizes the state in banning public sleeping, and so in effect criminalizing the homeless. Shelters, particularly when they are part of an integrated program to address rather than simply ameliorate the problem, make demands on those who use them, such as that they be free of drugs or alcohol, and that they participate in job training or counseling programs. The flipside of compassion is compulsion.

The usual approach to this kind of problem is to develop standards to assess and balance the conflicting values involved. If we grant, for example, that prohibiting people from sleeping or meeting other essential bodily needs on the street is unjust unless they have "adequate" alternative places to meet those needs, the problem becomes one of specifying what is "adequate." Feldman is deeply critical of "shelterization," and it is easy to see the point of his criticisms, since shelters are too often dangerous, crowded, and unsanitary. Similarly, he heaps scorn upon policies intended to exclude the homeless from public spaces for the animus they show against the homeless. What he does not do is offer a careful examination of the harms (if any) that, say, public sleeping imposes on others, nor to set out the criteria that a housing program would have to meet to provide a reasonable alternative to the street, in light of those harms. And it is easy to see why one might want to resist that exercise, since it is far from clear how these values are to be assessed and weighed against one another. Instead, he attempts to reconceptualize the problem as one of enlarging political spaces so that the homeless become participants in the process of dealing with this issue. Although I am skeptical of the adequacy of that approach, *Citizens Without Shelter* makes an important contribution by exposing the limits of our current frameworks and by boldly proposing a new way of thinking.

Rights, Democracy, and Fulfillment in the Era of Identity Politics: Principled Compromises in a Compromised World. By David Ingram. New York: Rowman and Littlefield Publishers, 2004. 280p. $70.00 cloth, $27.95 paper.

— Farid Abdel-Nour, *San Diego State University*

David Ingram situates his work within the tradition of critical social theory and traces its normative impulse to the young Marx's aspiration for human fulfillment. However, in order to avoid the trap of utopianism, he adopts a pragmatic attitude and highlights the importance of compromises. At the same time, by remaining cognizant of the price paid for normative compromises, he is able to maintain his pragmatic attitude without losing sight of the aspiration for human fulfillment and perfectibility that inspires his work.

This book is an attempt by Ingram to bring this impulse to bear on the "the problematic intersection of democracy and multiculturalism" (p. 2) in order to yield an understanding of a progressive identity politics consistent with democratic struggle. Democratic fairness, he argues, does not preclude attention to the special needs and claims of identity groups, nor does it necessarily reject the idea of group rights. To navigate this intersection, Ingram, following François Lyotard, offers two models of identity, each of which has applicability to individuals as well as groups. He distinguishes between a somewhat rigid "separatist-preservative" model of identity and a more fluid "synchretic-transformative" model, and finds occasion throughout the book to rely on both in different contexts. As part of his attempt to navigate the intersection of democracy and multiculturalism, he also offers a theory of rights that "supplements formal rights with substantive entitlements" (p. 9) and that is attentive to the disabling effects of capitalism everywhere, especially in the global North–South gap.

The bulk of the book is simply divided into three parts, one on identity, one on democracy, one on rights, and offers in one early chapter and concluding remarks a thin, abstract overall frame, which I have tried to reconstruct in the above paragraphs. The chapters within each of the parts deal in detail and very insightfully with different theoretical debates of great political significance under the topic at hand. For example, the part on identity contains an insightful chapter on race and one on disability. In each, Ingram makes a compelling argument for resisting the temptation to reduce all identity groups to the image of cultural groups demanding recognition. He argues convincingly that neither racial politics nor disability politics can be squeezed into that box. Furthermore, he argues for the need to distinguish between legitimate groups that can make demands for group rights and illegitimate ones whose identity is better comprehended along the synchretic-transformative model of identity. Here he uses the examples of whites in the United States and "deaf world" as identities that cannot be well understood via the separatist-preservative model and that do not have legitimate grounds for group rights.

The other two parts of the book offer equally compelling chapters, with the part on rights being more illuminating than the one on democracy. In dealing with international justice, Ingram makes a particularly insightful distinction between attempts at justifying human rights, on the one hand, and attempts at their democratic legitimation, on the other (p. 198). Justification is a tall order in a pluralist world, for it requires that people across deep cultural divides be rationally convinced of the truth and rightness of beliefs undergirding human rights. The democratic legitimation of these rights, on the other hand, merely demands that such people be convinced of the duty to act according to their dictates. Conviction of one's duty to act in a particular way may, but does not necessarily, involve questions of truth and rightness and certainly does not require consensus over the truth and rightness of any beliefs. Ingram's contribution to this intuition, which underlies John Rawls's idea of an overlapping consensus, is to bring in Jürgen Habermas's discourse ethics and the democratic deliberative processes that it entails. The result of this move is to allow the participants in legitimating discourses over human rights to continue to aspire toward the ever-greater perfectibility of the human rights regime. In time, they can potentially seek agreement over a richer set of rights than the short list of basic rights that Rawls's overlapping consensus accounts for between reasonable peoples.

As the reader will have detected by now, Ingram has a very ambitious purpose and a project with multiple nodes that could have been approached in any number of ways. He could have set out to work out a theory for a politics of identity and subordinated discussions of democracy and rights to that purpose. Or, perhaps more promisingly, he could have focused on the challenges facing the discourse of human rights in a pluralist world and looked toward the questions of identity and deliberative democracy with that purpose in mind. He might even have chosen to make this a treatise on method and to make the tension between pragmatism and perfectibility the dominant theme of which all other discussions illuminate an aspect in a particular context.

Part of the attraction of *Rights, Democracy, and Fulfillment in the Era of Identity Politics* is that it refuses to succumb to any easy options. It does not explicitly follow any of the above paths or any dominant organizing principle that I could detect. But this is also its weakness. There is a way in which it constitutes a collection of interconnected insights and discussions, many of which are mutually illuminating, but which never quite gel into an explicit whole. I have found it rewarding to read the book as a collection of essays, and have been unable to reap the benefits of the author's apparent efforts to make it into something more. I am left with a sense of a missing level of theorizing to mediate between the overly abstract purpose articulated in the early and late chapters and the very detailed, rich, self-standing discussions in each of the three main parts of the book. Ingram clarifies a number of extremely important points about identity politics and theories of rights, including human rights, political representation, and theories of jurisprudence. He works out some important pieces of potentially larger claims and establishes a number of interesting distinctions, some of which I have alluded to. But the project has yet to take the shape of an elegant, explicit, well-developed theory of democratic politics in a pluralist world. I look forward with some anticipation to its future development in such a form.

Desolation and Enlightenment: Political Knowledge After Total War, Totalitarianism, and the Holocaust.
By Ira Katznelson. New York: Columbia University Press, 2003. 208p. $29.00 cloth, $17.50 paper.

— Lisa Disch, *University of Minnesota–Twin Cities*

This eloquent volume, which originated as Columbia University's Leonard Hastings Schoff Memorial Lectures in 1997–98, has a dual mission. The first is personal. It is a "critical homage" to the post–World War II scholars who were Ira Katznelson's teachers. Practicing their craft "in the midst of a moment stamped by the greatest shocks and stresses the Enlightenment tradition had ever faced," these scholars produced a powerful revision of American Enlightenment liberalism that Katznelson believes could serve as a guidepost for today (p. 157). Confronting the "twentieth-century compound of total war, totalitarianism, and holocaust," they would neither simply reaffirm the faith in reason and science as secular grounds of moral progress nor simply repudiate ideals of autonomy and freedom as "mere fantasy or, worse, the main source of radical evil" (pp. 33, 39). They "sought instead to renew and protect the Enlightenment's heritage by appropriating and transforming social science, history, and the study of public policy" so as to provide illumination in the face of desolation (p. xiii). Katznelson's principal aim is to show how the work of this group might make possible a subtle intervention into "today's fervent but thin controversies about social inquiry and the status of Enlightenment" (p. xv).

The second mission is professional. Katznelson calls for an intellectually richer, more expansive practice of political science, one unlike that which takes pride of place in the discipline's flagship journal. He offers a mordant description of reading the *American Political Science Review,* describing it as "an exercise in applied schizophrenia, the result of the stark disjuncture dividing description and analysis from judgment and normative purpose" (p. 161). Echoing the demands of the recent "Perestroika movement" (2000–2001), that broad coalition of political scholars who called the discipline to task for the hegemony of hard science methods in the study of politics, Katznelson calls for a more integrated and engaged practice of political study. He argues for combining the "deduction of politics from norms with its extrapolation from facts, affiliating engaged social criticism with disinterested social science to discover truth about how things work" (p. 3).

Are these two missions not at odds? Is not the period to which Katznelson lays claim as a personal legacy a founding moment for institutionalizing the very bifurcation that he takes it as his professional mission to override? Indeed not. The central provocation of this volume is his assertion that the postwar period gave birth to a lost intellectual tradition: the "political studies enlightenment" (p. 3). This intellectual project eludes the typical cartography that maps the discipline in terms of an empirical versus normative divide. It bound together such diverse thinkers as Hannah Arendt, Robert Dahl, Richard Hofstadter, Harold Lasswell, Charles Lindblom, Karl Polyani, and David Truman—scholars who would not typically be featured on a common syllabus, let alone be credited with a shared program. Yet Katznelson insists that they ought to be. He contends that their intellectual commitments are best grasped through the "prism" of Karl Mannheim's injunction: Replace "a thin behavioralism, concentrating on the externally perceivable and 'content to attribute importance to what is measurable merely because it happens to be measurable,' [with] a new and deeper set of systematic studies suspended between empiricism and 'truth'" (p. 164).

Katznelson devotes the body of this volume to what he calls an "intellectual history of the political studies enlightenment" (p. 153). This is an effective, albeit disingenuous, rhetorical move insofar as he represents himself as *documenting* an overlooked postwar tradition. I believe that what he undertakes in this book is more creative and controversial: He invents the "political studies enlightenment" as a tradition that is to serve as both inspiration and legitimating ground for those contemporary scholars whose work does not conform easily to today's demarcations of subfield and methodology.

Given his desire to persuade his readers to see the political studies enlightenment as a single tradition, it is puzzling that he chooses to divide it into two "wings"—one "Europe-oriented" and the other "specializing in the United States"—and to develop his argument by devoting a chapter to each in turn (p. 112). The effect of this organization is both to reestablish the gap that he sets out to bridge and to suggest that the political studies enlightenment exists almost exclusively as an effect of Katznelson's interpretive lens. He offers no evidence that there was collaboration and contact across its two "wings" but rests his case almost exclusively on provocative rereadings of classic texts.

The author begins with the European wing, reading the work of Arendt and Polyani, specifically *Origins of Totalitarianism* and *The Great Transformation,* as exemplars of "historical social science" (p. 64). He emphasizes how the two "stand out for their risky, difficult, and compelling efforts to combine structural and moral periodizations" (p. 54). He also notes that by virtue of this combination, both texts, but especially *Origins,* are either wrongly overlooked by the "social and political sciences," or read one-sidedly (p. 61). He chastises "empirically oriented political scientists" for being so put off by Arendt's "Heideggerian style" that they fail to appreciate that "Arendt in fact performed like a very good political scientist of comparative politics" (pp. 119, 120). Thus, they overlook how her project and practice can be linked to that of the American political science mainstream.

Katznelson's portrait of the United States–focused wing of the political studies enlightenment is the most unorthodox aspect of his argument. He rebuts commonplace accounts of this period—understood as a time when scholars turned away from the state to study the political process, when quantitative methods effected a sharp divide between normative and empirical analysis, and when empirical scholars institutionalized a complacency about United States institutions—by assigning the Columbia University Seminar on the State to the position that behavioralism typically holds as its defining intellectual force. This seminar was a locus for inquiry by a methodologically plural group of scholars who gathered not in a spirit of optimism but in anxiety regarding "the character and security of liberal states and their abilities to advance pluralism while coping with its perils" (p. 120). The state remained their central object of analysis, although they aimed to make its study "realistic and behavioral" by treating "rules and institutions," rather than metaphysical ideals and singular interests, as the means for integrating a "diverse and fractious country" (p. 130). Katznelson identifies this concern, to study the modern state as "the cord by which the Enlightenment and desolation are tethered," as a defining project of the political studies enlightenment, one that exemplifies its capacity to contend with the darker sides of progress (p. 120).

Desolation and Enlightenment is a provocative retelling of the history of twentieth-century political science. Contrary to the complacent portrait of the 1950s, he demonstrates that there was already at midcentury a revision of the Progressive Era optimism regarding science and the state that was initially so formative for the discipline's mainstream. In addition, he starts a new conversation about the history of the discipline by insisting that this revision was initiated not only by refugee scholars but also by the American political science mainstream. This is a reinterpretation of the postwar period of political science in America that is sure to be controversial. It promotes the postwar period, a time that most political theorists demonize as the origin of an apolitical turn to a hard science model of political study, as a resource for a more ecumenical, less balkanized version of the discipline.

This was, for me, the most inspiring thread of the book. How many of us who teach contemporary political thought would present Polanyi and Dahl as allies? How many of us who teach philosophy of social science would group them as methodological kin? Katznelson prompted me to ask these questions. He also prompted me to think about how I reproduce on my own course syllabi the very distinctions of subfield and methodology that movements like Perestroika have so productively challenged. As a contribution to that struggle, as well as to the more abstract debates about the meaning and place of Enlightenment ideals in contemporary political life, this book is timely and important.

Language Rights and Political Theory. Edited by Will Kymlicka and Alan Patten. Oxford: Oxford University Press, 2003. 368p. $99.00 cloth, $24.95 paper.

— Chandran Kukathas, *University of Utah*

Language rights and language policy are significant issues in contemporary politics and have become an important subject for political theorists today. Yet until now, there has been no major work or edited volume dealing with language rights from the standpoint of normative political theory. Will Kymlicka and Alan Patten have put together a volume of essays to remedy this situation. According to the editors of this valuable collection, linguistic diversity has emerged as a major source of controversy in a number of distinct political contexts. In their comprehensive introduction to the topic, and the volume, they identify these contexts as including at least five areas: Eastern Europe, regional languages/minority nationalisms, immigrant integration, European Union/transnational democracy, and indigenous languages/biodiversity. As political theorists in recent years have explored ideas of citizenship, nationhood, multiculturalism, and deliberative democracy, it has become increasingly evident, they say, that political theories often rest on presuppositions about people's language repertoires. It is important that these presuppositions, and their implications, be explored and subjected to critical scrutiny. The aim of this collection is to do precisely this. In the end, it is entirely successful in its ambitions. The chapters are of high quality and deal with issues that are important. Anyone presently interested in working on language rights in political theory should begin here.

There are 13 essays in *Language Rights and Political Theory,* all of them worthy of discussion. The following merit special mention, at least because they offer theses that are particularly challenging. Philippe Van Parjis's essay, "Linguistic Justice," asks us to consider a case for subsidized language training, arguing that native speakers of dominant languages have an advantage over those who have to learn a second. Particularly in a world in which English is the lingua franca, there is, according to Van Parjis, a case for native speakers of English subsidizing those who would remain at a disadvantage without the linguistic tool that dominates world politics, commerce, and scholarship. The case is made by assuming at the outset that differences in the costs borne by some groups to gain benefits all should be able to enjoy have to be justified: Those lucky enough to be born into families speaking a dominant language have no automatic entitlement to the advantages that brings. For Van Parjis, linguistic justice demands that the linguistically rich compensate the linguistically poor—preferably with cash transfers (p. 168).

I have two observations. First, it is not clear why linguistic inequality needs to be singled out as a basis for justifying wealth transfers. Given that it is only one variable that impacts upon inequality more generally, if one wants to justify redistributing wealth, why complicate matters by isolating this particular asset or talent for separate calculation? Is a surfboard rider entitled to get any more money if his mother tongue is Korean? Second, if there is a case for addressing linguistic inequality as a problem of distributive justice, or of what Van Parjis calls "cooperative justice" (pp. 154–56), perhaps there are other inequalities that should also be addressed in similar fashion. For example, people who do not get skin cancer produce a public good insofar as they do not require expensive medical treatment that could be a drain on health-care expenditures—they keep either our taxes or our health insurance premiums lower. Yet those with dark skin are much less likely to get skin cancer because of the advantages their pigmentation brings, while those who have white skin have to spend money on sunblock, hats, and extra clothing—or air-conditioning if they are forced to stay indoors on sunny days—to contribute to this public good. Should hats, air-conditioning, and sunblock be subsidized to deal with this injustice; or should we establish a system of cash transfers from blacks to whites? Does the case against doing this rest on the fact that the transfers involved would be trivial, or is the whole idea of continually extending the scope of distributive, or cooperative, justice simply absurd?

Van Parjis's argument also relies upon an assumption that is controversial, and which sparked substantial controversy among some other essayists in the volume. This is the assumption that having to learn a second language is a liability, rather than an asset. On this question, Stephen May's essay, "Misconceiving Minority Language Rights," takes issue with the arguments of Thomas Pogge ("The Rights of Hispanics in the US") and David Laitin and Robert Reich ("A Liberal Democratic Approach to Language Rights"). Pogge, for example, argues that any support for teaching Spanish in U.S. schools must be constrained by "an overriding commitment to the interests of the children that our schools are supposed to educate" (p. 121), and that this means promoting English first. Along with Laitin and Reich, Pogge is concerned that speaking English is the key to social mobility, and that encouraging minority languages disadvantages children who will grow up less proficient in the language of mobility. Yet, as May points out, it does not look as though African Americans have found that speaking English for 200 years have kept them out of urban ghettos. Equally, the advantages of bilingualism are all too often overlooked by theorists, even though most universities seem to recognize the benefits of having a second language in insisting that their students devote some attention to acquiring knowledge of a foreign tongue. (If this point is sound, and we admit how badly most English-speaking university students learn other languages, then perhaps should native speakers of English be compensated for being deprived of the incentive that will ensure that they learn a second language?)

These discussions make for only a small sample of the many problems addressed and arguments developed by the essays in this excellent collection. While all the chapters may be read with profit, I would make special mention of a few others. Idil Boran's "Global Linguistic Diversity, Public Goods, and the Principle of Fairness" offers an acute analysis of the way in which the conceptualization of linguistic justice bears on arguments for the protection of linguistic diversity. Jacob Levy's "Language, Literacy and the State" provides a characteristically insightful assessment of the problem of minority language protection, considered in light of the very different imperative to protect minority speakers. And Alan Patten's "What Kind of Bilingualism?" is an especially instructive essay on the strengths and weaknesses of two alternative understandings of language rights, one attaching rights to persons and the other to territories. Finally, the long introduction to the volume written by the editors is itself a masterful survey of the field.

AMERICAN POLITICS

School Board Battles: The Christian Right in Local Politics. By Melissa M. Deckman. Washington, DC: Georgetown University Press, 2004. 240p. $39.95 cloth, $26.95 paper.

— James L. Guth, *Furman University*

Although it is almost conventional wisdom that the Christian Right was born in local political controversies, often over public education, scholars have usually focused on the movement's role in national electoral politics. Melissa Deckman restores some balance with an interesting study of Christian Right activity in school board politics. Although the book provides only a good start in that endeavor, it suggests many avenues for future research.

Deckman begins with an historical survey of Christian Right activity in educational politics, reviewing the battles over evolution, anticommunism, sex education, and "secular humanism" and introducing key actors in the movement's educational efforts, including textbook watchdogs Mel and Norma Gabler of Texas and Robert Simonds of Citizens for Excellence in Education, as well as broad agenda groups, such as the Christian Coalition, Eagle Forum, and Focus on the Family. As a part of their programs, several of these groups encouraged Christian activists to run for local school boards during the 1990s. The nature and success of these efforts constitute Deckman's main concern.

The data come in two forms. The first source is a mail survey of a stratified national sample of school board candidates ($N = 671$), while the second entails intensive case studies of two school boards with significant Christian Right representation, one in suburban Fairfax County, Virginia, the other in rural Garrett County, Maryland. Both the survey data and case studies provide useful (and sometimes differing) insights, but the case studies emerge as the most fascinating, revealing both the diversity of school board politics and the complexity of Christian Right responses.

The survey confirms some suspicions about Christian Right candidates, but it dispels even more. Using alternative definitions of "Christian Right" candidates (based on relationship to movement organizations and adherence to conservative policy positions), Deckman compares them to other office seekers. Not surprisingly, Christian Right activists often belong to orthodox congregations, are quite active in church, and hold more conservative policy views than their counterparts. Still, this is not a gulf between the "religious" and "secular": Non-Right candidates also tend to be religiously affiliated and active, but more often in mainline Protestant or Catholic communities. And although some interesting demographic differences appear (e.g., Christian Right candidates more often run their own businesses), both groups are drawn from the more privileged sectors of the citizenry.

The similarities extend to other areas. Most candidates are drawn into school board politics by purposive incentives, desiring to influence policy, with few differences among groups—although Christian Right candidates are more likely to mention concerns about the moral or religious climate of the schools. Most candidates get primary encouragement from friends and family to run, but some are prompted by sitting school board members. Contrary to expectations, Christian Right candidates do not report any substantial urging by Christian Right interest groups, although a few acknowledge being pushed by fellow church members. On the whole, though, school board candidacies are "self-starting," with encouragement and support mostly from friends and neighbors.

Deckman also questions the standard account of how conservative Christians come to dominate local school boards. On the basis of a few episodes in California and Florida, some journalists have reported that Christian Right candidates win election by via a "stealth strategy," hiding their real views on educational issues, confining their campaigns to the venue of conservative churches, and avoiding any "public" discussion of their priorities during the campaign. Whatever its validity in the original locations, this description is confirmed neither by the survey nor the case studies. The survey shows that although Christian Right candidates do not stress their more controversial ideas (e.g., creationism or vouchers), they leave no doubts about their conservative views on education, views that often have considerable public support, especially in traditionalist areas. Nor do they campaign much in conservative churches or draw support mostly from religious activists. In the case studies as well, Christian Right candidacies were openly conservative and highly visible, and they often had considerable public backing.

What happens when they win? The most fascinating section deals with the question of how Christian Right members participate in school governance. Once again, stereotypes are shattered—or at least cracked. In Garrett County, a Christian Right majority sought to institute major changes in policy, but often yielded to resistance from professional educators, pressure from community groups, and sometimes to internal factionalism. Thus, the Christian Right majority pursued its objectives, but those objectives changed as a result of interaction with the community. In Fairfax County, a Christian Right minority was frustrated in advancing its goals, but cooperated with other Republicans to stymie the more liberal (and less popular) policies of a Democratic majority. In neither case did the Christian Right faction exceed the boundaries of democratic politics: A majority often gave in to community pressures, and a minority found legitimate ways of influencing school policy. As Deckman rightly concludes, "At the very least, the experiences of Christian Right school board members challenge many of the common preconceptions about conservative Christians who govern" (p. 165). Of course, this conclusion derives in large part from case studies, as did the earlier alarmist accounts. Nevertheless, the scenarios she describes are probably at least as common as the others, if only because they comport well with her systematic survey data on school board candidacies.

Aside from the substantive merits of *School Board Battles,* it is also a pleasure to read: The findings are discussed in lucid prose, with a minimum of jargon. The only discordant note comes from Deckman's sporadic effort to assimilate the findings onto models of "priestly" versus "prophetic" religious politics, otherwise dubious analytic categories that seem especially ill suited for a discussion of educational politics. Fortunately, these interludes are but a brief annoyance and do not detract from the many valuable contributions of this work.

For Better or Worse: How Political Consultants Are Changing Elections in the United States. By David A. Dulio. Albany: State University of New York Press, 2004. 289p. $71.50 cloth, $23.95 paper.

— Jeffrey M. Stonecash, *Syracuse University*

This analysis addresses two important developments in American political campaigns: the increase in the presence of political consultants and their effect on campaigns. In pursuing this analysis, David Dulio focuses on the central questions of whether consultants are changing the nature of campaigns and whether their presence has displaced the role of political parties. The analysis draws heavily on a survey conducted in 1999 among 505 political consultants by American University staff.

Dulio first traces the rise of the consulting industry and its relationship to the rise of candidate-centered campaigns. He argues that technological developments and dealignment combined to make consultants much more important. Computers led to electronic voter registration files. The widespread availability of computers and software programs made it easier to use those files for analyzing the voting patterns of registrants, pulling samples for polls, analyzing survey results, and selecting specific sets of voters to receive direct mail with specialized messages. These abilities became more important to candidates as the number of independents and swing voters increased. When partisan voting was high and split-ticket voting was low, there was little reason to employ these talents, but as partisan voting declined, the hiring of consultants who were skilled in using the technology to find and access these "movable" voters became very important.

The author then focuses on perhaps the central question that troubles many about consultants: Do they tell candidates what to say they believe in? That is, are polls conducted and then candidates largely advocate what is most popular in the political district, regardless of what the candidate believes in? As in *The Candidate* movie, do candidates just become vehicles for what consultants find will "work" to win elections? Dulio argues that this does not occur. Instead, consultants help candidates figure out how to articulate what they believe in and present it in a manner that is compatible with most of the electorate while downplaying what is not compatible. Surveys are first used to "find which of the 'candidate's traits match best with the qualities the voters are seeking' and the issues on which they are the most compatible" (p. 71). "Consultants 'help candidates understand how, but not where [in terms of issue positions or ideological stance] to position themselves" (p. 84). "Consultants help candidates define their campaign message through extensive research on the electorate and the candidate's opponent" (p. 82). "It is the consultant's job to put the candidate in the best light possible, but they cannot change a candidate's prior voting history or their publicly stated positions on issues" (p. 74).

I have done polling for candidates for 20 years, and Dulio has this exactly right. Polls are conducted to find out if a candidate has an edge on some issues (which he should stress) and not on others (which he should try to avoid). If most voters are pro-choice and she is pro-choice, and the opponent is pro-life, this position must be stressed. If the public is opposed to tax increases, but the incumbent voted for a tax increase during a recession to provide state aid for schools, then this poll information is valuable for knowing that the incumbent is vulnerable on this issue and that the issue has to be framed as making a tough choice only for the sake of helping the schools. The incumbent cannot change her position, but having knowledge of how this could be a problem means taking the initiative to try to positively define the action. Some issues can be exploited (pro-choice on abortion) and some must be justified to the electorate. A vote for a tax increase cannot be changed or ducked, but it can be explained. The analysis of voting districts and the technologies of direct mail then make it possible to focus relevant messages for specific audiences.

While the argument is well presented by the author, this section could have been improved somewhat with one or two case studies indicating how it plays out in specific situations. That might help convince skeptics that polls do not determine the positions of candidates, and it might demonstrate how polling information helps candidates decide how to present their views and explain past decisions. While that would help, Dulio still does an excellent job presenting his general argument.

The next major issue is the relationship between consultants and political parties. Dulio presents a nuanced and interesting argument. He argues that consultants first became important because parties were struggling with the role they should play as technology and dealignment emerged. Candidates embraced the skills of consultants simply because they needed some way to get to voters. He argues that parties, adapting in general to a changing world, then began to move to incorporate consultants, drawing upon their resources to help their party candidates. The national parties initially tried to develop "in-house" capabilities by building media studios, but they soon realized that keeping up with changing technologies was expensive and that it would be better to contract with private consultants to draw on their skills, while letting the private entities pay the costs of upgrading equipment. Eventually, parties realized that they could engage in a division of labor in which the parties themselves raise money and direct it strategically to contests that are most important to the party and might be won, but allow consultants to do what they do best: use technology to conduct polls, analyze results, and target messages to voters (p. 125). His argument is that parties and consultants, despite the popular image, are not adversaries but that ultimately they have become allies, with each doing what they do best. Again, some examples of how the two interact in specific situations might help, but the argument is clear and persuasive.

In *For Better or Worse,* Dulio develops an analysis of the role of consultants that focuses on the central issues of whether they are altering campaigns and pushing political parties aside. He produces a well-developed argument that consultants have not taken over campaigns in some sense, but that they serve the goals of candidates and parties. It is an argument worth reading.

Polls and Politics: The Dilemmas of Democracy.
Edited by Michael A. Genovese and Matthew A. Streb. Albany: State University of New York Press, 2004. 192p. $54.50 cloth, $17.95 paper.

— Robert M. Eisinger, *Lewis & Clark College*

Reading a book that germinated from conferences is a bit like eating at a recommended restaurant in an unfamiliar city. Tasting untried food [for thought] is heightened by the novelty of the locale. Chapters serve as courses; some are better than others, and in the end, one often is pleased by the experience, even if the parts of the experience were in need of refinement. *Polls and Politics* emanates from a 2002 Loyola Marymount University conference on the topic. The book unites leading scholars in the field, as they examine questions about the role of polls in American democracy, and it is entertaining, provocative, and at times flawed. The flaws, however, are not in a particular author's argument, but rather in what is omitted in the collected volume. It highlights the dilemmas associated with public opinion polling in the twenty-first century, but does little to aid the authors in seeking ways of measuring or mitigating those flaws.

This book concentrates on presidential and media polls, and arguably justifiably so. Presidential campaigns are America's greatest political theater, and quadrennial election day exit polls are voraciously consumed by experts and laypersons alike. However, because of the omission of analyses of how governors, prime ministers, or even members of Congress employ polls, readers are left with a noticeable theoretical void: Are presidents unique in their gauging of public opinion, or are they the norm? Do other chief executives have the resources and polling apparatuses available to them? How does parliamentary democracy shape leaders' pursuit or abandonment of Burkean democracy? One hopes that future conferences pursue these questions, as their answers will shed light on the role of polls and representative democratic politics.

Arguably the book's strongest chapter, "Presidential Leadership and the Threat to Popular Sovereignty," is a case study of the Nixon administration's polling operation. Lawrence Jacobs and Melinda Jackson methodically show that Nixon and his advisers were more than casual poll consumers. Rather, the president and multiple key advisers immersed themselves in wording specific poll questions, evaluating academic journals, and wooing or discrediting certain pollsters. This systematic effort at gauging and influencing public opinion provides the groundwork for presidential power to include the "ability to launch laserguided public relations campaigns" (p. 48).

Chapter 5 provides a solid historical foundation that underlies the legitimacy of polls. Michael Traugott notes that the public pays attention to media poll data, even if they are ignorant of polling methods. The concomitant interest in, and rise of, polling may fuel polling of dubious quality. Do polls give the public a voice in democracy? Traugott responds, "The potential for polling to fulfill this role is clearly there. . . . [P]olls provide the public with an independent voice that can act as an antidote to elites' interests and frames of issues and policy" (p. 91).

Chapter 6, coauthored by Matthew Streb and Susan Pinkus, concerns the "devious, fraudulent and popular" (p. 95) push polls that are proliferating the political landscape. Largely descriptive in nature, this chapter outlines various examples of push polls, names who has promoted them, and concludes that unscientific and unethical gauges of public opinion ultimately "undermine the public's faith in legitimate polling" (p. 112). It is here where one yearns to read the conversation among Traugott, Streb, and Pinkus. How threatened is polling as a legitimate enterprise? Do people not trust polls or pollsters because of the rise of garbage polls? What would these authors have thought of the 2004 election polls? Might the September and October discrepancies, albeit small, among media poll answers about presidential preference suggest that citizens perceive polls as legitimate gauges of the public's mood, or do people discard polls as illegitimate because different polls yield mildly different answers?

James Fishkin makes a compelling case for deliberative polls in Chapter 8. Citing Madison among others, Fishkin argues that genuine deliberation enervates the tyranny of the majority, and that citizens who participate in deliberative polling show "a willingness to make at least modest sacrifices of self-interest for the public good" (p. 154).

One small disappointment in the book is the citation format. It is regrettable to read any text ideally suited for undergraduates that contains such sentences as "Critics assert . . ." and "Others claim . . ." (e.g., pp. 2–3) without appropriate footnotes to let the readers know precisely who is claiming and asserting. Similarly, sometimes the casual nature of a conference boosts the desire to choose sexy examples over scholarly ones. While Clinton adviser Dick Morris is surely a person who can articulate why leaders should poll, his arguments cited in this book were made earlier and with greater precision by Archibald Crossley, George Gallup, and Elmer Cornwell. Christopher Hitchens and Benjamin Ginsberg are not given enough space, nor is Herbert Blumer; their cogent arguments deserve development if only to expose students to the history that underlies and surrounds the debate about how politicians assess, and should assess, the public's mood.

Why did not the authors include others' ruminations on the topic? Only so much can be included in a compendium, and this book is no exception. That written, scholars of the presidency and public opinion are still searching for theories to guide us. Samuel Kernell's "going public" argument notwithstanding, we need to question our methods and models if we are to explain how polls are used and what implications they have for democratic governance. While it is easy to criticize a volume of collected essays for what it excludes, this book is a fine complement to an undergraduate course in American politics. Individual chapters make for excellent homework assignments and are grist for the essay mill. The students will find readable, provocative questions. *Polls and Politics* will guide interested students in locating many of the major questions surrounding polls and democracy, and it may assist those same students in locating parsimonious answers for future scholars to debate and ponder.

Empowering the White House: Governance under Nixon, Ford, and Carter. By Karen M. Hult and Charles E. Walcott. Lawrence: University Press of Kansas, 2003. 264p. $40.00 cloth, $19.95 paper.

— Phillip G. Henderson, *The Catholic University of America*

This is the sequel to the authors' previous study, *Governing the White House,* which examined White House organization and process in the Hoover through Johnson administrations. Drawing on scores of White House documents from the presidential libraries, Karen Hult and Charles Walcott assess the development of the chief of staff and staff secretariat positions, as well as several other institutional components of the White House, such as the Office of Public Liaison, the White House Counsel's office, the Office of Communications, the Office of Congressional Relations, and the Office of Speechwriting and Research.

The authors argue that in terms of organizational innovation and the institutionalization of structures, the presidency became more distinctively "modern" during the Nixon, Ford, and Carter years (p. 165). In their previous study of the Hoover through Johnson period, which they refer to as the "early modern presidency," the authors indicated the prevalence of a catch-as-catch-can approach to White House structure, with the notable exception of the Eisenhower administration, where sound process and organization received top priority.

As noted in their first volume, many of the key institutional innovations of the Eisenhower era, such as the chief of staff position, the cabinet secretariat, and the Planning Board and Operations Coordinating Board of the National Security Council, were abolished in the Kennedy and Johnson administrations in favor of ad hoc task forces. This informal approach to policy led to lax coordination, duplication of effort, and a clear decline in institutional memory.

In *Empowering the White House,* Hult and Walcott indicate that the three presidencies of the 1970s had greater continuity in institutional development. With the exception of such entities as the National Economic Council and the Office of Homeland Security, the authors suggest that the White Houses of the Nixon, Ford, and Carter years "generally resemble the White House of today" (p. viii).

The Nixon White House became the cornerstone of the modern institutional presidency. Not only did Nixon reconstitute key organizational innovations from the Eisenhower years, including the White House chief of staff and cabinet secretariat, but he also elevated the Johnson era speechwriting and domestic policy units to full-fledged institutionalized status.

While the authors argue that a general approach, which they call "the standard model," is most effective in "allowing presidents to pursue their policy and political objectives amid the myriad constraints they confront," they stop short of saying that there is "one best way" to organize the White House. They do contend, however, that "the structure and operations of contemporary White Houses *can* make a difference in presidential politics, policy, and performance" (p. 2).

The decisions of Gerald Ford and Jimmy Carter to abandon the chief of staff position had critical consequences—mostly negative. Ford's "spokes-of-the-wheel" system, for example, allowed nine individuals direct, unmediated access to the president. Inadequate staffing led to "errors of fact and conflicts with previous presidential statements" (p. 33). It also resulted in what Press

Secretary Ron Nessen described as a "pretty loose and formless" structure in senior staff meetings in which "some of the people attending don't seem to understand what the purpose is" (p. 33). Although Donald Rumsfeld was appointed "staff coordinator" on September 15, 1974, five weeks after Ford became president, the staff management system, in the words of one key participant, "did not work nearly as well as the Haldeman White House did" (p. 35).

President Carter also adopted a spokes-of-the-wheel approach, saying: "I never have wanted to have a major chief of staff between me and the people who worked for me" (p. 38). Carter's staff members were young and inexperienced, and a premium was placed on loyalty to the president. This informal system engendered chaos and confusion. According to the authors, "[w]hat Carter did not appreciate was the vital role played by the White House chief of staff in organizing the president's time, helping him set priorities, managing the paper flow, and limiting access to those officials who needed decisions only the president could make" (p. 38).

Although deinstitutionalizing a structural innovation like the chief of staff position had adverse consequences, Hult and Walcott suggest that structural innovation is not without costs. For example, Nixon's White House Counsel, John Dean, demonstrated an inventive flair for meddling in the affairs of other staff units by asserting broad authority over matters that had not previously been under the purview of the White House Counsel's office. Dean's efforts highlighted "the recurrent competition over organizational turf in the Nixon White House" (p. 108). Similarly, Carter's office of White House Counsel became increasingly engaged in the policy process. At one point, the staff was almost totally absorbed by matters dealing with Israel and the Middle East (p. 113). Under Lloyd Cutler, the office "suddenly was transferred from a quiet, lawyerly operation that tended to legal business to a highly visible force in shaping and executing presidential policies" (p. 114).

Hult and Walcott indicate some fairly clear lines of continuity in White House organization across the three presidencies in their study, despite efforts by Ford and Carter to initially abandon elements of Nixon's staffing arrangements. Although they point to variation in the nature of structuring the White House, the propensity to retain or reestablish key components of White House structure appears to be increasingly irresistible. No president today would seriously consider trying to get by without a chief of staff. Even Bill Clinton's initial appointment of childhood friend Mack McLarty, in what could be described as a "weak" chief of staff system, eventually gave way to a more hierarchical and disciplined "strong" chief of staff system under the direction of Leon Panetta.

Although the Hult and Walcott study adds significant insight to the body of work on the institutional presidency, it is at times a bit tedious in setting forth the history of White House units. For example, the discussion of interest group outreach in Chapter 4 could easily be condensed into a few pages in the authors' previous chapter on public outreach. On the other hand, while inundating the reader with detail on White House units that deal with domestic policy, the authors barely scratch the surface of arguably the most important institutional component of the modern presidency—the National Security Council (NSC) and its staff. Granted, entire books have been devoted to this topic. Consequently, the authors may have considered elaboration on the NSC to be redundant with other works. Nonetheless, it is a significant oversight to omit at least a chapter-length discussion of the evolution of the NSC in a book that focuses on institutional development.

Despite these shortcomings, *Empowering the White House* is a worthwhile addition to the literature on the institutional presidency. Refreshingly, the authors do not seem to embrace the long-standing practice of the political science community to elevate the personal styles and idiosyncratic proclivities of presidents over proven procedures and process when it comes to White House organization. At least on the domestic side of policy, the work of Hult and Walcott goes a long way toward establishing a blueprint for the type of White House organization and procedures that can serve presidents well in their decision making and administration. There may not be one best way, but there are clear indicators of patterns and processes that are worthy of emulation. The learning curve does not need to be nearly as long as some presidents have made it.

Democracy Defended. By Gerry Mackie. New York: Cambridge University Press, 2004. 483p. $85.00 cloth, $29.99 paper.

— Melvin J. Hinich, *University of Texas at Austin*

The main thesis in this book is that social choice theorists, and especially Bill Riker and his Rochester "school," have cast a "long dark shadow over democratic politics" (the title of Chapter 1). Gerry Mackie then proceeds to dispel this dark shadow by an excessively argumentative interpretation of an impressive number of intellectual accomplishments in the study of social choice, public choice, and what is called in political science "formal theory." His main attack is directed against the work of Riker, especially that author's work on disequilibrium in majority rule.

Democracy Defended contains an impressive survey of social choice theory. It is worth reading by someone familiar with the basics of this theory, but I find much of the book tedious. But one basic matter of preference orderings that Mackie presents us with is his personal view about the political and economic world. His world seems to be shaped by his upbringing in a small Oregon town and his experience as "founder and elected leader of a large forestry worker's cooperative movement" and his related political activities (p. 3). In Mackie's world, "if the question is about the public interest, then individual preferences are naturally sufficiently similar to one another's to avoid cycling most of the time. And if the question is fixed sum redistribution, then destructive self-seeking preference *should* be excluded from public consideration (as taught in kindergarten)" (p. 94). The first sentence of this quotation is a statement about political behavior that is not supported with any evidence, even if we can agree about a definition of "most." The second sentence is obviously normative. It does not apply to the election and voting behavior of the Executive Committee of the Department of Government at the University of Texas at Austin where the Exec Com determines salary increases and other allocations by voting.

As Mackie points out, social choice theory is about aggregation of preferences. The work stemming from Arrow's Theorem deals with rank order preference orderings. Most of this work is about aggregation and not voting, but the applications to voting involve very simple voting schemes that are used in committee choice. The preferences of members of a voting committee are often influenced by arguments made as the committee deliberates, but sometimes the preferences are determined by pure self-interest. The study of how individual preferences are determined is a complex and difficult problem. I am not convinced about the value of studies of group choice by college students facing group decisions about small amounts of money.

The main theoretical paradigm for the study of mass politics is the spatial theory of electoral competition. The best-known result in spatial theory is the Median Voter Theorem. This theorem fails to hold if the votes are a simple probabilistic function of distances to the choices, if there are more than two choices under consideration, and if there is abstention due to alienation or indifference.

The author falls into the trap about cycles set by the early social choice theorists, who applied their results to simple committee voting. Although I have encountered what appeared to be cycles in academic committee meetings, the games played within and among legislative committees is complex. There is usually one committee that controls the bills that are voted on. In some cases, a single person, the speaker of the house, determined the agenda. The agendas are not the simple paired comparisons that I use in class to show how strategic voting is the sensible game play. The typical legislative agendas are made by the introduction of alternative bills and amendments to bills. Since information is valuable in a game, the players will attempt to disguise or hide their preferences. If there is a well-developed political space for alternative bills and legislators' preferences for bills, then the most preferred position of legislators will probably be known. Once again this is an empirical issue. The trade-offs implied by the shape of individual preference functions will not be known. Since all legislators know that they have to pass a budget and deal with a number of issues in a finite time using the structure that they agree to use as part of the game, the choices they face are limited, and the political game is played out behind closed doors and rarely in an open vote.

Mackie dismisses vote trading and other forms of logrolling as unstable due to the lack of knowledge of the preferences of others. The knowledge set that the vote organizers possess is very important to the stability of a coalition that has to predict future behavior. The assumption of common knowledge (all players know the probabilities of all outcomes in the game) that Austen Smith and other such game theorists use is crucial for the result quoted at the bottom of page 162. Common knowledge is unlikely since probabilities are estimated and people have different priors even if they have the same information. The assumption of common knowledge in game theory has not been tested by experiment or empirical analysis of game behavior. I believe that the reality of most political games is that the play is strategic but with lots of errors and confusions.

Turning to mass politics, elections are infrequent. We have presidential elections in this country every four years and congressional elections every two years. Since politicians have to raise money for their campaigns, they have to convince contributors that they will support their vital interests. Many of the contributors to campaigns are business and labor political interest groups. Business abhors political instability since such instability jeopardizes investments. Contributors are concerned about what an election outcome will imply for their interests. Thus, the most likely outcome of an election is a change in the leadership in Congress and in the Executive that is unlikely to result in large changes in public policies. This is also true for most proportional representative democracies, but there are exceptions.

The AK Party won a controlling majority in the Turkish parliament as a result of the 2002 election and several special elections held afterward. The present government is attempting to weaken the power of the army and to move the country toward an Islamic direction on the main axis of what Professor Ali Çarkoğlu and I have found to be a two-dimensional political space, using respondents' political attitudes from two face-to-face national surveys of Turkey in 2001 and 2002. We have shown that the voters have not shifted on the religious dimension but that the AK Party won because it was able to convince many of the moderates that they were the most credible party in the election to bring Turkey out of its depression. Turkey has a 10% threshold for election into their parliament. If the AK Party moves to change the Turkish constitution to make the country an Islamic country, there may be a civil war. Almost all Turks are Sunni Moslems and most are moderates, but there are serious differences of opinions about the role of religion and nationalism in political and social life in Turkey. Turkey's democracy is more typical then the idyllic model of Mackie's Oregon town.

Although Mackie presents some of the two-dimensional spatial voting results, he relies heavily on the unidimensional empirical results of Keith T. Poole and Howard Rosenthal based on legislative roll-call data. The information content in roll calls is limited, and the data and methods cannot statistically test the dimensionality of the political space. My collaborative work on attitudinal data from U.S., German, and Chilean surveys indicates that the political spaces of those countries are two-dimensional with a third valence dimension for candidate or party competence.

The final third of this book is a rebuttal to Riker's historical analysis where he claimed to have found voting cycles. Since I did not buy Riker's arguments when he published them, I basically agree with Mackie's analysis. I felt that Riker did not provide enough evidence about the preference orderings of the players in those voting games.

I have some special criticisms of this book. The material on pages 25 and 26 are inappropriate. There is no reason to mention that poorly researched polemical article in the *New Republic*. Who cares what goes on in Harvard's Government Department? Also, who cares about what economics students in Paris want or do not want? These pages pander to the haters of mathematics and the use of logic in political analysis. The term "Starship Rochester" is also juvenile. As a matter of fact, Bill Riker was doing some experiments in political game theory when Otto Davis and I as graduate students introduced the first multidimensional spatial theory of elections in 1965. The history of the development of formal theory and public choice is more interesting than the superficial treatment presented in this book.

Neopluralism: The Evolution of Political Process Theory. By Andrew S. McFarland. Lawrence: University Press of Kansas, 2004. 208p. $35.00 cloth, $16.95 paper.

— Matthew J. Burbank, *University of Utah*

Pluralism and its advocates once dominated the study of American politics. And by staking out a strong positivist position on the fundamental issue of political power in their battle with elite theorists, pluralists also influenced the discipline of political science as a whole. Yet, in contemporary American politics, pluralism is rarely invoked as a theoretical guide, and when it is discussed, it is often as a foil for some alternative conceptual approach. So what happened to pluralism? In *Neopluralism,* Andrew McFarland seeks to explain what happened and to restore pluralism to a place of prominence in American politics and within the discipline of political science.

The pluralist approach, as McFarland explains, began with the group theory of Arthur Bentley and David Truman. Although not all of their ideas became a part of pluralism, these early group theorists contributed the notion that to understand politics, we should study how public policy is made and that policy is made through an ongoing series of interactions among numerous contending agents. The second stage in the creation of the pluralist approach was the development of the idea that political power could be understood in causal terms and that the exercise of power could vary across policy areas. This causal view of political power was articulated most prominently in the work of Robert Dahl. To this point, McFarland's explanation of the development of pluralism is fairly conventional. His thesis, however, is that pluralism did not stop at this second stage but in fact continued to develop, even though this theoretical progress has not been widely recognized.

McFarland points out that the pluralism of the 1960s was confronted with a number of challenges. Some were grounded in differing interpretations of empirical evidence, such as those of scholars who observed the policy process and found evidence of "iron triangles" or "agency capture," rather than power being widely dispersed among competing actors. Other challenges, however, were more fundamental, such as Mancur Olson's (1965) *The Logic of Collective Action* and the "the two faces of power" critique (Peter Bachrach and Morton Baratz, "Two Faces of Power," *American Political Science Review* 56 [December 1962]: 947–52). Although many may have regarded these criticisms as effectively undermining pluralism, McFarland argues that continued research into the policy process demonstrates that while the objections of critics must be taken seriously, they are not insurmountable obstacles. Indeed, his neopluralism is essentially an effort to bring together the central features of 1960s pluralism with decades of empirical research on interest groups and policymaking and to do so in such a way as to recognize the merit in many of these criticisms.

McFarland's neopluralism is thus intended to be "a general theory of power, policymaking, and interest groups" (p. 62) that incorporates an array of related approaches, such as advocacy coalitions, issue networks, and policy niches. More ambitiously, the author seeks to infuse neopluralism with an historical dimension and to integrate neopluralism and the study of social movements. In doing so, his work both narrows and broadens the conception of classic pluralism. He seeks to narrow the scope of pluralism by moving it from a general explanation of how power is distributed in the United States to a theory of the policymaking process. Simultaneously, he broadens pluralism by attempting to incorporate a variety of developments in the discipline into what is now termed "neopluralism."

In sum, Andrew McFarland undertakes a sweeping reinterpretation of a broad range of social science research on political power, policymaking, interest groups, social movements, and the state. This book is both an intellectual history and an effort at theory construction. In the end, it is more successful as an intellectual history because he presents a compelling argument that we should reconsider the place of pluralism in the study of politics. It is less successful as an articulation of a coherent theoretical framework. While his "neopluralist synthesis" is impressive in scope, neopluralism never really comes across as a logically coherent whole. As it is presented here, neopluralism remains more of a collection of related concepts—some clearly complementary but others potentially antithetical—organized under a rather broad umbrella.

Whatever limitations it may have, this book deserves a wide audience among students of American politics, interest groups, and public policy. McFarland has written an impressive work that speaks directly to important themes in American politics and the discipline of political science. As such, *Neopluralism* should be read and debated within the discipline for years to come.

Public Pensions: Gender and Civic Service in the States, 1850–1937. By Susan Sterett. Ithaca, NY: Cornell University Press, 2003. 240p. $39.95.

— Michele Landis Dauber, *Stanford University*

Susan Sterett's book is a useful, although flawed, history of litigation in the states over municipal and state pensions for civil service employees. In particular, Sterett examines the nineteenth- and early-twentieth-century history of state-level efforts to expand the scope of permissible state and local pensions, from firefighters and police officers to other civil service employees and finally to such groups as mothers and the elderly. Generally, state courts interpreted their constitutions to permit pensions for employees but not for the broader public, despite scattered efforts to argue that all labor should count as "service" to the public, meriting reward by state governments.

Sterett shows that before the Civil War, states and municipalities began to provide pensions to firefighters and police officers. These payments were challenged by taxpayers and others on the grounds that they violated the public purpose doctrine, which held that the states could only expend public funds in the interest of the public as a whole, as opposed to a class or private interest. In response, state courts generally found that because such workers labored in the service of the whole community, payments to them on injury, death, or retirement furthered the public interest. By the 1920s, this rationale was extended to civil service workers more generally, though advocates of this extension had to overcome a broad sentiment that other employees, such as clerks and teachers, were not entitled to public pensions because their work lacked the element of danger inherent in fighting fires or crime.

In addition to pension payments, states and localities provided relief to the indigent poor; however, state courts held that programs without a means test lacked a valid public purpose. So, for example, Sterett writes (p. 117) of the Arizona Supreme Court's 1916 decision striking down that state's mother's pension law because taxpayers "ought not to be made or required to help pay pensions to those who have enough and to spare of the world's goods." State court opinions like this one applying the public purpose doctrine effectively barred states and municipalities from providing universal old-age or mother's pensions, since no state court was willing to see lifetime laboring or mothering as a service to the community as a whole. Sterett does a particularly good job of uncovering the gender dimensions of these judgments about service and dependency.

In sum, the author shows how the efforts of reformers to launch a more thoroughgoing welfare state by utilizing state legislatures as Justice Brandeis's "laboratories of experimentation" were a total failure. If her story ended there, this book would make a significant contribution to the history of U.S. social provision and to our understanding of the reasons that the New Dealers decided to pursue federal, rather than state, old-age pensions during the 1930s. However, she has a more ambitious, and ultimately unsuccessful, agenda.

Sterett argues that the failed state-level efforts to extend the "service" argument to provide pensions to such groups as the elderly and mothers prefigured the successful adoption of this very argument at the federal level during the New Deal. For example, she states that "once Roosevelt signed the Social Security Act in 1935, and corporate lawyers challenged the programs toward which employers were required to pay taxes—unemployment insurance and old age social security—it was state constitutional debates concerning public payments that provided the groundwork for explaining why the programs were indeed constitutional" (p. 4). Similarly, Sterett claims that "[t]he elderly also never won a claim to service constitutionally before the New Deal. That awaited the determined work of political executives who believed that by explaining to Americans that the elderly were receiving payment for work they had done, they would be able to eliminate the stigma of that form of public assistance" (p. 180).

It is worth pausing to think about the kind of evidence that might support this argument. At minimum, we would expect to see the lawyers who defended the Social Security Act against court challenges follow the earlier lead of the lawyers who had defended state-level universal old-age pensions in the three states where they were enacted. The New Dealers would have marshaled the precedents provided by state cases expanding the definition of "service" to include civil service workers and argued that just as civil servants provided a service to the state, so did all workers. They would have argued in their briefs or in their oral argument to the Court that if the state had a valid public purpose in providing pensions for teachers and clerks, then the federal government had a public purpose in making such payments to all waged workers. And we might well expect to see some trace of this argument in the Supreme Court opinions approving the act. But this "service" argument was absent from the legal fight over the Roosevelt administration's programs, despite Sterett's repeated allusions to its presence.

The most likely reason for this absence is the fact that the public purpose doctrine never applied against the national government, which instead was governed by the more lenient standard that legislation be in the general welfare, a determination that the courts had always left to Congress. The greater latitude enjoyed by Congress, as opposed to the states, in questions of spending was well known to judges, commentators, and lawyers at the time, including Columbia law professor Frank Goodnow, on whose *Social Reform and the Constitution* Sterett relies. Goodnow wrote extensively in that volume (1911, pp. 310–17) about the lack of any public purpose limitation on congressional spending, noting that there were numerous precedents for charitable spending by the federal government that could be marshaled in support of old-age, sickness, and accident pensions. He concluded his discussion of pensions by saying that "there is much ground for the belief that such pensions, particularly if confined to indigent persons, might constitutionally be provided by the federal government" under the auspices of the more expansive General Welfare Clause, rather than by the states.

Thus, it is unsurprising that the New Deal lawyers charged with defending the Social Security Act ignored the argument that universal old-age pensions served a public purpose because all work was service to the state: The public purpose doctrine restricting state and municipal spending was entirely irrelevant to acts of Congress. Instead, the New Dealers exhaustively sought out relevant precedents for federal spending under the General Welfare Clause, which they found in such efforts as disaster relief, the Children's Bureau, and the Morrill Act. For New Dealers looking to expand the redistributive capacity of the federal government, arguments attempting to analogize the powers of the states and the federal government must have been a particularly unappealing option. Indeed, such arguments were frequently mobilized by those opposing the Social Security Act, who hoped that the Court would shrink the general welfare power down to the size of the public purpose doctrine.

Regrettably, Sterett is unaware of this crucial distinction between the national and state governments where the law of public spending was concerned, as when she asserts (p. 180) that in upholding the old-age benefit provisions of the Social Security Act in the 1937 case of *Helvering v. Davis,* the Court had held that Congress "could determine that insurance programs divorced from public service and indigence could serve a public purpose." But this is a misreading of *Helvering,* which mentions neither the notion of "service" nor the public purpose doctrine. Justice Cardozo's majority opinion concluded that the old-age benefit scheme was valid because Congress was entitled to great deference in its determination of the general welfare, and it had determined the relief of unemployment was such an object. Moreover, the Court offered its agreement with Congress, if any was needed, that "unemployment is an ill not particular but general, which may be checked, if Congress so determines, by the resources of the nation" (p. 641).

In the end, Sterett's argument that disputes over state pensions were somehow a precursor to the New Deal is an example of what the humorist Steve Martin has called "semantic causality"—that is, the imputation of causal connection based purely on a similarity of words, rather than on the identification of any actual casual mechanism. It is certainly true that the legal basis for old-age pensions was discussed and disputed in the states and at the federal level, and that the state discussions preceded those at the federal level. This does not mean, despite Sterett's efforts to suggest the contrary, that there was any relation between the two. It is unfortunate that *Public Pensions* is marred by this overreaching, as its historical contribution to our understanding of the legal and political dimensions of state-level public pensions is a valuable one.

Regulation in the States. By Paul Teske. Washington, DC: Brookings Institution Press, 2004. 272p. $52.95 cloth, $22.95 paper.

— Michael Mintrom, *University of Auckland*

Government regulation of economic activity is endemic to modern society; a vast array of everyday transactions are affected—and shaped—by rules and requirements imposed by governments. The intentions behind such regulations are typically assumed to be benign. Through regulation, governments can rectify problems caused by deceit, unaccounted costs, or unchecked power that would otherwise limit the scope and the effectiveness of markets. From a fiscal perspective, regulation is a relatively low-cost form of government activity. The budgets of regulatory agencies represent small items of government expenditure, compared with the budgets of agencies engaging in service provision or income redistribution. Because government regulation of economic activity is very often taken for granted and is of low salience to citizens, it is rarely the stuff of headline news. Among scholars, regulation has also tended to receive short shrift. Yet, as an area of public policy research, the study of government regulation deserves close scrutiny. At its best, research in this area can provide insights into the intersections of markets and governments, using the tools of positive political economy to good effect.

Since the publication in 1990 of *After Divestiture: The Political Economy of State Telecommunications,* Paul Teske has produced a stream of articles and several books investigating state government regulation within specific areas of economic activity. *Regulation in the States* represents a capstone to this research agenda. Substantively, the book provides an excellent overview of regulation as a policy instrument. The focus on regulation promulgated by state governments rather than the federal government is refreshing. Teske offers a lot of new information both on the politics of state-level public policymaking and on factors generating variation on state applications of regulatory measures. Methodologically, the book also makes a significant contribution. Throughout, the author uses quantitative data and regression analysis to test hypotheses about when, why, and how, and with what consequences, states have regulated particular areas of economic activity. Given the range of state regulatory activities covered, the quest for consistency in the use of data and data analytics methods is commendable, and inconsistencies are well explained.

The book is divided into six parts. Part I contains three chapters. In turn, these review the basics of regulatory policy as practiced at the state level, competing theoretical perspectives on regulation, and issues to do with measurement and methodology. Part II focuses on the regulation of monopolies, with separate chapters on telecommunications and electricity. Part III covers cases of information asymmetry and how regulation can address them. Chapters are devoted to cases in the insurance, finance, and health care industries. Part IV covers occupational regulation, with chapters focusing on the regulation of attorneys and of medical doctors. Part V covers two cases in environmental regulation: clean air plans and groundwater protection. Part VI contains four chapters that, together, offer conclusions on the broader topic of regulation in the states. Here, Teske synthesizes the results from the various quantitative studies, discusses reform efforts, highlights the importance of state courts in the regulatory process, and considers likely future directions for state regulation. This structure gives the book a strong sense of cohesion. In so doing, it meets a difficult challenge, since the 16 chapters cover a diverse set of issues.

Readers with specific substantive interests will likely gravitate to particular chapters and ignore others. I think the book is strongest in its treatment of monopoly regulation and state responses to information asymmetries. Here, the analyses clearly indicate the key role that political considerations play in explaining differences in state regulatory regimes. As such, these chapters underscore the unique contribution that political scientists can make to the explanation of regulations. The focus on the importance of political institutions and the ideological predispositions of legislators for regulatory design takes us well beyond the traditional focus on pricing, dead-weight losses, and incentives associated with purely economic studies of regulation.

Since the range of activities subject to government regulation is ever expanding, individual readers might wish that additional topics had been given chapter-length treatment in the book. Teske acknowledges this point, noting that regulation is often divided into two broad categories—economic and social—and that this book focuses on topics within only one of these. Clearly, scope remains for a range of inquires to be conducted into the ways that states engage in and manage social regulation. On that score, questions arise as to how effective traditional tools of policy analysis might prove in helping us interpret justifications for alternative policy settings. Can they help us investigate differences in the design and effects of state marriage laws or laws affecting aspects of human reproduction? How might they help us to explore variation in the ways that states address public disputes arising out of religious and cultural differences? Elements of economic globalization raise even more fundamental questions. Among other things, these concern jurisdictional sovereignty and the adequacy and appropriateness of both longstanding and newer state regulatory measures. Moves from the traditional core of regulatory practice call for moves away from traditional regulatory analysis. But future analyses will likely gain most analytical traction when they consciously build on traditional approaches. For this reason, Teske's volume represents essential reading for those seeking to produce rigorous analyses of emergent, paradigm-shifting developments in both economic and social regulation at all jurisdictional levels.

Aside from the substantive matters covered here, another feature of this book deserves comment. Of its 16 chapters, 10 involve coauthorships between Teske and former graduate students. In the majority of cases, earlier versions of these chapters appeared in high-quality peer-reviewed journals, where the coauthors received equal credit for their contributions and were often listed as lead authors. Thus, while Teske conceived of this book and the research agenda it is based on, as he worked on it he facilitated the training of an impressive number of graduate students and introduced them to the world of academic publishing. As he advanced the careers of his students, Teske undoubtedly benefited from the sustained practice of enlightened self-interest. This broader project, therefore, presents an attractive model of how a talented scholar might effectively combine the pursuit of a significant research agenda with first-rate teaching and mentoring. Overall, *Regulation in the States* makes a sound contribution to an aspect of American politics and policy that for too long went understudied. It should prove a valuable addition to reading lists in graduate-level public policy and state politics courses.

Bureaucrats, Politics, and the Environment. By Richard W. Waterman, Amelia A. Rouse, and Robert L. Wright. Pittsburgh: University of Pittsburgh Press, 2004. 184p. $24.95.

— Daniel McCool, *University of Utah*

This is not a book about the environment. Rather, it is an effort to improve existing models of bureaucratic behavior, especially in regard to how bureaucrats relate to the larger political environment. It relies on data from questionnaires from the U.S. Environmental Protection Agency and the New Mexico Environment Department.

Richard Waterman, Amelia Rouse, and Robert Wright are especially concerned with presumptions made by principle-agent models, but they also include a discussion of the theory of budget-maximizing bureaucrats (p. 10). In regard to the principal-agent model, they argue that it is improved by relaxing "the assumptions of an information asymmetry and goal conflict. We seek to treat those concepts as variables rather than as constraints" (p. 22). Unfortunately, the authors never clarify if these are dependent, independent, or interceding variables. This severely limits the explanatory power of the theoretical changes they propose. For example, the authors "hypothesize that many different relationships can exist between principals and agents" (p. 31). However, they do not present a hypothesis that specifies an independent and dependent set of variables. The closest they get is on page 65: ". . . [O]ne can hypothesize that the greater the level of expertise involved in an agency's functions, the harder it will be for principals to monitor a bureaucracy's behavior, and the more likely it will be that an information asymmetry exists." They operationalize expertise by counting the degrees held by bureaucrats. This hypothesis is tautological; both variables are measures of the possession of specialized knowledge.

The authors make another attempt to present a hypothesis on page 68: "[A] high level of discretion also is likely in situations where agents take advantage of an information asymmetry." Again, there is no effort to indicate which variables are independent and dependent. Later, they discuss the "causes of this discretion" (p. 74), but causation cannot be identified without an understanding of which variables are independent. The authors find evidence that the bureaucrats who responded to their questionnaire "see themselves as having discretion" (p 77). However, their data do not reveal if this level of perceived discretion is typical of most agencies, or even if it is higher in agencies with information asymmetry. Without a comparative context, the authors cannot explain the role of discretion and how it might affect the theoretical assumptions they are discussing. They argue that "we need to model a variety of political principals at both the state and federal levels when we examine the question of who controls the bureaucracy" (p. 92). This is certainly true, but without an identification of causative factors, we cannot reach conclusions about who controls whom.

After a lengthy discussion of the data, Waterman, Rouse, and Wright offer two sets of conclusions regarding the principal-agent model and the budget-maximizer model. In regard to the former, "we need to consider not only different types of political principals, but also the different ways bureaucrats are likely to respond to them" (p. 111). Or in other words, the models variously labeled as issue or policy networks, subsystems, or advocacy coalitions are more useful than dyadic principal-agent models (see p. 128). In regard to budget-maximizing, the authors conclude that their "findings undermine the basic budget-maximizer assumption that personal and bureau utility is perceived as the same thing" (p. 125). This conclusion is based on data that indicates that many of their respondents did not know if their agency's budgets had increased or decreased in recent years. It is somewhat of a leap of faith to interpret such data to mean that bureaucrats are not interested in how their agency's funding affects them personally.

Despite the authors' failure to adequately specify independent and dependent variables, their overall conclusion is an important one: "[W]e recommend the development of theories that provide a more dynamic interaction between multiple principals, multiple agents, and agents motivated by a variety of goals. We are especially critical of theories that include normative bureaucrat-bashing assumptions" (p. 130). This book does not make a significant contribution to the development of such a theory, but it does point out avenues of future research.

The last section presents what the authors call "a list of twenty commonly advanced characteristics of the bureaucratic process, which we present as 'myths'" (p. 130), and they posit that these myths have been "commonly expressed in the bureaucratic literature" (p. 133). Unfortunately, none of them is accompanied by citations, and so we do not know who is perpetrating them. But it is presumptuous to make such claims based on evidence from just two agencies. On the basis of the evidence presented in this book, we have no idea if these agencies are widely representative of bureaucracies in general, or they are two outlying aberrations. Thus, it is not possible to generalize from their findings and use them to declare large segments of the bureaucratic literature as mere myths.

The authors ask some very important questions that have puzzled many scholars, and they attempt to answer some of them. I admire them for setting such a difficult goal for themselves. However, *Bureaucrats, Politics, and the Environment* provides precious few answers to these questions, and ends by making assumptions that are far beyond what can be sustained by the evidence presented.

The Constraint of Race: The Legacies of White Skin Privilege in America. By Linda Faye Williams. University Park: The Pennsylvania State University Press, 2003. 440p. $35.00 cloth, $27.95 paper.

— Dean E. Robinson, *University of Massachusetts, at Amherst*

Linda Faye Williams's book seeks to identify "how, when and why American social policy became fused with the politics of race" and what that has meant for the development and evolution of the welfare state in the United States. The author's central argument is that the welfare state was "grafted onto preexisting conditions of race relations," and the long-term consequence of this grafting is that even today, the organization of social policy in the United States continues to reproduce advantages for whites and disadvantages for many people of color. Further, as social welfare policy institutionalizes white advantage, this in turn tends to block movement in the direction of more universalistic social policy (p. 14). Williams supports this contention by revisiting the political history of social welfare policy, from just after the Civil War up to the Clinton administration's efforts at welfare "reform" in 1996.

William's study is part of a small and relatively recent subset of scholarship on social welfare policy in the United States—including Jill Quadagno's *The Color of Welfare* (1994), Michael K. Brown's *Race, Money, and the American Welfare State* (1999), and Robert C. Lieberman's *Shifting the Color Line* (1999)—that looks at the ways racial subordination was perpetuated in the design and administration of various social welfare policies. Social policy has never addressed this legacy, with the result that whites generally enjoy the better of the two tracks of social welfare policy: one being the entitlements typically available by way of employment, the other being means-tested government provision.

Williams departs from these other works in two important ways. First, *The Constraint of Race* covers more ground historically, by beginning with an examination of Civil War pensions and the Freedman's Bureau, and offering subsequent chapters on the New Deal era, the civil rights/Great Society era, and the period of retrenchment signaled in part by Ronald Reagan's election in 1980. The author devotes two chapters to the presidency of Bill Clinton—giving considerable attention to his civil rights and social policy record—and includes a very useful chapter that documents continuing disparities between whites and people of color on a number of key economic indicators, such as income, wealth, and employment. This historical sweep supports one of her central points: In the history of the United States, the federal government has intervened twice to extend civil and social rights to African Americans and other minority groups—in the decade or so following the Civil War and in the decade or so following the Civil Rights movement. Both periods were relatively short-lived, and both efforts were limited by the *segmented* nature of U.S. social policy—the fact that entitlements to public goods are established and administered at the federal and state level—and by the fact that challenges to white privilege are typically followed by periods during which conservative politicians exploit white opposition to the federal government's interventions on behalf of racial minorities, who are stereotyped as dangerous, parasitic, and therefore undeserving of public support. Indeed, Williams correctly notes that in the 1990s, "the tone of public rhetoric throughout the debates over crime and welfare was eerily similar to that of the Reconstruction era" (p. 275).

Perhaps the most significant difference between this study and others of its genre has to do with its focus on white privilege, the "flip side" of nonwhite subordination. Throughout the historical analysis, Williams highlights the ways in which white identity carries with it an implicit "property value." By this she means that "whiteness" represents *and* facilitates a different opportunity structure in the areas of housing, education, employment, and criminal justice. Whiteness reflects accumulated advantages of income and wealth. Here, she is drawing from concepts from the broad field of critical race theory, which seeks to explain and examine the construction of white identity and the psychological and material benefits it has conferred over history.

The reassessment of Clinton's legacy is among the noteworthy contributions of this study, and the two chapters covering that administration clearly support the central argument for looking at whiteness or white privilege to make sense of developments in social policy. Williams looks at crime, welfare reform, and other initiatives to make the point that although Clinton enjoyed broad support from African Americans, the laws he signed in these areas reflected conservative, not liberal, orthodoxy. The crime bill of 1993 stiffened mandatory sentencing and disregarded racial bias in sentencing, most significantly as it concerns the death penalty. The welfare legislation of 1996, the Personal Responsibility and Work Opportunity Reconciliation Act, established time limits and work requirements. Clinton offered at best tepid support of affirmative action. These positions partly reflected his stance as a member of the Democratic Leadership Council, an organization that advocates political centrism and a rhetoric that seeks in part to distance the party from any obvious association with racial minorities, especially African Americans. Clinton's policy choices also reflected the political dominance of the Republican Party after 1994.

The Clinton years also provide Williams with an opportunity to analyze black intragroup differences—or interests, really—that were apparent in the response of black political elites to threats to affirmative action and Aid to Families with Dependent Children, and to the call for more punitive measures in criminal sentencing. Black elites vigorously defended the policy most important to middle-class interests—affirmative action—and basically capitulated on the others. This class dimension probably could have been probed more deeply.

Indeed, Williams probably pays too little attention to the social and political construction of class identity. Race and class identity are constituted mutually, and that goes a long way in explaining why appeals to whiteness, once explicit but today coded, enjoy such resonance among so many white Americans. This is a really a minor quibble, more a matter of emphasis than omission.

Williams ends her study by revisiting the debate about how best to advance civil and social rights in the contemporary era. She correctly rejects the position of Theda Skocpol, William Julius Wilson, and others who would emphasize universal policies over those that are targeted more specifically to minority groups on essentially two grounds. First, since universal policies like the Great Society are often perceived as "racial" in their impact, there is hardly any guarantee that many whites would not *perceive* universal programs as targeted, and therefore oppose them. Second, and perhaps more importantly, racial subordination *requires* special remedy because of continuing discrimination on the basis of race.

Williams's argument is convincing, and her grasp of the scholarship on the welfare state and critical race theory guarantees engaging and insightful reading. *The Constraint of Race* should be read by students and scholars interested in social welfare policy in the United States, and the ways that constructions of racial identity have shaped and will continue to shape debates about the nature of public provision in the United States.

On Capitol Hill: The Struggle to Reform Congress and Its Consequences, 1948–2000. By Julian E. Zelizer. New York: Cambridge University Press, 2004. 376p. $30.00.

— Sean Q Kelly, *Niagara University*

Julian Zelizer's book joins a number of others that have been taking up the issue of congressional reform. Recent books by political scientists E. Scott Adler (*Why Congressional Reforms Fail,* 2002), Nelson Polsby (*How Congress Evolves,* 2003), and Eric Schickler (*Disjointed Pluralism,* 2001) are among a growing list that seeks to explain the success or failure of congressional reforms during the twentieth century. One of the interesting aspects of these books is that they are authored by mostly younger scholars who are reassessing, and sometimes seriously challenging, a genre of scholarship that was pioneered during the seventies by such scholars as Lawrence Dodd, Bruce Oppenheimer, Barbara Sinclair, and James Thurber, to name a few, many of whom were American Political Science Association Congressional Fellows working in Congress during the period of substantial congressional reform in the 1970s.

Zelizer's work is quite different from these other works, which emanate from political science, for several reasons, not the least of which is that he takes an historian's approach to the topic. As an historian, he does not employ a mode of explanation that relies on individual behavioral assumptions (e.g., the reelection goal employed by Adler): "In order to understand the institutional changes that shaped Congress, it is essential to look beyond the motivations of legislators—the subject that has dominated the attention of political scientists" (p. 3). *On Capitol Hill* relies on rich descriptive detail to support the main thesis of the book, rather than on quantification, interviews, and other methods familiar to political scientists. Zelizer has collected archival documents from the papers of former members of Congress, interest groups, and other sources. While the use of archival material is standard operating procedure in history, it is far less common in political science. Political scientists, especially congressional scholars, may well be impressed by the richness that these documents can impart to research, and may want to consider how the use of archival sources might improve their own work. Archival documents provide a portal into the world of the politician and provide data (in this case qualitative) for better understanding politics from the perspective of the participant.

The central theoretical idea that Zelizer develops is that congressional reform is "a thoroughly *historical* process that is messy, slow, and involves multiple institutions" (p. 3). Congressional change did not occur in a linear and predictable manner, he argues; rather, the "narrative about congressional reform takes place in fits and starts. The changes were not inevitable or automatic; they resulted from a fierce and protracted struggle" (pp. 3–4). The body of this work seeks to advance the view that "change takes a long time and does not tend to occur in the dramatic bursts of innovation often depicted in high-school and college textbooks" (p. 264). While scholars in the American political development tradition will be familiar with this approach, it is an observation that those outside of its literature would do well to consider deeply.

More contemporary reforms have been well studied by other scholars; readers seeking new revelations about congressional reform will not find them here. What is at the heart of Zelizer's book is a different intellectual approach to the study of change that is augmented by new archival material; and this is its significant contribution. The difference in approach orients the outstanding description of battles within Congress, and between Congress and external actors (e.g., presidents and interest groups), to control and change the policymaking machinery in Congress. It also allows the author to highlight the unintended consequences of institutional change and connect prior historical changes to contemporary developments in Congress.

For example, early in the book, Zelizer focuses his attention on "Bomb-Throwing Liberals" in the 1950s who were interested in promoting the cause of Civil Rights and other progressive legislation. Early on, these members identified the congressional committee system, which was controlled by southern conservative Democrats, as an obstacle to progressive legislation and as a source of fragmentation in the policymaking system. Beyond the opportunity to advance progressive legislation, reformers argued, were potential gains in legislative efficiency through a more centralized legislative machinery, and opportunities to assert congressional power vis-à-vis the executive branch. They focused on the closed nature of congressional decision making as a major prop for the restrictive and conservative committee system. They built coalitions outside and inside Congress in support of their reform aspirations.

In the 1950s, these reformers existed on the edges of political power in Congress, but by defining what they saw as the major impediments to advancing their progressive agenda, "they slowly laid the foundation for an alternative to the committee process" (p. 35). While many of the reforms they championed did not show results until the early 1970s, they "launched the critical agenda-setting stage" (p. 35) of the battle for reform and thereby significantly influenced the reforms that resulted. In short, it is important to understand the impact that the reform advocates in the 1950s had on the reforms of the 1970s. The "problem definition" offered by early reformers shaped the reforms that were eventually put into place. Reform advocates sought to open Congress to public scrutiny as a means of pressuring the committee system to become more responsive to the demands of progressive policies; they also sought to purge the influence of money in politics.

To some extent, the reformers were successful, but the new political reality that reformers created for themselves and their successors resulted in unintended consequences. A reformed Congress provided opportunities for partisans to further undermine the committee process, develop new political targets, and develop attack strategies against other members. When the Republicans were in the minority, they used this new institutional environment to frustrate Democrats' legislative initiatives. With a substantial change in the American political mood toward conservative ideas, and with the Republicans in control of Congress, conservatives seized on reformed institutional arrangements to press their own agenda in Congress: "Republicans paralyzed the Clinton administration during its second term. When the era of divided government ended with the election of President George W. Bush, the GOP used the process to severely weaken the fiscal infrastructure of the American state" (p. 267). Reforms conceived in the 1950s (though an argument could be made that a similar reform impulse goes back even further) shaped the politics of the future, our present. Herein lies the broad contribution of this book and some potent lessons for political science.

On Capitol Hill has potential appeal to a diverse readership. It is accessible to a general readership interested in American history. The primary obstacle for many potential readers, however, will be the price tag. Cambridge has not, to date, offered this book as a soft cover; it should. The volume should be *required* reading in graduate courses on Congress in political science; both its methodology and its substance are necessary for future congressional scholars. It easily could be used in undergraduate courses, depending on the faculty member's approach to the course. Finally, all serious scholars of Congress should have this book on their book shelves. It will quickly become the standard source for those seeking a history of the modern Congress.

COMPARATIVE POLITICS

Transnational Politics of the Environment: The European Union and Environmental Policy in Central and Eastern Europe. By Liliana Andonova. Cambridge, MA: The MIT Press, 2004. 224p. $57.00 cloth, $23.00 paper.

— Jonathan Golub, *University of Reading*

After the collapse of communism at the end of the 1980s, the 10 new emerging democracies in Central and Eastern Europe (CEE) all sought to accede to the European Union. But to join the EU, applicant countries had first to adopt the vast array of existing EU laws, including a host of tough environmental laws. Some states coped more successfully with regulatory harmonization than did others, so what explains variation in their patterns of national adjustment?

In her book, Liliana Andonova breaks this question into two dependent variables—the preferences and strategies of the CEE industries subject to new regulation, and the extent of legislative reform and compliance with EU standards. She constructs and tests a theoretical explanation for both these aspects of national adaptation that focuses attention on the importance of international markets, transnational networks, and domestic institutions. The book employs detailed process tracing to compare adjustments in the Czech Republic, Poland, and Bulgaria to chemical safety regulations and air emissions standards for large combustion plants. Elegantly presented, the analysis marshals an impressive array of published and unpublished material and numerous interviews.

The main conclusion about actor preferences is that export-oriented companies and multinational corporations welcome harmonization when it offers them net economic advantages. Firms in the chemical sector embraced EU regulations because for them, the economic benefits, such as improved access to the EU market and the ability to attract more investment, would outweigh the compliance costs. The electricity industry, geared predominantly to domestic markets, had little or nothing to gain from meeting expensive EU air quality standards and thus opposed harmonization. In terms of actual reforms, all three states quickly adopted and complied with EU chemical safety regulations. With the EU air pollution regulations on large combustion plants, the Czech Republic again managed to comply quickly and

Poland did so more gradually, whereas Bulgaria only formally transposed them into national law, with no actual compliance on the ground.

In Andonova's account, the two dependent variables are closely, but not perfectly, linked. Thus, much of the book is devoted to exploring the extent to which, and under what conditions, big domestic economic players manage to control the environmental reform agenda. The chemical safety case studies document the privileged position of exporters in policy formulation and how in all three states, they exploited their contacts with government. In addition, the capacity-building activities of transnational and domestic business networks helped overcome obstacles to compliance with EU chemical regulations by providing information, training, and compensation mechanisms for smaller or reluctant firms. With pro-EU industry playing such a major part, an important related finding is that national historical, economic, and institutional differences had almost no impact on how the three states adjusted to EU chemical regulations.

By contrast, the air pollution cases illustrate that the structure of domestic institutions and the position of veto players does matter in the absence of pro-EU exporters, or, more generally, where losers are a significant enough factor that they need to be coerced or bribed, as was certainly the case with EU rules on large combustion plants. Andonova argues that when authority is concentrated in the hands of actors with broad constituencies (the Czech Republic), harmonization proceeds more smoothly than when veto players with narrow constituencies enjoy power (Poland and Bulgaria).

Besides the nature of veto players, Andonova identifies other important independent variables. The relative wealth and economic liberalization of Poland and the Czech Republic, at least compared with Bulgaria, allowed their governments to offer a range of financial compensatory mechanisms to regulated firms. And the substantial technical and financial support provided by the EU throughout the 1990s, particularly to the Czech Republic and Poland, also helped smooth the harmonization process. Surprisingly, the author finds that environmental groups played hardly any role in the success or failure of regulatory reform, apart from during the immediate postcommunist period.

Overall, Andonova makes a compelling case, but at points her argument is so streamlined that it misses opportunities to engage with more of the rich existing literature, especially in EU studies, that explores domestic–international linkages. For example, she does not mention it, but her findings certainly challenge parts of the "trading-up" thesis expounded by David Vogel and others. That thesis highlights the importance of Baptist–bootlegger coalitions in the diffusion of stringent environmental standards, yet such coalitions were apparently absent here.

Many will question Andonova's tendency to privilege institutional factors and perhaps overstate the power of industry. For instance, while we might attribute Bulgaria's lack of compliance with EU air pollution standards primarily to its domestic institutional structure (p. 154), this outcome is heavily overdetermined. Most importantly, of the three states examined in the book, Bulgaria was the only one from the "second wave" of enlargement, and with no chance of the country's joining the EU in 2004, it is not so surprising that regulated firms could put up such successful resistance to expensive policy reforms. Moreover, Bulgaria was by far the poorest of the three states, it had the lowest level of general public support for expensive environmental improvements, and it was the only one that experienced extreme instability throughout most of the 1990s. Likewise, while institutional factors help explain why the Czech Republic was able to adopt EU air standards faster than Poland did, so do public attitudes about nuclear power, a particularly attractive source of energy when trying to cut air pollution. In Poland, public aversion to the nuclear option (and support for local coalminers) was especially strong; in the Czech Republic it was muted.

Critics will also stress that the entire issue of CEE adaptation is a big nested game, with "returning to Europe" as the ultimate constraint and payoff. Thus, Andonova's claim that the chemical and electricity industries could really have vetoed implementation of environmental reforms (pp. 81, 122) appears highly unlikely, since this would have torpedoed accession. True, in Bulgaria, environmental ministers did try unsuccessfully to play the European card as a commitment mechanism to force reform on a reluctant industry (p. 164), but their failure could well reflect the faint prospects of Bulgarian membership, rather than the inherent power of the electricity sector.

It will be interesting to see how well Andonova's arguments generalize to a different selection of cases. To isolate the power of markets, networks, and domestic institutions from the confounding influence of EU accession, one could look, for instance, at diffusion of EU rules to states that had little chance of ever gaining membership, or diffusion to the United States or Asia. One could also look at how CEE states have adopted stringent non-EU environmental rules.

Despite some limitations, *Transnational Politics of the Environment* is an important book that will appeal to environmental policy experts, comparativists, and international relations scholars. It helps bridge the domestic–international divide while providing a wealth of detailed information on the structure and reform of the chemical and electricity sectors in emerging market economies. And it offers an optimistic prognosis about environmental improvement in the many states that already feel the economic and institutional pull of the EU.

Democracy Transformed? Expanding Political Opportunities in Advanced Industrial Democracies. Edited by Bruce E. Cain, Russell J. Dalton, and Susan E. Scarrow. New York: Oxford University Press, 2003. 328p. $49.95.

— John D. Robertson, *Texas A&M University*

For those who assume that within those countries we commonly consider to be the wealthiest and most stable liberal democracies, there exists a single shape, form, and formula for democracy itself, they need to consult this bold, innovative, and informative volume. Bruce Cain, Russell Dalton, and Susan Scarrow have edited a superb collection of 11 chapters (including the introduction), written by 15 internationally renowned students of comparative political analysis (including the editors), dealing with a simple and clear set of questions, namely: 1) Has representative democracy among the most industrially advanced liberal democracies of the world undergone a series of reforms during the past 20 years or so? 2) What is the nature and form of this reform? 3) In what way, if at all, has this change affected the quality and nature of democracy in these countries?

The central thesis guiding the volume is that these most wealthy, stable, and liberal democracies are facing a "third wave" of reform to their democratic politics. Chronologically, the first wave was the rise of representative democracy, replete with elected officials and political parties serving as the primary instruments of voice and opposition to check and balance concentrated elite interests in the political system. The second wave has been the relatively recent rise of direct democracy, which has seen an increased role of the public in distinctly newer and direct forms of input into political decision making, expressed through referendums and the rise of local politics. The third wave is that of advocacy democracy, characterized by the very recent and less readily recognized growth in the channels for direct interaction between citizens and government, and the markedly intense involvement of citizens in the policy deliberation stage of policymaking. This final wave of reform does not spell the end of a definitive elite–public division within democracy. Rather, it opens the way for a significantly modified set of expectations among the public regarding access, transparency, and accountability, with important implications for public inclusion, the degree of political equality, demands for enlightened understanding of policy, an energized and active role in agenda setting and framing, and new thresholds for achieving effective participation.

The authors rely upon a rich array of impressive cross-national empirical evidence to examine the range and depth of democratic reform across the working sample of 18 large advanced industrial democracies. Empirical and quantitative analysis are presented on national election patterns (Dalton and Mark Gray, "Expanding the Electoral Marketplace"); referendum reforms (Scarrow, "Making Elections More Direct"); Parties Manifesto Project results, including saliency of democratic themes in party platforms, such as democratic discourse and inclusiveness of party electorate in key party electoral decisions (Miki Caul Kittilson and Scarrow, "Political Parties and the Rhetoric and Realities of Democratization"); electoral system change (Shaun Bowler, Elisabeth Carter, and David M. Farrell, "Changing Party Access to Elections"); freedom of information laws (Cain, Patrick Egan, and Sergio Fabbrini, "Towards More Open Democracies: The Expansion of Freedom of Information Laws"); subnational government reforms and decentralization laws (Christopher Ansell and Jane Gingrich, "Trends in Decentralization"); administrative and ombudsman legal reforms (Ansell and Gingrich, "Reforming the Administrative State"); and legal reforms (Rachel A. Cichowski and Alec Stone Sweet, "Participation, Representative Democracy, and the Courts"). While extensive, the presentation and discussion of the measures and empirical findings is clear and concise to the reader.

The introductory chapter and the final two chapters offer thorough considerations of the effect of reforms on the nature of democracy and afford the reader prescient insight into the changing nature of democracy. Mark E. Warren's essay, "A Second Transformation of Democracy," leads the reader through the evidence supporting the third wave reforms ("advocacy democracy"), and cautiously suggests that, indeed, the reforms underway do point to a cumulative transforming effect on the very nature of democracy within advanced societies. While the patterns across the 18 democracies differ in many respects, the weight of the collective evidence clearly underscores a modified relationship between the governed and the governing, with dramatic implications for the role of individual rights and juridical politics in democracy, the role and significance of traditional political parties and interest groups, and the manifestation of evolving institutional checks and balances within the various wealthy liberal democracies.

In the concluding essay, Cain, Dalton, and Scarrow focus more on the institutional nature of reform, yet invite the reader to ask what broader implications arise with advocacy democracy. While understandably not central to their balanced and objective assessment, the likely limitations of democratic reform taking hold on a global scale can also be considered. If these 18 wealthy and stable liberal nation-states, which have now advanced toward or are indeed within the realm of advocacy democracy, are prone at times to propose the virtues of representative or even direct democracy to less economically stable, and certainly less democratically experienced nation-states, one might reasonably assume that the public within the latter countries will view the former as perhaps hypocrites. They might ask with some justification, "Should everyone not aspire toward advocacy democracy if the most liberal democracies are on their way to this, or in fact have already arrived?" Why should those nation-states whom liberal democracies have encouraged to join the ranks of democracy in general settle for the second-class status of merely representative democracy when the wealthy few have shown through the test of time and practice the advantage to their systems and public of advocacy democracy? Yet, having weighed the evidence offered in this volume, one might also reasonably ponder whether or not we really want a global environment where advocacy democracy has entirely given way to representative and direct democracy.

Of course, it is understandably beyond the scope of *Democracy Transformed?* to explore the cultural or historical obstacles to achieving the third wave reforms, especially for countries beyond the scope of their sample. Nonetheless, this volume and its various chapters certainly point toward a growing credibility gap between those at or near the advocacy threshold of democracy, on the one hand, and those aspiring to achieve the traditional representational democracy level, on the other. By promptly placing democratic reform among advanced industrial democracies into its proper and yet complex perspective, Dalton, Cain, and Scarrow and their various contributors have perhaps unwittingly drawn attention to a global cleavage between the "haves" of advocacy democracy and the "have-nots" of representational democracy—a cleavage that may have far wider implications than public and elites learning to adjust to complex and interdependent political relationships within stable, wealthy, liberal democracies.

Why Ethnic Parties Succeed: Patronage and Ethnic Head Counts in India. By Kanchan Chandra. Cambridge: Cambridge University Press, 2004. 368p. $80.00.

— Subrata K. Mitra, *University of Heidelberg*

In the post-9/11 world where the "clash of civilizations" has moved beyond classroom debates to the public realm, it is refreshing, and challenging, to see a study that does not give ethnicity an easy ride. The title of this book is slightly misleading, though, because even while it concedes that appeals for political support on the basis of ethnic categories based on "race, caste, tribe or religion" (p. 2) are frequently made, sometimes with considerable success, it asserts that such tactics do not always succeed. When they do, it is not necessarily because of their putative appeal to sentiments but, instead, because both ethnic candidates and their supporters, rather than being swayed by appeals to their nonrational selves, are actually driven by sophisticated calculations of expected gain. Their utility calculus takes the size of the ultimate prize as well as the probability of winning it into account when they choose to align themselves with one set of politicians as opposed to another. Kanchan Chandra's parsimonious and general model explains why ethic parties in India, riding on Hindu or, for that matter, Tamil nationalism, succeed in some contexts but not in others.

Chandra's core argument asserts that "ethnic parties are most likely to succeed in patronage-democracies when they have competitive rules for intra-party advancement and the ethnic group they seek to mobilize is larger than the threshold of winning or leverage imposed by the electoral system." But since the result of ethnic appeals is contingent on "organizational, demographic and institutional variables," the mere appeal to ethnicity is no guarantee of success (p. 15). The author derives these conjectures deductively from revealed preferences of politicians and their perception of expected gain and subjective probabilities, and tests their implications on the performance of several Indian parties that fit the definition. Her data are drawn from quantitative sources, such as the Indian census, electoral statistics, an attitudinal survey of the electorate, elite interviews, and content analysis of political discourse. The analytical capacity of this wealth of information gleaned from multiple sources is considerably enhanced through an innovative application of Gary King's ecological inference (EI) method to estimate partisan voting at the constituency level, to facilitate meticulous, empirically rigorous tests of precisely specified empirical conjectures.

The main contribution of this lucid, elegant, and rigorous study, based on meticulous fieldwork and an innovative take on new techniques, is a shot in the arm for the beleaguered practitioners of statecraft, trying to rein in ethnic politics through institutional design, legislation, allocation of office, or a touch of the old-fashion pork barrel. In an era when advocates of markets, social capital, and "unlimited access to Divinity afterwards" are doing their best to limit the scope of political analysis, Chandra achieves exactly the opposite effect by bringing the lost territory of "ethnic behavior" back into the purview of comparative politics.

The author's use of her model for cross-regional comparison in India is impressive. She explains the performance of the Bharatiya Janata Party (BJP) in the parliamentary elections of 1991 in Gujarat (its best nationwide performance, with 56% of Hindu votes, according to her estimates, using King's EI technique, p. 268), with reference to the party's successful induction of the representatives of Backward Castes into its fold. It was a masterly move by a party with a reputation for its identification with values of the Hindu upper castes. Based on her model, one finds in this case a convergence of interests between the guests, namely, the lower-status entrants to middle-level leadership status within the Hindu nationalist BJP, and the politically entrenched, upper-caste hosts, both of whom stood to gain from the electoral success of the party. To this successful demonstration of the heuristic power of her model, those familiar with the otherness of India might retort that this is so much old knowledge in a new jargon. If, after all, caste matters in Indian politics, then what is new? I shall defend Chandra's analysis here, not because her finding regarding the BJP strategy in Gujarat is new but because of the method she uses to get there. She reverses the causality: Caste matters because politics matters, not the other way round. To make this point, she contrasts Gujarat with the BJP's comparatively poor performance in the eastern state of Bihar, where the same strategy was also tried by the leadership but with considerably less success. There, the elites of the backward castes had a better choice in terms of the Janata Dal, led by their own community, much better placed to win the election, and where, in consequence, the likely pickings were much richer.

The specific empirical puzzle that *Why Ethnic Parties Succeed* has set out to solve, and the repertoire of tools and data that it brings to bear, complement one another. But in the course of its taking shape, the text has acquired new ambitions that set different standards against which to evaluate its significance. When one holds the findings of this book to account for its professed goal to propose "a theory of ethnic party performance in 'patronage democracies'" (p. xvii), the empirical base appears too narrow to sustain so broad a claim. The scope of generalization from Chandra's Indian data is bounded by four restrictive conditions. First and foremost, the model appears underspecified to sustain a cross-national generalization, for it does not take into account the role of the state as an explanatory variable. India, a "responsive state" with her panoply of quotas, legislations, and judicial doctrines facilitates the functioning of ethnic parties in a manner that is not often the case (in comparison, for example, with Sri Lanka). Secondly, even within India, subnational movements and ethnic parties in Kashmir and Assam have had a different itinerary from the examples used by Chandra, which goes to show how credible generalization would require additional geopolitical variables. Thirdly, the peculiar flexibility of the inner structure of Hinduism, where categories tend to be inclusive, suits the author's model rather well, better than, for example, the Shia–Sunni schism in Pakistan or Catholic–Protestant differences might. Finally, though early in the text she attributes the appeal of ethnicity to both material and psychological rewards, the latter disappears from the analysis fairly soon (one can understand why, but that is precisely the point), and the early phases of cultural nationalist movements before they become parties are lost from the story line altogether.

As all those who analyze or merely dabble in party politics must know, many volunteers give their time and efforts to politics, not because they expect tangible rewards but because it makes them feel better about themselves. That is also true of many supporters of ethnic parties. The concept of values, in many ways reminiscent of the politics of evolutionism and idealism, is seldom present on the agenda of contemporary political analysts. This is particularly true for those of the quantitative persuasion. Not taking the intrinsic appeal of ethnicity completely on board makes political analysis vulnerable to the perils of a desiccated, disenchanting modernity and, in the process, to the loss of the empowering role of ethnicity as agency from the scope of analysis altogether. By implicitly raising this issue, perhaps unwittingly, Chandra makes a useful contribution to the scholarly agenda of the relationship of ethnicity and democracy.

Despite the minor caveats, I strongly endorse Chandra's elegant and lucid analysis of India's ethnic political parties. The effective and effortless confluence of area studies and middle-range political theory is one of the finest I have seen in recent years.

The Rule of Law in Nascent Democracies: Judicial Politics in Argentina. By Rebecca Bill Chavez. Stanford: Stanford University Press, 2004. 272p. $55.00.

— Pilar Domingo, *University of Salamanca*

This book is a welcome addition to a young but growing body of scholarly work on rule-of-law construction in new democracies from a political science perspective. Rebecca Bill Chavez develops a persuasive study of the conditions under which rule of law is more likely to emerge, through an analysis of judicial politics at the national level in Argentina and in two of its provinces, San Luis and Mendoza.

Rule of law is defined in the book as meaning effectively limited government in which the actions of public officials are subject to checks and balances through mechanisms of horizontal accountability. Rule of law not only encompasses state agents but also requires that all powerful social or economic actors be subject to controls of legality. A key indicator of rule of law, for the purposes of the study, is the degree of judicial independence in a given political system.

In turn, judicial independence is measured through five indicators: the degree to which the executive violates merit-based appointment mechanisms; the degree to which tenure protection of judges is violated; the degree to which judges' salary protection is vulnerable; variation over time in the size of supreme courts, allowing for court-packing opportunities; and the frequency with which justices rule in favor of the executive in contravention of the law (somewhat more difficult to assess). Judicial independence (and rule of law) is presented as a continuum along which different political systems move in one direction or another over time.

The book analyses the factors that explain the position of political systems on this spectrum. Rule of law is more likely to emerge in the following conditions. First, it is a more likely outcome in a context of competitive party politics and divided government. Under conditions of monolithic political party government with strong internal party discipline and unified government, there are fewer constraints bearing on the executive to refrain from waiving formal rules of separation of powers and engaging in the informal subordination of the judiciary. Second, argues Chavez, the dispersal of economic power fosters rule of law. Competing economic actors will favor strict application of laws protecting property, enforcing contracts, and mediating economic conflict. Where economic power is concentrated in a small elite group and, moreover, if it overlaps with political power, rule of law is likely to be inhibited as unfettered powerful interests will move to subvert judicial independence. A third factor is the degree to which reform coalitions in society are able to mobilize sufficiently to activate mechanisms of legal accountability (even in contexts of concentrated political and economic power). Here, the author stresses the importance of the media as a mobilizing force, and also takes into account the impact of transnational networks and international organizations.

The theoretical framework is tested through a diachronic study of the Argentine experience at the national level in rule-of-law construction and through a comparative analysis of two provinces, San Luis and Mendoza, placed at either end of the judicial independence spectrum. San Luis offers an extreme example of judicial subordination to the executive. By contrast, horizontal accountability mechanisms and judicial independence in Mendoza are exemplary. By examining the evolution of the conditions for rule-of-law construction over time through in-depth analyses of these cases, and by taking into account the indicators of judicial independence, the author is able to evaluate different combinations and sequences in the development of the factors under observation. This qualitative approach allows for a nuanced study of rule-of-law construction and an examination of the different forms of interaction over time and space. Moreover, the case studies are pertinent precisely because they show that similar formal institutional arrangements can yield very different outcomes in terms of levels of rule of law.

While the theoretical premises are developed in a careful and robust manner, they are accompanied at the outset by what might be considered a number of unnecessarily sweeping generalizations that merit more care. Notably, in line with a common tendency in Anglo-Saxon legal analysis, the author falls into the easy trap of blaming the civil law tradition for weak rule of law. While in no way excusing the authoritarian roots of the civil law tradition, it is worth signaling that the political experience of several countries in continental Europe, for instance, points to important success stories in terms of limited government and rooted democratic polities based on rule of law despite code law. While the civil law tradition, as well as the colonial legacy, are by no means irrelevant to our understanding of the workings of rule of law, the book goes a long way toward precisely demonstrating that explanatory factors for weak judicial independence may have much more to do with structural problems than with the legal tradition.

And this is wherein lies the principal contribution of *The Rule of Law in Nascent Democracies*. Moving on from a more formal institutionalist approach—without dismissing its relevance—the book engages in an innovative way of examining rule-of-law construction in terms of real power relations in state and society. What matters, ultimately, is the interaction between certain formal institutional arrangements and the nature of power structures—political and economic—in society. Moreover, the analysis also allows for a dynamic analysis of societal processes through the mobilization of different social actors, which can challenge power structures from below through the activation of accountability mechanisms. The international and transnational context is also identified as relevant.

Chavez has written a theoretically rigorous and innovative work on the complex question of rule-of-law construction that represents a worthy contribution to the literature. Political analysis of democratic processes in transitional societies has increasingly taken on this weighty matter of how to create working mechanisms of horizontal accountability in young democracies. This volume is a healthy reminder that we need to look beyond the formal institutional framework for an understanding of rule-of-law construction.

The River Runs Black: The Environmental Challenge to China's Future. By Elizabeth C. Economy. Ithaca, NY: Cornell University Press, 2004. 368p. $29.95.

— Judith Shapiro, *American University*

Most people are aware of China's severe environmental problems. The country's cities are among the world's most polluted; one of the major rivers sometimes fails to run to the sea; the desert is now only a few hours' drive from Beijing. China's falling water tables, transnational ecological impacts, thirst for energy, and emissions of greenhouse gases and ozone-depleting substances have made its environmental problems an urgent concern not only for its own citizens and policymakers but also for the world. A book that attempts to analyze the causes and implications of this situation is welcome.

The River Runs Black makes the case that China's environmental problems are so severe and challenging that they endanger the country's long-term economic and social well-being. The main argument is that decentralization, which has worked so stunningly to promote China's economic growth, has had poorer results for the environment. An inadequately staffed and underfunded environmental protection bureaucracy, whose status is often inferior to the government agencies and industries it is monitoring, cannot implement China's environmental protection regulations in a uniform and predictable fashion. Instead, rare individual leaders—the mayors of Dalian, Xiamen, and Shanghai, for example—have taken a personal interest in environmental issues, thereby attracting foreign investment and support for environmental projects. The majority adopt a "develop first, clean up later" approach, often with devastating results.

The book includes chapters on history, environmental degradation, governance challenges, nongovernmental organizations (NGOs), the role of the international community, comparative issues, and scenarios for the future. Although much of the text reads as a policy briefing or compilation of English-language secondary sources, it comes to life in a lively chapter on some of the individuals and groups that comprise China's fragile environmental civil society. Leading environmentalists, such as Tang Xiyang, Xi Zhinong, Liang Congjie, and Liao Xiaoyi, have interesting stories of courage, personal setback, and commitment, and Elizabeth Economy tells them well.

China's participation in the global environmental community is also clearly described and useful, with such interesting details as the fact that 80% of the country's environmental protection budget comes from foreign donors. Economy draws a valuable contrast between Japan's environmental aid and concern and the failure of the United States to engage China on environmental issues.

The book would have been enlivened by a detailed portrait of one of the environmentalist mayors and his city, and by greater discussion of the "model green city" concept, now being touted as a possible solution to China's environmental ills. (During the Mao era, the country was required to imitate models like Dazhai, the agricultural brigade, with highly mixed results.) The section on the ongoing use of top-down political campaigns to resolve environmental issues is disappointingly undeveloped (for a sophisticated, field-based discussion of Chinese environmental law-enforcement campaigns, see the work of legal anthropologist Benjamin Van Rooij). These omissions reflect the fact that most of the research for the book appears to have been conducted outside the country.

Economy concludes her book with three scenarios for China's environmental future, one rosy, one mixed, and one bleak. The positive scenario would require "effective application of the rule of law, greater citizen participation in the political process, and the strengthening of civil society" (p. 264). World Trade Organization membership is good for the environment (the author is no antiglobalization activist); electricity from the Three Gorges Dam—and a new natural gas pipeline from Xinjiang province to Shanghai—improve China's energy mix. (Presumably, the dam does not break or silt up, millions of resettled farmers are finally compensated, and in a real stretch of the imagination, the Muslims in the West are allowed to benefit from the exploitation of their resources.) The Chinese Communist Party opens the political process to multiparty presidential elections. Participation in international environmental regimes promotes government accountability, while NGO and media vigilance keeps development schemes from exploiting poor hinterlands for the benefit of richer coastal regions.

In the "mixed" scenario, largely a continuation of current patterns, automobile use explodes without increased fuel efficiency, cleanup is favored over pollution prevention, and Western regions are further despoiled and devastated. The success of a continued "patchwork of environmental protection practices" (p. 268) depends on a handful of environmentally inclined mayors from wealthy cities, and China's NGO sector and international partners remain constrained and frustrated.

Economic downturn causes a third, "environmental meltdown" scenario. Widespread violent protests follow layoffs, the social welfare system is overwhelmed, and the country opens itself to poor environmental practices and polluting industries from abroad. China falls into chaos and civil war, with the environment just one of many causes of mass disaffection.

Economy suggests that the United States has a major role to play in affecting these outcomes, thereby revealing the book to be, at heart, a policy paper. One of her core questions is whether environmental issues will provide a wedge for democratic political reform or revolution. To explore this line of inquiry, she devotes a lengthy comparative discussion to the experiences of Eastern Europe, Russia, and the rest of Asia, where environmental issues have often provided a cloak for democratic activism. However, as oft-discussed as these matters are in Washington, beleaguered Chinese environmentalists avoid them, precisely for fear of being perceived as having a political agenda. Indeed, if Chinese policymakers read this book, they may be tempted to revisit their limited tolerance for environmental groups.

For those unfamiliar with China's environmental degradation, bureaucratic complexities, and the transnational influences and projects detailed here, *The River Runs Black* provides a useful introduction. But despite its dramatic title, the volume is no page-turner. It will not effectively lead students or general readers to care about these issues, nor will it satisfy those seeking fresh analysis of some of the most troubling environmental problems of our time.

Subnationalism in Africa: Ethnicity, Alliances, and Politics. By Joshua B. Forrest. Boulder, CO: Lynne Rienner, 2004. 279p. $55.00.

— William J. Foltz, *Yale University*

Sub-Saharan Africa suffers from states that are too weak to deliver services or even basic order to their peoples. Things really are falling apart, and challenged by a wide variety of locally based resistance movements or popular indifference, control by the center cannot hold. Joshua Forrest, author of solid monographs on Guine-Bissao and Namibia, takes on the challenge of describing and explaining the whole "process of political mobilization by regionally-based forces" (p. 1) that defy central authority, and for which he coins the term "subnationalism." By this he includes a wide panoply of movements seeking autonomy, redress for grievances, secession, revenge, or old-fashioned plunder, so long as they are based on a population or region that is a subset of the juridical nation.

In categorizing these movements Forrest first distinguishes "uni-ethnic" and "interethnic" movements. In both cases, however, building alliances is a fundamental skill, whether the alliances bring together villages, clans, moieties, or whole ethnic groups. Such alliances may be facilitated by precolonial collaborative practices, resurrected and appealed to in contemporary times. Such practices constitute a "historical and cultural *subtext*" (p. 1) that can produce surprising alliances among seemingly disparate peoples. Important as alliance building and precolonial practices may be, they cannot explain the rise or success of autonomy-seeking movements. They are subject to four "overarching causative factors" (p. 2): a history of state intervention in regional affairs; long-term economic inequities; conscious or ascriptive adherence to ethnic or regional identity patterns; and the instrumentalist leadership of movement elites. Beyond those four "overarching" processes, three additional factors from the postcolonial period play a role: changes in the international state system, weakening of ties between center and periphery, and improvement in rebels' organizational capacity.

Those are a lot of variables whose relationship one to the other, definition, boundaries, and metrics are not clear. Whenever the facts provide no comfort to the argument, Forrest seems to stir a few more "factors" into the mix. For example, on pages 54 to 56, "international disfavoring of subnational uprisings," "social convergence and social change," "the inability of some movement leaders to overcome interethnic factionalism," "central-state elites' ability to manipulate ethnic and local leaders," and "the fragility [of] public bureaucracies" are presented as factors inhibiting subnationalism, and six more represented as factors promoting subnationalism. But of course, additional factors may upset the mix and cause another to have a contrary effect (e.g., "bureaucratic fragility"). Then, too, some factors operate both as dependent and independent variables (e.g., regional leaders' ability or inability to repress factionalism).

The reader seeking theoretical clarity on local and regional political movements in Africa may come away frustrated. Nonetheless, Forrest's work makes important contributions to our understanding of the process. First, he provides excellent, brief case studies of subnationalist movements paired according to a core common characteristic. Some of these pairings are surprising, for example, the separatist movements of Senegal's Casamance region and of southern Sudan. For all the differences of scale and habitat, the rebellions share problems of maintaining interethnic alliances and a history of discriminatory treatment at the hands of the central government. Ethiopia's Oromo rebellion and Nigeria's Biafran secessionist movement are paired as examples of single-ethnic separatism, and Forrest shows how each has been limited by a common failure to form alliances with neighboring ethnic groups. Other pairings include Angola's National Union for the Total Independence of Angola (UNITA) and Namibia's little-known Caprivi Liberation Army; Zulu, Afrikaner, and Tigrayan ethnic supremacist movements manipulating appeals to past glories; and a wide variety of "retraditionalizing" movements that, in the absence of effective control by central authorities, have restored or reinvented kingships based on some version of precolonial—and colonial—precedent. Readers may quarrel with Forrest's choice of core factors providing the rationale for the pairings, but they can learn much in the process.

Two lessons in particular emerge from Forrest's choice of cases. First is the importance of building alliances, both within and across ethnic lines. As was the case of nationalist independence movements 40 and 50 years ago, the success of subnationalist movements for the foreseeable future is likely to require ethnic bargaining and alliance formation. As Forrest states, alliances may increasingly be based on patterns of cooperation and trust established in precolonial times. Second, while recognizing that scholars have been right to dispel simplistic views of African primitive "tribalism," they now must acknowledge ethnicity in its many levels and forms as a major factor in much of Africa's social and political life, a factor that requires the careful empirical study that *Subnationalism in Africa* demonstrates, as well as the careful reconceptualization that this book's limitations invite.

Social Protest and Policy Change: Ecology, Antinuclear, and Peace Movements in Comparative Perspective. By Marco Giugni. Lanham, MD: Rowman & Littlefield, 2004. 320p. $75.00 cloth, $32.95 paper.

— David S. Meyer, *University of California, Irvine*

The same things that provoke social movements also promote institutional pressures for change, shifts in public opinion, and policy change. As a result, disentangling the effects of protest on policy presents an ongoing analytical challenge. If we look, for example, at the volatile and diverse movement against the American war in Vietnam, it is virtually impossible to track clear lines of influence between the volume or disruptiveness of protests covered in the news and spending on the war or casualties produced in battle. At the same time, by reviewing memoirs, archives, and the broader outlines of policy, we know that the movement had longer-term effects, including (minimally), ending military conscription and establishing much stricter political and military tests for long-term commitments of American combat forces overseas. The latter set of constraints, codified as the Weinberger, then the Powell, doctrines, held sway over American military policy from the end of Vietnam until, finally, violated by the current American intervention in Iraq. At the same time, politicians of both parties vigorously try to reassure nervous youth, many of whom are children of the protest generation, that the draft is indeed gone for good. Clearly, the protests against the Vietnam war mattered, though perhaps not exactly in the ways that activists hoped or as much as they dreamed.

Moreover, how can a scholar disentangle the independent effect of protests on policy changes from a host of other causal factors, including the high costs of the war, growing opposition in Congress and public opinion, and the growing expert recognition of alternative means for fighting—or managing—the Cold War with somewhat less direct attention to dominoes. Detailed historical exegesis can help delineate paths of influence; at the same time, examining in detail the complicated trails of policy reforms in the wake of one movement at one time does not neatly translate to a template model for evaluating the impact of other social movements on other policy areas, nor does it allow us to understand when and how social movements matter. These difficulties have led many social movement scholars to avoid the study of policy outcomes altogether, turning their attention to a range of other issues more easily specified.

Neglecting the policy issue, generally the outcome most important to movements, their targets, and their opponents, is to miss the main game. Social movements emerge to make challenges on policy issues, and the belief that they might actually change policies is what produces the hope and fear in organizers, participants, and policymakers that leads them to act as if they might matter. To his credit, Marco Giugni has directly embraced the challenges of assessing the effects of movements on policy, and students of movements and of policy will be provoked, encouraged, and grateful.

Seeking patterns and a general model of influence, Giugni looks at three distinct movement policy areas (environmental protection, nuclear power, and peace) in three democracies, the United States, Italy, and Switzerland. He traces the development of distinct, albeit overlapping, movements in each national context, and the development of each policy area, and then uses time series analyses to test different models of how movements might affect policy. The first section of *Social Protest and Policy Change,* entitled "Historical Overview," provides a chapter on the study of policy outcomes of movements, followed by a succinct summary chapter of all three movements in each country, and another on each policy area. The policy summaries focus on easily measured elements of policy, notably budget allocations for all areas, or amount of nuclear power generated for one case.

In the second section of the book, Giugni argues that movement events by themselves are unlikely to have substantial direct influence on policy; instead, they depend upon supportive public opinion and sympathetic policymakers. He advances a *joint effects* model of policy influence, in which the effects of protest events generate policy reforms in the direction they desire when they enjoy favorable opportunities represented by allies in government and public support. To test his ideas, Giugni has collected time series data on protest events, public policy, public opinion, and partisan balance in each country, no small achievement. Using time series regression, he finds the joint effects model works better than direct effects alternatives in explaining policy change, at least as captured through spending. He finds the strongest influence of social movements on environmental protection, some effect on nuclear power, and much less visible influence on national security policy. He argues that these differential effects across policy areas reflect the varied autonomy that policymakers enjoy on each issue.

Scholars will quibble with both the models and their specifications; indeed, Giugni actively invites debate and correction by providing a very helpful and extensive set of appendices explaining his methodological choices and his data. I appreciate the author's commitment to giving his critics enough data to advance alternative explanations. To take the case of nuclear power in the United States, for example, Giugni suggests that the movement faded before it could exercise much influence, and policymakers turned from nuclear power because of the increased costs of constructing and operating plants. He does not discuss, however, how those costs and difficulties increased partly as a result of stricter safety requirements imposed in response to public concerns. The production of nuclear power increased in the movement's wake, but new plant orders disappeared at about the same time as the movement. In the case of peace movements, which emerge when military spending threatens to increase, it is easy to imagine modest increases in spending as the significant effect of mobilized opposition, albeit not what activists hope for.

We can trace out similarly complicated narratives of each movement's influence to run against Giugni's more parsimonious models, and the ensuing discussion cannot help but shed more light on the difficult, but critical, issue of the effect of movements on policy. Just as William Gamson's (1975) classic *Strategy of Social Protest* did 30 years ago, Marco Giugni's important book is likely to inspire another long wave of scholarship on the policy effects of movements.

China's New Nationalism: Pride, Politics, and Diplomacy. By Peter Hays Gries. Berkeley: University of California Press, 2004. 224p. $24.95.

— Yan Sun, *City University of New York*

Arguments in the West over the existence of a "China threat" frequently atomize and even demonize China, as the author of the book points out at the beginning. Is Chinese nationalism benign or malign? The rise of Chinese nationalism in recent times has become an issue of great interest and importance to the world because of concerns over China's intentions as economic growth propels the country's ascendance onto the world stage. This timely account analyzes the sources and dimensions of the new nationalism, from what Peter Hays Gries calls the "rarely told Chinese side of the story" (p. 4). It is premised on a refreshing perspective that "to understand Chinese nationalism, we must listen to the Chinese" (p. 4). Gries's attempt to introduce Western scholars to the views of these new nationalists is an important contribution in itself, as so often mainstream views of Chinese nationalism in the West construe it as a tool to legitimize Communist Party rule. This book gets it right by taking Chinese nationalism seriously and treats it as a matter of genuine popular base and emotional content.

Central to the book is the contention that Chinese nationalism cannot be interpreted in isolation and that Western dismissals greatly oversimplify reality. This point is on target and well grounded. Gries explicitly, and correctly, rejects subjective interpretations of Chinese nationalism commonplace in the Western media and among Western commentators. As a correction, the author insists on the centrality of the empirical context in which Chinese nationalism is situated and must therefore be understood. The latter position may seem like common sense but unfortunately is all too often neglected. In this connection, the book advances four sound arguments. First and second, Chinese nationalism must be understood in the context of the country's relationship with other nations and with its past. Third and fourth, Chinese nationalism involves the Chinese people, not just the party and elites, and it involves their genuine emotional attachments to their national identity.

Despite the author's good intentions, sound perspective, and strong arguments, the book falls short of delivering what it aims at: to tell the Chinese side of the story. It is still largely his account of that story, with what one may call an outsider's interpretations. I say "outsider" in the sense that a Chinese may read the same texts quite differently from Gries, and what Gries reads from a Chinese text may surprise a Chinese reader of the same material. Among the constant sources of the nationalism identified by Gries are the father–son relationship, the old and younger-brother relationship, face, moral authority, and self-image. Other than self-image, which may be construed as a cultural universal, the rest do not hold up convincingly and can take the Chinese by surprise. Granted, they are not a monolith and may differ over the sources of their nationalism. But one will have to go back to the early nineteenth century to find a Chinese who might base his national identity on Confucian concepts of moral authority or hierarchal relationships.

In his first substantive chapter, on how Chinese identity has been defined by dynamic interactions with the West, particularly the United States, Gries finds the teacher–student relationship to be a prominent metaphor in Chinese writings about America. He cites prominent figures in China's modern history to show the people looking up to America as teacher. But to say one has something to learn from a friend or one looks up to the friend as role model is quite different from saying that the friend is your teacher. Gries's contention that new Chinese nationalists today "clearly want to exchange roles within their teacher-student relationship with America" (p. 34) is even more stretched, and unsubstantiated even by his own evidence. In all instances where Gries asserts that the Chinese are casting China as father to the United States, one finds no reference to the idea in the the original source.

For example, citing this quotation from a Chinese magazine—"Facing an ancient Eastern colossus, America is at most a child"—Gries asserts that "by 'altercasting' America as a child, China can play the superior elder" (p. 33). In another citation of two Chinese authors who declare the United States to be a "spoiled child," Gries again asserts that by "altercasting America in the role of child, Li and Qiang depict China as a parent—and clearly believe that parents have both the right and responsibility to discipline their children" (pp. 34–35). But where did the two Chinese authors say or even imply that China may be the "father"? These are all Gries's own inferences. Most Chinese would understand the casting of America as a child to mean that America is a young country with a short history, or that the American way of acting as it wishes in the world is childish.

Such subjective interpretations abound in an otherwise rich and lucid empirical account. Recounting how Chinese nationalists enjoy retelling the story of Margaret Thatcher falling off the stairs after meeting with Deng Xiaoping, Gries comments that "the message is clear: the British must kneel down and beg forgiveness from their betters" (p. 51). This interpretation is not normally what a contemporary Chinese would think of. Instead, the Thatcher story suggests to the Chinese the metaphor of the downfall of a once arrogant colonial overlord. In discussing how Chinese self-image has been influenced by Sino-Japanese relations, Gries repeatedly emphasizes the Chinese presumption of moral superiority and teacher–student hierarchy. But nowhere in this chapter does he point to the basis of the Chinese assumption of Japanese moral inferiority: Japan's aggressive war of the 1930s and 1940s and its lingering reluctance to show genuine remorse.

Gries's victimhood thesis excellently captures an important source of contemporary Chinese nationalism. Unfortunately, his further elaboration only shows more lack of understanding for the Chinese victimhood narrative. Here, the author appears to be more critical of the "obsession" with Japanese atrocities than of the nature of Japanese actions (regardless of the exact death toll during Japanese invasions). Contrary to Gries's claims of the Chinese pursuit of "face" in the world on this matter, the Chinese are bothered that they, as victims of the Japanese war, are now frequently demonized in the Western world as possible aggressors in the future, whereas those refusing to fully acknowledge war guilt are not thought of as likely aggressors. The victimhood literature in China is also often used to assert the point that as past victims of foreign aggression, the Chinese are not likely to revisit the pain on others. Gries is also on weak ground when he lumps Iris Chang, the author of The Rape of Nanjing: The Forgotten Holocaust of World War II (1998), together with the mainland Chinese. Chang is a Chinese American, with primarily America-bred sensitivities and outlooks. In depicting Chinese efforts to display Japanese war atrocities as obsessive, "face" saving, and satisfying a need for "foreign validation" (p. 84), Gries fails to ask a different question that will help him better understand the Chinese mindset: How would the world react if Germany reluctantly apologizes for its war atrocities and its leaders pay annual visits to shrines honoring the country's wartime soldiers and officers?

In another otherwise well-chosen topic, the Sino-Japanese and Sino-American "apology diplomacy," Gries again wrongly locates the Chinese motives. "Apologies are about power relations. Offenses to the social order threaten established hierarchies, and one way that the aggrieved can regain social position is vengeance" (p. 89), writes the author. He fails to appreciate the cultural imperative of apology in not only Chinese but also Japanese culture. Apology in Confucian cultures is not about power or hierarchy, but acknowledgment of the source of wrong and exhibition of contrition. The party that apologizes sincerely is easily forgiven and reparations absolved. The fact that the Chinese have not accepted Japan's repeated apologies is not due to obsessions with power but to the fact the Japanese can apologize in one instance but visit war shrines in another. Such inconsistencies, in other words, show lack of sincere repentance.

Likewise, in the Belgrade bombing of the Chinese Embassy and the U.S. spy plane incident, the seeking of apologies was the seeking of justice. In this case, the Chinese may have been even more surprised by the American perception of no need for apology than the Americans were surprised by the Chinese demand for apology. After all, the U.S. spy plane was all the way off the Chinese coast and the Chinese embassy was bombed, not the other way around. To Gries, "[a]pologies are another means of restoring threatened social hierarchies. The form an apology takes depends critically upon the relative status of the parties involved" (p. 89). But to the average Chinese, apologies are means of showing judgment on right and wrong, and the form of apology depends on the degree of wrongs involved.

My comments should perhaps not be read so much as criticisms of Gries as pointing to a deeply troubling question: If Gries, a student of Chinese politics with a childhood background in China and a consciously "Chinese-oriented" perspective, is not able to "get it right" about the nuances of Chinese nationalism, then misunderstandings and misconceptions should be even worse among mainstream Western commentators. But his earnest effort is a major step in the right direction toward better understanding.

Despite the Odds: The Contentious Politics of Education Reform. By Merilee S. Grindle. Princeton: Princeton University Press, 2004. 288p. $55.00 cloth, $22.95 paper.

— Jean C. Robinson, *Indiana University*

No one argues with the claim that education is a necessary ingredient for any recipe for economic and social development. With higher rates of literacy and numeracy, with more years of schooling, a cornucopia of benefits emerge—from lower birth rates to healthier workers to more autonomous women to more engaged citizens. And yet despite the recognition that everything good comes from education, expanding and reforming public education is a costly and contentious process in all political systems. Scholars and politicians alike are pessimistic that educational reform can be instituted. Given the costs and the conflict, what can explain successful implementation of educational reforms?

Merilee Grindle's study is a thoughtful and carefully developed analysis that explains the conditions under which educational reform has been successful in Latin America, despite the odds. In the process, the author provides us with a wise approach that focuses on the political processes of successful policymaking and implementation, rather than on the barriers to reform. Grindle, known for her work on policymaking, has recently made several calls to scholars to engage in the analysis of institutional reforms that have worked. This full-length sequel to her chapter in Joseph S. Tulchin and Allison M. Garland, eds., *Social Development In Latin America: The Politics Of Reform* (2000) and to her contribution to Carol Graham, Merilee Grindle, Eduardo Lora, and Jessica Seddon, *Improving The Odds: Political Strategies For Institutional Reform In Latin America* (1999) makes good on her plea that political scientists interested in policy need to shift their gaze from the obstacles to policy reform and focus more on explaining how it is that some reforms are actually working.

What Grindle has done here is to present, much in the vein of the process tracing one sees more and more in institutional narratives by sociologists and political scientists, the complex multifaceted stories of educational reform in the 1990s in Mexico, Bolivia, Ecuador, Nicaragua, and the state of Minas Gerais in Brazil. In explaining how the reforms have made it past the opposition of teachers' unions, the power of entrenched educational bureaucracies, and the self-interest of political actors, she focuses on the strategic choices of executive leaders. She is no naive policy analyst who ignores the reality that "almost any proposal to alter policies will engage interests opposed to change" (p. 196). Rather, she presents us with a carefully crafted research design that shows how leaders have been able to create strategic opportunities to open up possibilities for reform.

Do not look to this book to learn about the effectiveness of the quality-enhancing educational reforms undertaken by many Latin American governments. This is not an evaluation of educational policies per se but, rather, a study of the processes of policy change. It could as well be about welfare policy or environmental policy, although we do learn a lot about the efforts to create policies that might indeed enhance the quality and extent of public education in Latin America. The real strength of Grindle's cases is that they point to the critical importance of strong political leaders who care to force changes even if entrenched powers resist. In some ways, this is a story of strong and savvy leaders—not charismatic, perhaps, but leaders who are convinced that governance is about leading. The author does not address this angle because her focus is on the politics of reform efforts, but it is there. One senses that without Governor Hector Garcia's insistence, reforms would not have happened in Minas Gerais; or that the sheer stubbornness of Amalia Anaya, who worked with at least three different presidencies was critical in ultimately resulting in educational reforms in Bolivia. I would like to have seen more analysis of the emergence of these reform entrepreneurs, or reform-mongers, as Grindle calls them. Under what conditions do they emerge? Are there institutional contexts that are more likely to give rise to such reform leaders?

But this, after all, is more than an analysis of leaders—it is also an analysis of how strategic choices get made. The contribution of this comparative study is that while Grindle enables us to learn relevant details about Mexican and Ecuadorian presidential politics, the most important lesson is that successful policy reforms happen because of the strategic decisions of reformers. These start with choices about when and where to raise the need for reform, continue on to decisions about stepping back from advancing reforms while continuing to build support through think tanks and initiatives outside the center, and then managing the political conflict—sometimes by co-opting, sometimes by negotiating, sometimes by containing—that necessarily arises when the reform is brought back to center stage.

The argument is based on solid comparative case studies that highlight the major sources of opposition in reform efforts: the bureaucracies, political parties, and teachers' unions. Organized not by national case study but by serial examination of leadership choices, policy development, political opposition, and implementing and sustaining reforms, Grindle's book draws well-documented conclusions about the ability of reformers to stand up to opposing interests and to "capitalize on their own institutional sources of power" (p. 189). It is a comparative study in the best sense, gathering evidence and testing hypotheses by examining "sequences of strategic interactions" (Peter A. Hall, "Beyond the Comparative Method," *APSA-CP* 15 [Summer 2004]: 1–4).

Scholars who are looking for rational choice explanations of reform, or for quantitative empirical analyses of institutional strength and weakness, will be disappointed in this study. What I found most engaging about *Despite the Odds* was its insistence that politics, and the management of conflict, really do matter. Time and again, political economists remind us that policy change—whether educational, environmental, or social—should not be able to emerge because self-interest gets in the way. And yet, in fact, change does occur. Deft handling of comparative case studies shows Grindle at her best, and demonstrates that policy reformers can and have overcome resistance, not by ignoring politics but rather by engaging it.

Democratic Reform in Africa: The Quality of Progress. Edited by E. Gyimah-Boadi. Boulder, CO: Lynne Rienner, 2004. 351p. $59.95 cloth, $23.50 paper.

— Lisa Anderson, *Columbia University*

There are fads in political science and in the policy world and the 1990s produced a prominent example: the decade's virtually universal infatuation with democracy. The end of the Cold War seemed to have removed the last remaining impediments to democracy—communist tyranny and subversion—and around the world, humankind's natural inclination toward freedom and equality was expected to be exhibited in heart-warming periodic elections. Or so it seemed. Policymakers celebrated the collapse of authoritarian regimes and welcomed new nations into the community of democracies; political scientists busied themselves with analysis of the mechanics of transitions to democracy, the technicalities of democratic institutions, and the myriad contributions of civil society.

By the end of the decade, intimations that all was not what it seemed were beginning to surface. Not all elections were producing benign and enlightened policies, much less policymakers. Indeed, some elections were little more than competitions among autocrats, others but a prelude to civil unrest and conflict. The transition to democracy was not nearly as straightforward as had been hoped.

By the end of 2001, and thanks partly to the attribution of the events of September 11 to a band of criminal zealots nestled away in a long-forgotten (and hardly democratic) corner of the world, democracy was fast fading as the policy dilemma and scholarly puzzle of the moment.

Suddenly, states, particularly failed, collapsed, and fragile states, were the project of the day. Not only were they obstacles in the development of democracy, but they also seemed to be potential sources of international threats, including drugs, crime and terrorism.

Democratic Reform in Africa displays this paradigm shift in its very pages. Reflecting their normative and theoretical commitments, the authors are reluctant to abandon the focus on democracy with which they began. The outgrowth of a conference held in June 2000, the book was clearly intended to be a sort of midterm review of the progress of African states toward democracy. Its authors are too good, too honest, and too alert to what had been going on in Africa, however, not to have noticed and not to acknowledge the fragility of not only the democracies but also the very states of the region. As a result, there is a subtle but discernable tension in a number of the contributions, as the reality of collapsing states shows through the worn cloth of democratic facades.

The book's editor, E. Gyimah-Boadi, tacitly acknowledges this tension in his introduction, saying that while "the results of the reforms, especially in the political realm, have sometimes been outstanding . . . this volume also confirms that success is not the only theme in the story of Africa's reform experience" (p. 2). Gyimah-Boadi, both in his introductory overview and his contribution on "Civil Society and Democratic Development," and Larry Diamond, author of the volume's concluding reflections, are the most reluctant to abandon democracy and democratization as the driving analytical perspective. Most of the other contributors are not only far less sanguine about the prospects for democracy; in fact, they are far less committed to the analytical framework imposed by an emphasis on democracy in theory.

Indeed, it would be a pity if policymakers and political scientists interested in issues of economic reform, conflict, and state formation and deformation passed up this volume because it seemed to be yet another exemplar of the now vast and deeply unsatisfying literature on democratization. It is that, but it is also, and more usefully, an early volley in what promises to be an equally vast effort to understand exactly what the end of the Cold War *really* meant for politics in Africa.

From Michael Bratton and Robert Mattes's "What 'The People' Say about Reforms" to Steven Freidman's "South Africa: Building Democracy After Apartheid," the contributors report over and over that if democracy does not "deliver," people are willing to move on to something else. As Friedman puts it, democracy was advertised "as a means to material ends (which may, presumably, be dispensable if superior means present themselves)" (p. 237). Africans appear to be sick and tired of living with promises unfulfilled, services undelivered, and futures unsecured, and they are increasingly willing to consider supporting someone—anyone—who might make the trains run on time. That might be a technocrat (see Bratton and Mattes, p. 74), but it might be a crook. As Sahr J. Kpundeh points out in his excellent "Corruption and Corruption Control," "recent empirical evidence suggests that corruption thrives where the state is unable to protect private property and contractual rights or to provide institutions that support the rule of law" (p. 124). Protection rackets have the great merit of providing protection, and when no one else is doing that, rackets have considerable appeal.

Similarly, there is a bracing candor in several essays, notably Stephen John Stedman and Terrence Lyon's "Conflict in Africa," about the corrosive quality of violence in many parts of the continent: "Wars in Africa," they tell us, "are increasingly becoming regionalized and the distinction between civil war and international war less meaningful" (p. 143). This does not bode well for the stability of some of the most important countries in the region. In Nigeria, for example, as Adigun Agbaje observes, "ethnic and regional groups and their militias have equally become a major part of the political landscape, often inflicting or threatening to inflict violence for political advantage. . . . These . . . altercations persist amid official fear about an increase in the illegal importation of small arms into the country by various groups and from literally all entry points into Nigeria" (pp. 218–19).

The empirical evidence amply displayed in this very useful volume does not support a focus on democracy and democratization, however much we—and its authors—may wish it did. The Africa described here is not a land of miracles, of development against the odds, of the triumph of liberty against doubters and naysayers. It is a much more predictable, pedestrian place, a place of ordinary aspirations to, and daily struggles for, law and order, clean drinking water, good health, and, not least—but not only—reliable, accountable government. It is a credit to the authors that, however reluctantly, they provide us with the evidence to draw that conclusion.

Transnational Identities: Becoming European in the EU. Edited by Richard K. Herrmann, Thomas Risse, and Marilynn B. Brewer. New York: Rowman & Littlefield, 2004. 328p. $75.00 cloth, $29.95 paper.

— Gerard Delanty, *University of Liverpool*

The extent to which people in European countries are becoming more European in their identity is the subject of this very timely book, which presents important new empirical data on the Europeanization of identities. A variety of approaches, ranging from social psychology and political science to ethnographic and discourse analysis, explores the many ways European identities are being transformed. The contributors want to establish the degree to which both citizens and elites identify with Europe and how such forms of identification relate to other identities, in particular, national identities. The aim of the volume is less theoretical and philosophical abstraction than to devise middle-range approaches to measuring transnational identity and to assess its impact and significance.

There are at least two different levels of analysis in this endeavor. One concerns the increasing extent to which people identify with the European Union and the question of whether one can speak of a European identity emerging out of these identities. The second concerns the extent to which the EU is itself able to articulate an identity. These two levels are not quite the same. The first level is one that can only be established by an analysis of the concrete social identities of individual citizens, whereas the second must be established by a different means, such as the analysis of policy statements, discourses of different kinds, and the identities of elites. An introductory chapter by Richard Herrmann and Marilynn Brewer offers a useful conceptualization of social identity, which can be seen in terms of three aspects, which together define the relation between individuals and a group: It defines the composition of the group, the symbolic attributes of the group, and the ways the group is differentiated from other groups.

The three chapters in Part I are written by social psychologists, who share a concern with looking at identity in terms of individual group relations and dynamics. Glynis Breakwell introduces identity process theory, arguing that identities are a product of interaction and are in a constant process of change, rather than being simply fixed. She claims that Europe is an empty and changing category and does not itself have an identity but nevertheless influences individual identities. This is a position that is skeptical of an overarching supranational European identity. One way Europe has become a basis of identity is the degree to which it is perceived as a real entity. Emanuele Castano thus argues that the EU must acquire a psychological existence in the minds of citizens. This suggests the importance of a cognitive dimension more than a symbolic identification. Amélia Mummendey and Sven Waldzus document the tendency for national models of collective identity to be projected onto a common sense of Europeanness, with the result that European identity might simply increase cross-national animosities. However, they also note the representations of Europe that emphasize diversity.

The next three chapters deal with European identity as expressed in the identities and discourses of elites. Brigid Laffan, a political scientist, argues that the EU has become a powerful social construction but is not an alternative to the nation-state. As a normative, symbolic, and cognitive entity, it has become embedded in the process of Europeanization. This analysis insists on the multiple orders of Europe, but does not claim that there is a supranational European identity. The next two chapters employ discourse analysis. In a study of EU delegates and civil servants, Ruth Wodak finds common definitions of being European. Similarly, Euginia Siapera reports on interviews with journalists who cover the EU, showing how three different repertoires influence the construction of Europe. This points to a constructivist view of Europe as created in discursive contexts, rather than being an underlying identity. Moreover, it suggests less a supra-European identity than a multiplicity of European identities.

In Part II, the chapters concern the extent to which identification with Europe is not exclusively an elite phenomenon. In one of the most important chapters in the volume, Jack Citrin and John Sides show by means of survey data that while identifications with Europe are not as intense as national identification, complementary attachments to nation and to Europe are increasing. While relatively less than 10% put Europe first, a significant and increasing number express equal attachment to Europe and the nation. As a result, there is a decreasing number of people who say that they are more attached to the nation than to Europe. This is a significant empirical finding. The next chapter by Michael Bruter makes the important point that European identity is conceptually different from support for European integration and can be researched in terms of its civic and cultural expressions. Ulrike Meinhoff's chapter does not explicitly address European identity, which, she wonders, could be a product of closed questionnaire survey formats. Her chapter instead is based on in-depth interviews on issues that are not central to the volume as a whole.

To the extent to which a common theme underlies these diverse studies, it is a social constructionist approach to Europeanization, as Thomas Risse suggests in an excellent closing chapter. European identity is expressed not just in the awareness of the cultural diversity that constitutes Europe, but in the formation of new and more reflexive kinds of identity, which draw from many different kinds of collective identity, ranging from ethnic to national to EU.

One of the main conclusions of *Transnational Identities* is that the increase in individual identities that have a relation to Europe does not amount to a common European identity. This is partly because of the diversity of views as to what constitutes Europe and the fact that many attachments to Europe are compatible with national expressions of belonging. It follows from this that the EU does not have to compete with national identities for it to create an identity, although this is unlikely to be a singular identity. Not surprisingly, then, many of the contributors disagree with neofunctionalists, who would see institutional changes leading to changes in identity, and disagree with supranationalists, who see a zero sum relation between national and European identities.

This is the most up-to-date collection of studies on measuring European identity and brings research significantly beyond the limitations Eurobarometer studies. The only defect in an excellently edited volume is that some of the chapters are more concerned with methods than with substantive conclusions, and where they are attempted, the results are limited by relatively restricted empirical studies, with the result that these chapters inevitably offer meager generalizations. An exception in this regard is the excellent chapter by Citrin and Sides, and Thomas Risse's conclusion partly compensates for this tendency.

Development Projects for a New Millennium. By Anil Hira and Trevor Parfitt. Westport, CT: Praeger, 2004. 216p. $74.95 cloth, $29.95 paper.

— Goran Hyden, *University of Florida*

This book fills a vacuum for those interested in development policy and administration. Development administration has been a subfield very much in limbo for the past two decades as the market and civil society have attracted prime interest among development analysts and practitioners. Anil Hira and Trevor Parfitt argue that development assistance agencies, such as the United States Agency for International Development, its bilateral counterparts in Europe, and the World Bank, continue to insist on a mode of operation that perpetuates the same weaknesses that were identified in earlier literature on development administration and management. These agencies insist on short time spans for the activities that they fund. They treat these projects as if void of people. They fail to share information with potential beneficiaries. Above all, they do not hold themselves to the same high standards of accountability and transparency that they impose on recipients of their aid. With more and more development activities being carried out by community-based organizations and other nongovernmental bodies, these aid agencies have increasingly become part of the problem, not just the solution. The gap between donor and recipient has simply become too big to overcome.

Development Projects for a New Millennium is organized into seven chapters of varying length. It begins with a useful overview of development aid in the post–Cold War era. It focuses on why foreign aid has become less important and why new concepts as well as theories in aid delivery have evolved to reflect postmodernist influences on what development entails and how it may be practiced in an era when positivist assumptions are no longer taken for granted. The subsequent three chapters examine the shortcomings associated with dominant approaches to project analysis and implementation. The authors point to the blueprint approach to project design, logical framework analysis, and environmental impact analyses as examples of inappropriate and unhelpful approaches to development. The two authors also remain critical of recent initiatives aimed at bringing about more policy dialogue with aid recipient governments because the terms for these consultations are really set by the donor organizations. Chapter 4 is a critique of efforts at decentralization by the donors. In the view of the two authors, these have been conceived first and foremost as administrative and cost-saving measures instead of providing true opportunities for people to have a say in development.

The next two chapters introduce the concept of participation and how various forms of participatory analyses have come to dominate the mode of operation of nongovernmental organizations (NGOs). A distinction is made between forms of participatory analyses that are primarily consultative and those that are empowering. The growth of a gender perspective in development has encouraged an increasing emphasis on empowerment, not just of women but also of other groups that hitherto have lacked a voice in public debates. The book ends with a discussion of project evaluations where the main point is that donor agencies have great difficulty in accepting independent evaluations and rarely really learn anything from these studies that shape their practices.

I find the greatest value of this book lying in its historical perspective on ideas and practices in development policy and administration. It is a useful text in this subfield, whether the student's interest is in policy analysis or implementation. It demonstrates how, despite shifts in approach, reductionist ambitions have prevailed at the cost of consideration of context. The concluding chapter on evaluations includes a list of measures that the two authors consider could be done to enhance the prospect that development projects will really yield anticipated outcomes. A longer time horizon for planning and evaluation, more input from potential beneficiaries, participatory forms of evaluation, and the need for considering a project's political sustainability are among measures that are recommended. Unfortunately, the authors do not provide much evidence of how the measures can be made to work. Above all, they do not take their argumentation to its logical conclusion by asking whether improvements are possible so long as donor agencies remain as dominant as they are, especially in sub-Saharan Africa.

There are increasing calls for restructuring the ways donor funds are disbursed in recipient countries. The bilateral agreements that each donor seeks with a recipient government or an international NGO put definite limits on accountability, transparency, and, not the least, sustainability—political as well as economic. These critics believe that a new phase in development management would only come if and when donors are ready to treat foreign aid as investments in public trust funds, incorporated in recipient countries and jointly managed by boards made up of representatives appointed by the recipient government, civil society, and resource providers. By letting these funds serve as intermediaries, much of the direct pressure from donors to micromanage would cease. A more demand-driven approach to development would emerge. Perhaps most importantly, this approach would facilitate local institution building and lay the foundation for more democratic forms of governance in society at large. The two authors do not consider this approach, but it is a more realistic route to the reform of development management than trying to wed participatory forms of development to the imperatives of bureaucratic organizations like the donor agencies. Bernard Schaffer ("The Deadlock of Development Administration," in Colin Leys, ed., *Politics and Change in Developing Countries,* 1969) drew attention to this impossibility already 35 years ago. It is a pity that the authors in their postmodernist euphoria do not go further to examine whether their own calls for action really can be acted upon without a more radical shift in the way that donor agencies themselves operate.

Political Parties After Communism: Developments in East-Central Europe. By Tomáš Kostelecký. Washington, DC: Woodrow Wilson Center Press, 2002. 240p. $25.95.

Political Parties in New Democracies: Party Organization in Southern and East-Central Europe. By Ingrid van Biezen. New York: Palgrave, 2003. 256p. $69.95.

— Seán Hanley, *University College London*

Much previous work on political parties and party systems in new democracies has examined them as a means to democratic consolidation and regime stability. Two new studies seek to give finer-grain comparative analysis of party development in the relatively successful new democracies of Southern and East Central Europe. Tomáš Kostelecký's *Political Parties After Communism* aims to give a broad overview of the development of party politics in Poland, Hungary, the Czech Republic, and Slovakia. Kostelecký first outlines the historical evolution of parties in the four cases from the midnineteenth century until the collapse of communism and then gives a detailed survey of the development of parties and electoral politics between 1989 and 2002. Subsequent chapters take a more thematic approach, reviewing and synthesizing a range of research to assess the impact of political culture, historical legacies, social cleavages, and the institutional "rules of the game" on party development. A concluding chapter weights these different factors and seeks to highlight broader trends across the region. These are then contrasted with current patterns of party development in Western Europe.

Kostelecký argues that despite high levels of electoral and organizational volitility, party systems in the four states have acquired discernable patterns of left–right competition. These patterns vary depending on the relative importance of cultural and moral issues and the extent of the political right's enthusiasm for the free market. Such crystallization is underpinned by a growing rationality on the part of voters when making party choices; by a growing correlation between social characteristics and political opinions; and by the establishment of a degree of linkage between parties and social interests, albeit largely detectable at the level of aggegate voting partterns. Such social interests reflect a combination of precommunist cleavages, divisions generated by the communist system itself, and more recent conflicts generated by postcommunist reforms. Such interest-related issues, Kostelecký claims, have gradually displaced the personality and identity politics that characterized the early postcommunist period. In acquiring clearer sets of programmatic divisions and firmer social linkages, the author suggests, East Central European party systems are moving in the opposite direction from those of Western Europe, where class-based, ideological party politics has undergone extensive de-alignment in recent decades. Paradoxically, however, despite their differing trajectories, party systems in the two parts of the continent are coming together around a weak form of class politics, a process the author describes as "limited convergence" (p. 168).

As a general survey, this is a curiously uneven work. It has a strong bias toward examining historical and social-structural factors at the expense of institutions and political processes. While Chapter 5, for example, on the impact of electoral systems is barely 14 pages long, the preceding chapter on social cleavages extends to some 50 pages. Moreover, even within this extended discussion of cleavage, fully 20 pages (pp. 117–36) are devoted to gender divisions—an important and neglected topic, but not, according to the author's argument, a key influence on party competition. Class and socioeconomic cleavages, by contrast, which he sees as informing party competition in all four cases, merit only an eight-page discussion (pp. 106–14). Given the author's background as a sociologist and political geographer, it is disappointing that he did not choose to develop broader arguments or engage with any of the influentual literature relating patterns of postcommunist party competition to varying structural-historical pathways through communism.

The more limited argument that Kostelecký presents—that there has been a shift across the region from a "politics of symbols" to a "politics of interests"—also requires elaboration, as it leans heavily on findings from the Czech case. There is considerable evidence that in Hungary and Poland, socioeconomic issues, while more important to party competition, are framed in "value" terms by both Left and Right for whom issues of identity remain central. Accordingly, in these states, party electorates are heterogeneous cross-class alliances closer to those found in U.S. politics, rather than the traditional European division between economic "winners" and "losers" reproduced in the Czech Republic. There are also some clear gaps in Kostelecký's analysis. Despite noting that East Central European parties' lack of cohesion and stability makes assessing the party system consolidation difficult, the parties' internal dynamics and organizational life are not considered.

This institutional dimension of party development in new democracies is the topic of Ingrid van Biezen's *Political Parties in New Democracies.* Van Biezen seeks to identify how the origins of political parties in Europe's newer, post–1974 "Third Wave" democracies may have influenced their organizational development and internal politics. She does this through four detailed case studies of parties in Spain, Portugal, Hungary, and the Czech Republic. Rejecting comparison with party developments in contemporary Western democracies or generic models of democratization, she argues for focus on periods of party formation capable of relating specific patterns of democratization to specific patterns of party development. In practice, this entails comparing the emergence of parties in the Third Wave cases with the formation of parties during the "First Wave" of democratization in late-nineteenth-century Western Europe. Van Biezen argues that the Third Wave differed from the First in that it rapidly extended political competition, rather than gradually extending participation in a restricted but already competitive political system. Such differences, she claims, have consequences for parties' organizational development. First Wave parties tended to be mass parties with deep social roots, which represented the class interests of previously excluded groups. Third Wave parties, she hypothesizes, by contrast, should be top-down elite creations preoccupied with legislating on the broad institutional issues stemming from the introduction of democracy, rather than representing society. The existence in the late twentieth century of electronic media and the advent of state funding as a democratic norm, she suggests, would give few incentives for parties in Third Wave democracies to develop mass organizations or sink deep social roots. Rather, they should favor catchall electoral strategies, high levels of professionalization, small, inactive memberships, and a concentration of internal party power in the hands of parliamentary elites.

Van Biezen then tests these hypotheses empirically. She first explores party origins, organization, and funding in a detailed chapter on each national case. These chapters bring together an impressive array of primary data and secondary sources on democratization and party development in each state, although in the East European cases, the author is clearly handicapped by a lack of language skills. She concludes her analysis with three more comparative chapters examining parties' internal power dynamics, funding, and organization across the four cases. Overall, the author's hypotheses are confirmed. Parties in all cases demonstrate a clear trend toward etatization, elite domination, and catchall electoral politics. This is especially pronounced for Hungary and the Czech Republic. The one surprising finding is that it is party executives, not parliamentary elites, who tend to wield most internal power and control most resources. This, van Biezen suggests, may reflect the need of parties in new democracies to control and discipline legislators with low levels of party loyalty.

As with Kostelecký, Van Biezen's attempts to find common trends across all cases leaves largely unexplored *differences* between and within cases, which might yield further insights. Not only are Southern European parties organizationally more developed than the East European cases—seemingly a legacy of contrasting nondemocratic regimes—but in all four cases, "historic" parties—often former Communist parties—often diverge sharply from the expected pattern, seemingly because of ingrained organizational and political traditions. In stressing modes of democratization, funding norms, communications technology, and formal party rules, van Biezen's analysis also tends to overlook the importance of the real political dynamics of the four states. How, for example, might one explain the recent transformation of Hungary's center-right FIDESZ from the archetypal cadre party she describes to a social movement with mass participation and affiliated interest groups? Finally, notwithstanding the stress on party formation, the use of Western Europe as reference point is also perhaps problematic. Granted, as both Kostelecký and van Biezen note, much party theory derives from the West European experience. Western European parties have also served as both political models and political allies for those in new democracies. We should also note the European integration in pushing forward the "limited convergence" of parties and party systems across an expanding European Union. Ultimately, however, it is perhaps not that surprising that patterns of party formation and party competition in newly democratic Southern and Eastern Europe did not closely resemble those in Western Europe. Scholars of party politics in Europe's newer democracies could perhaps benefit from rigorously thinking through from first principles the role and nature of parties, in the manner, for example, of John H. Aldrich's (1995) *Why Parties?*

Overall, Kostelecký's *Political Parties After Communism* offers an accessible, if uneven, overview of party development in East Central Europe, but few new ideas or arguments. Van Biezen's *Political Parties in New Democracies,* by contrast, is a more original and substantive piece of work, which makes valuable linkages between patterns of democratization and party organization and presents important and, in places, surprising new findings.

Free Market Democracy and the Chilean and Mexican Countryside. By Marcus J. Kurtz. New York: Cambridge University Press, 2004. 264p. $70.00.

— Heather Williams, *Pomona College*

Marcus Kurtz makes his contribution to a sizable body of works examining the impact of economic liberalization on political opening in Latin America, arguing that the countryside in Latin America offers us a crucial piece of the puzzle of why transitions to democracies may succeed despite widening social inequalities. The answer to this paradox, Kurtz argues, is that "competitive national-level democracy [is] based in part on conservative hegemony and peasant quiescence in the countryside" (p. 21). He contends that processes put in place by market-based policies, rather than coercion or clientelism, explain why peasants and rural workers continued to back the politicians who initiated and deepened programs of credit deregulation, price liberalization, and trade opening. Thus, the grim lesson of *Free Market Democracy and the Chilean and Mexican Countryside:* If those with the most to lose in neoliberal conditions are quickly atomized and dispossessed by transitions to market-based conditions, competitive multiparty electoral systems will endure at national levels. Or, in a semantic twist worthy of Alice in the rabbit hole, in modern conditions, democracy thrives on a certain lack of it.

Kurtz's conclusion is depressing but unsurprising, and it echoes works by a growing number of scholars, such as Kenneth Roberts or Guillermo O'Donnell, who find Latin American democracies in their present form not terribly inclusive and too often plagued by graft and petty autocracy. Kurtz argues that democratization in Mexico and Chile succeeded in part because neoliberal restructuring prevented the rural lower classes from mobilizing around their own interests, thus keeping elites from scuttling the process. The problem with this argument, however, is that one of his empirical premises is wrong, at least in Mexico. In that case, a better question might be why a transition to a competitive multiparty electoral system held up despite the fact that the rural lower classes *did* launch a substantial and widespread protest against neoliberal restructuring.

Kurtz insists on looking for evidence of his central variable—collective rural resistance to economic liberalization—in voting tallies and in panel data on union affiliation, reckoning that if rural lower classes had resisted neoliberal restructuring, they would have done so by voting for left-wing parties in federal elections. This makes little sense given what we know about the historical dynamics of rural collective action. The author duly cites important theorists of peasant rebellion, such as James Scott, Eric Wolf, E. P. Thompson, and Charles Tilly, but he seems not to have heeded the lesson that rural resistance is generally local and most often comes in the form of direct action, such as petitions, blockades, occupations, price riots, charivaris, and popular "liberation" of goods in dispute. As for union activity, affiliation to rural unions often is a weak measure of militance in the countryside because of legal and logistical constraints to collective bargaining in agro-industry. It is, then, rather astonishing that the author would carry out a book-length project examining a sector's capacity for collective action without ever collecting any data on protest events in the countryside. If he had done so, he might have found little unrest in the Chilean case but considerable peasant mobilization in Mexico. Beyond the Chiapas rebellion, which he dismisses unwisely as an anomaly that does not fit his case (it does, in fact), he ignores significant peasant protest in the states of Jalisco, Nayarit, Puebla, Morelos, Guanajuato, Sonora, Zacatecas, and Michoacan in the years 1993 to 2003. Significantly, where neoliberal restructuring affected them in the form of debt, termination of credit lines, and low prices at the marketplaces, peasants merged their grievances first with larger farmers and later with urban debtors. The most significant movement of this type, the Barzón Movement, claimed 500,000 members, placed several of its leaders in federal and state offices, and compelled federal authorities to offer several rounds of debt relief to bankrupt farmers in the countryside.

Problems mount when one examines the data the author presents as evidence. The heart of his case is that despite being steamrollered by Augusto Pinochet's neoliberal Chicago Boys and Carlos Salinas's Harvard-trained technocrats, poor rural voters in Chile mysteriously became a bulwark of support for conservative parties, and that rural voters in Mexico remained a reliable reservoir of votes for the Institutional Revolutionary Party (PRI). These data are intriguing but seemingly incomplete in the Chilean case; the data on Mexico are frankly deceptive. Kurtz includes data from three federal elections in Chile (1989, 1993, 1997) and two federal elections in Mexico (1994 and 1997). However, while the Chilean elections may in fact have been fairly open, the author's contention that votes in the 1994 elections or even the 1997 elections in the countryside were free and transparent is simply not accurate. Rural areas unfortunately lagged behind the cities in the march toward free elections.

According to Alianza Cívica, a well-respected election watchdog organization in Mexico, lower-class voters in the countryside were routinely threatened by PRI *caciques* with the loss of subsidies for planting and household consumption. The Mexican case becomes even more complicated if one examines municipal and state-level elections over time in Mexico, which Kurtz does not do. Here, support for the PRI is not uniform over time, indicating that rural voters did have the capacity to mobilize their discontent at the polls in a fair number of cases. While the PRI clung to power at the federal level throughout much of the 1990s, opposition parties made significant gains in municipal government and state legislatures. Opposition governors were elected in rural states like Morelos, Tlaxcala, Zacatecas, Chihuahua, and eventually Michoacan. Notably, whether or not they were in a position to deliver relief, where opposition party candidates got votes, they did so by promising small farmers they would address mounting producer debts, foreign competition in the marketplace, and low commodity prices. Even more incredibly, these choices were not always without consequences for rural voters. Constituents backing opposition candidates in Guerrero in 1989 and 1992 were attacked by police; more than two dozen were "disappeared" and killed and hundreds were wounded. Two years later in 1994 in the troubled state of Chiapas, municipalities who elected left-opposition Party of the Democratic Revolution candidates almost uniformly found block grants for administration and infrastructure cut off by the PRI-controlled state government. (This situation, in fact, was so grave that the Zapatista Army of National Liberation advised its members to boycott municipal elections in 1997.)

It is too early to tell, but it may be that history will turn Kurtz's argument on its head. Certainly, recent rounds of rural mobilization in Bolivia, Venezuela, Ecuador, Peru, and Brazil suggest that peasants may indeed retain some capacity for mobilization in a neoliberal world. If Kurtz's contention is that successful national transitions to democracy in neoliberal economies owe much to peasant acquiescence, unfolding events in Latin America today suggest that national transitions to democracy may not be terribly stable, that the rural poor are never so atomized and incapable of collective action as federal voting tallies seem to suggest.

Privileging Industry: The Comparative Politics of Trade and Industrial Policy. By Fiona McGillivray. Princeton: Princeton University Press, 2004. 224p. $55.00 cloth, $19.95 paper.

— Jeffrey Cason, *Middlebury College*

Of the various transnational economic linkages, international trade is the most explicitly and persistently political. It is also very closely tied to industrial and investment policies in domestic political economies. In this tightly and clearly argued book, Fiona McGillivray comes to terms with the political dimensions of trade and industrial policy by focusing on how electoral rules, strength of political parties, and industrial geography affect trade policy, and she provides a persuasive argument about the conditions under which politicians act to redistribute income toward particular industrial sectors.

McGillivray's point of departure is that trade policy is inherently redistributive. She also notes that certain types of trade policy might not at first glance appear to be redistributive, which makes them attractive to politicians. Furthermore, she assumes that politicians are most interested in political survival and will act within their particular institutional context to try to assure their continuation in power. In effect, she argues, the likelihood that trade or industrial policy will favor particular industries depends on political institutions and can be predicted on the basis of institutional and geographical variables.

The book is particularly impressive in its theoretical deductions and arguments. While highly disciplined when it comes to deriving hypotheses, McGillivray also has a good sense of the contexts that she is writing about. To begin with the theoretical arguments, she differentiates political systems along two dimensions: the strength of political parties and the electoral system (majoritarian single-member districts vs. proportional representation). She then examines three of the four possible combinations in detail: majoritarian strong-party systems, such as the UK; majoritarian weak-party systems, such as the United States; and proportional representation (PR) strong-party systems, such as Germany. Although it is somewhat difficult to do justice to her sophisticated argument in a review, the basic thrust of her case comes down to this: Industry, geography, and political systems can combine with one another in multiple ways, and most importantly, one can get a very good handle on understanding which industries will be protected or otherwise favored by governments by looking at their interaction. More specifically, in majoritarian single-member district systems with strong parties, the marginal or "swing" districts are likely to be favored with protection of their industries, while in majoritarian single-member district systems with weak parties, the most favored industries are likely to be those that are large and dispersed across many districts, as are those with long-serving representatives (i.e., those in "safe" seats), who are able to rise in seniority and be in a position to protect their constituents. Finally, strong-party PR systems are likely have governing coalitions favoring core supporters with protection or other kinds of benefits.

One will note that there is no significant discussion of the fourth possible combination, weak-party PR systems. McGillivray notes that such systems are atypical, although she does recognize that there is one very important case that fits this bill: Brazil. She includes some discussion of the Brazilian case and considers the possible theoretical implications of it, but in the end concludes that "given the relative ambiguity of prediction and the uniqueness of each weak-party PR case" (p. 41), she will not consider such cases in any detail. This is certainly an understandable conclusion, but it also points to the greater ease, in general, of prediction when it comes to strong-party systems. Even though the author does consider a weak-party majoritarian system (the United States), there is also a greater degree of uncertainty in this sort of case as well, since protection can depend on particular individual representatives in key committee positions who can protect or provide assistance to voters in their districts, and this can certainly be somewhat idiosyncratic. She does try to get a handle on this by testing for the effects of membership on key committees in the U.S. Congress, but this test does not, in the end, give her much explanatory leverage.

McGillivray recognizes some of the limits of her quantitative data. For example, she notes the limitations of using tariffs as an indicator of protection, particularly since governments have come up with numerous ways to protect and aid industries as actual tariff levels have declined in recent decades. To her credit, McGillivray tries to think creatively about this data, and comes up with some alternative measures to get at how politics affects industrial favoritism by developing some new measures on price dispersion in stock markets to get at changes in government industrial policy. Importantly, however, she weaves in qualitative stories to help illustrate and strengthen her arguments. She begins the book by focusing on the cutlery industry in the UK, the United States, and Germany, noting the different levels of protection for these industries, which then launches her theoretical discussion. Her consideration of the steel industry in Chapter 4 is an outstanding and brief summary of the travails of that industry in five industrialized countries, and it dovetails nicely with the theoretical argument. It does so, in particular, because she demonstrates that the pattern of protection and assistance in the steel industry is clearly not determined by the ideological predilections of the parties in power, and that right-of-center governments have done plenty to intervene in the market when it suited their political survival requirements. Much like George W. Bush's protection of the steel industry in 2002, conservative governments elsewhere found it useful to intervene in the marketplace when politically convenient. And McGillivray demonstrates, quite effectively, how the nature of this intervention depends on political institutions and industrial geography.

It is difficult to find much fault with *Privileging Industry,* and any criticisms tend to be of the sort that would ask the author to extend what she has done, since she is clearly on a fruitful track here. For example, she could have gotten greater leverage out of more discussion of a weak-party PR case like Brazil, comparing it to the weak-party majoritarian case of the United States. In addition, the measures of stock price dispersion could be improved and developed further, as McGillivray herself recognizes; the payoff for her methodological exertions are not terribly great at this point. In the end, however, this is a clearly argued book that significantly advances our understanding of the political dynamics behind trade and industrial policy in a comparative context, and that is a significant achievement.

The British Regulatory State: High Modernism and Hyper-Innovation. By Michael Moran. New York: Oxford University Press, 2003. 256p. $65.00.

— Leon D. Epstein, *University of Wisconsin–Madison*

Drawing on experienced scholarly observation, Michael Moran has written a strikingly critical evaluation of Britain's recent development of state regulatory authority. Its now legalistic form more nearly resembles the long-prevalent American pattern. Perhaps the British development would seem to follow from the privatization of industries in the last two decades of the twentieth century. Enterprises like railroads, telecommunications, and electricity are no longer government corporations but privately owned "public utilities," to use the American term for businesses especially affected by the public interest. For Moran, however, other forces also help account for the growth of regulation in Britain. He persuasively argues that legal regulatory authority has also been extended to several enterprises not previously nationalized and then privatized.

Mainly, the author believes that large-scale regulatory development came to Britain because of the obsolete nature of a long-standing clublike relationship between government and business enterprise, be it private or public. That relationship had survived from Victorian times despite the democratization of British politics in the half century after World War I. Not until the 1970s was the regulatory style of gentlemen's clubs effectively challenged. Thus, Moran describes a shift from the first two-thirds of the century, when British governing arrangements were "among the most stable and least innovative in the advanced capitalist world," to the last few decades of "turmoil" and leadership "in institutional and policy change" (p. 1). His context is impressively comparative, notably with respect to the United States, and he makes good use of a considerable body of relevant American scholarly literature. Moreover, he is well aware of the impact of globalization and Europeanization.

The United States, in Moran's words, invented the "characteristic institution of the regulatory state: the specialized agency designed to manage public control as an alternative to public ownership" (p. 14). Its development he attributes to the Progressive Era, the New Deal, and the social regulation of the 1960s. Its extension to Britain and other nations, the author suggests, owes something to American influence, although he does not blame the United States for the ways in which Britain adopted American-type regulations.

Nor does Moran portray Britain's pre-1970 arrangements as any kind of golden age. He does not profess an admiration for nationalized industries, and he does not champion the old order of club regulation. He treats such regulation as the protection of elites from democratic threats, and scorns its reliance on a gentlemanly trust between regulator and regulated in industrial, professional, and, most emphatically, financial affairs of the City. By the 1970s, at any rate, it appeared obsolete.

To show that the post-1970 transformation occurred in sectors besides those of the newly privatized industries, Moran describes what happened in banking and finance, in the professions of accounting, law, and medicine, and in sporting activity. Yet the major thrust of his analysis is devoted to the regulation of newly privatized enterprises. The privatization itself he treats as a constitutional revolution, overturning more than a century of political commitments to public ownership and transforming the British system into a laboratory of regulatory hyperinnovation. Creating a new network of regulators followed from a recognition that traditional company law was insufficient for the regulation of business enterprises of the kind called utilities. As Moran says, the most important privatized enterprises "required their own special governing systems" (p. 112).

That these systems failed is a central claim of *The British Regulatory State*. So, as might be expected, the prime exhibit is the collapse of railway regulation in the Railtrack crisis of 2001–2 that put into receivership the private corporation charged with maintaining railways and led to political intervention. Somewhat similar politicization, Moran finds, is characteristic of Britain's regulatory hyperinnovation in areas previously insulated in one manner or another from state control. The consequences, he argues, are chaos and fiasco. Of the latter, he offers five cases, in addition to rail privatization, to illustrate the inability of the state adequately to discharge its "high modernist" ambitions. At least two of these fiascos, concerning the Millennium Dome and the Community Charge (poll tax), do not seem to be administrative regulatory cases in the usual sense of the term, but instances of failed governmental policies of another kind. It is not clear whether Moran regards governmental failures, be they regulatory or general, as more frequent and serious in Britain than elsewhere.

At any rate, in concluding with doubts that the British government has the capacity to effectively realize its regulatory ambitions, Moran strikes a profoundly pessimistic note. Not only does he not yearn for the old club regulation or for nationalization, but he does not propose to emulate the American model, which as we well know has had its own difficulties, especially with respect to the colonization of agencies by the interests they are supposed to regulate. And if Britain cannot cope with the regulatory problem, what nation would be better able to do so? Probably, it is too much to expect an author to develop an attractive alternative regime in a book of merely 183 pages of text. The book's brevity, though it might cause a prospective buyer to question its price, does not diminish the significance of the author's richly suggestive, interesting, and cogent thesis.

Electoral Engineering: Voting Rules and Political Behavior. By Pippa Norris. Cambridge: Cambridge University Press, 2004. 390p. $70.00 cloth, $25.99 paper.

— Erik S. Herron, *University of Kansas*

Over 20 years ago, William Riker proposed that the study of electoral systems and their consequences constituted the best-developed research program in contemporary political science. Scores of scholars followed Maurice Duverger's lead, emphasizing how institutions affect the strategic environment facing political actors and the contours of party systems. Other researchers followed Seymour Martin Lipset's and Stein Rokkan's lead, privileging social cleavages as the main features influencing party system development. Recently, scholars have attempted to reconcile or integrate the competing approaches. Pippa Norris's new book fits into this trend by evaluating the two schools of thought side by side, harnessing valuable data produced by the Comparative Study of Electoral Systems (CSES).

Norris, a prolific researcher, covers a substantial amount of terrain in *Electoral Engineering*. She classifies electoral rules and addresses voting behavior and representation. She integrates these topics into a coherent volume by retaining her main theme: assessing the relative strengths of rational choice and cultural modernization theories in explaining how elections influence political competition. She ultimately concludes that institutional features consistently affect voting behavior and representation. But cultural factors play an important role in some circumstances.

In the first part of the book, Norris establishes her research questions, specifies the hypotheses, and presents a taxonomy of election rules. Chapter 1 succinctly outlines how rational choice institutionalism and cultural modernization explain the relationship between rules and outcomes, and sets forth a series of hypotheses that are featured in her later empirical tests. She also outlines the strengths and weaknesses of CSES data. In Chapters 2 and 3, the author turns to electoral systems. She divides the globe into four main groups: countries employing majoritarian, proportional, or mixed rules ("combined," in her terminology), and those with no elections. In her classification, majoritarian systems include plurality and majority rules, alternative vote, bloc vote, cumulative vote, and the single nontransferable vote. Proportional rules encompass varied forms of party-list systems and the single transferable vote. Combined systems are subdivided into independent and dependent systems, depending on the linkage between allocation tiers. In the next chapter, Norris sets forth the criteria by which election systems may be judged, based on notions of adversarial and consensual democracy.

After establishing the definitions of election rules, the second section explicitly evaluates how rules influence voting behavior. Chapter 4 addresses the relationship between election rules and the size and shape of party systems. Norris finds overall support for Duverger's propositions; majoritarian rules tend to exert a reductive effect on the number of parties in competition. Chapter 5 turns to the relationship between rules and cleavages. She finds that election rules affect the strategies parties pursue; societies with majoritarian rules tend to exhibit weaker cleavage structures. Further, cleavages are stronger in the most modernized societies, contradicting expectations from the cultural modernization approach. This finding is repeated in Chapter 6. Party identification is weaker in majoritarian systems than in combined and proportional systems. Its effect is not significantly different between industrial and postindustrial polities, but postindustrial societies counterintuitively produce slightly more partisanship than industrial societies. In Chapter 7, Norris finds that election rules influence turnout, with voters coming to the polls more readily when proportional rules are employed. But factors important in cultural modernization theory, such as social features and cultural attitudes, also influence the likelihood of voting.

Norris evaluates how election rules influence representation in the third part of the book. In Chapter 8, she investigates women's access to political office. Proportional rules tend to benefit women, and gender quotas create conditions for seat acquisition. These institutional rules mitigate cultural features, represented by religion in the analysis. Ethnic minority representation is the focus of Chapter 9. The findings in this chapter are less conclusive, and the author speculates that the model's parsimony may contribute to her failure to find clear connections between institutional rules and outcomes. Chapter 10 concludes the analytical chapters by assessing accountability through modes of constituency service. As expected, majoritarian systems provide incentives for politicians to engage in personal, rather than party-based, appeals to voters.

The book is strongest when Norris employs data from the CSES, evaluating her research questions by using public opinion surveys from 32 countries. Indeed, CSES data provide new insights into variation in citizen attitudes across established and new democracies. But the book also suffers from some shortcomings. Combined systems are defined as one of the main families of electoral rules. Yet the hypotheses set forth clearly defined expectations only for majoritarian and proportional systems. The reader is left with no way to disentangle the effects of combined systems, except to assume that they are an intermediate form that should fall somewhere in the middle. The classification of electoral systems is based on a comprehensive survey published in 1997. Because election rules are a moving target, updating the classification to account for changes would have been a valuable contribution. Further, Belarus's 2000 presidential election is included in the analysis, although it is probably not equivalent to the other cases under investigation. The manuscript also features minor inaccuracies scattered throughout.

All in all, *Electoral Engineering* is a fine introduction to the main debates in the literature on comparative electoral systems and their effects. The book's strengths lie in its scope and ambition. Norris's exploration of CSES data provides scholars with seeds for future research projects. More advanced readers may leave wanting more, however. Norris warns readers of this possibility in the introduction, recounting how a colleague responded to her plans for a comprehensive book about elections. He simply noted: "It is complicated" (p. ix).

The Economic Effects of Constitutions. By Torsten Persson and Guido Tabellini. Cambridge, MA: The MIT Press, 2003. 320p. $35.00.

— John M. Carey, *Dartmouth College*

Torsten Persson and Guido Tabellini have produced the most ambitious study yet that attempts to identify and estimate the effects of constitutional design on economic outcomes. They draw on data from more than 80 countries and control for demographic, historical, regional, and economic characteristics. The purpose is to determine whether the shape of political institutions has measurable impact on economic policies (e.g., fiscal balance, social welfare spending), on government performance (e.g., corruption indices, protection of property rights), and on direct measures of economic performance (e.g., productivity of capital and labor). The political institutions that draw the most attention are constitutional regime type and the method of electing legislators.

The authors are sensitive to subtle distinctions among constitutions and electoral rules, yet they follow previous political science research in focusing much of their analysis on the basic differences between presidential and parliamentary systems, and between majoritarian and proportional electoral rules. From these distinctions, they draw a number of intuitions on which they base their statistical analysis. One is that majority elections turn more dramatically than do proportional ones on swings in support among small groups of voters—those in pivotal districts, for example. This could produce more acute responsiveness by elected officials in majority-election systems, and so less use of their office for private gain. It could also encourage politicians to produce public policies that yield more targeted, as opposed to universalistic, benefits. Another is that presidentialism might foster accountability by maximizing internal checks and balances that prevent rent-seeking by politicians, whereas parliamentarism may maximize responsiveness to the broadest set of citizen demands by requiring governments to hold together broad, and potentially heterogeneous, support coalitions in order to survive in office.

The empirical scope of the authors' analysis demands a formidable effort at data collection, as well as a host of decisions regarding measurement and coding. Space limitations preclude discussion of these here, except to say that Persson and Tabellini are consistently clear and explicit about their decisions, providing readers with the necessary information on which to evaluate their results. At any rate, issues of inference are even more challenging than those of data in this project, and here the authors provide a real service in Chapter 5, which is a primer on statistical and econometric methods to address fundamental problems that plague cross-national comparisons in general—reverse causation, selection bias, unobserved variables, interactive effects, and potential differences across observations in functional forms of relationships among the variables (what they call "comparing the uncomparable").

Persson and Tabellini absolve from an obligation to read the chapter any readers willing to take on faith their methodological skills and their judgment, but for readers (like this one) who are anything short of econometrically proficient, the chapter is a worthwhile tutorial. The advantage over most pure methods texts is in already being immersed in the empirical data and the hypotheses regarding relationships among the variables at stake for the authors. With the thick theoretical and empirical context of the book at the front of one's mind, the issues of inference and methods are much easier to grasp, and the methods lessons in this chapter will serve any consumer or producer of cross-national comparisons well beyond this book.

The empirical results reported in the book are substantial, but a few key insights warrant particular mention. One is that with respect to accountability, the effect of presidentialism appears to interact with the quality of democracy, such that when presidentialism is good, it can be very good, although (unlike Mae West) when it is bad, it is *not* even better—in fact, far from it. That is, among regimes with high Freedom House scores, presidentialism reduces corruption and rent-seeking policies, whereas among lower-quality democracies, presidentialism contributes to policies that undermine property rights and reduce the productivity of labor. This prompts the natural question: Does presidentialism itself affect the quality of democracy? Empirically, Persson and Tabellini's presidential systems are lower-quality democracies than the parliamentary systems in their analysis, but fully untangling the relative import of various other factors (wealth, colonial history, regime age) besides regime type that may contribute to democratic quality is difficult.

Another critical result is the ratchet effect the authors detect with respect to spending policy under one particular institutional format, but not others. Specifically, parliamentary regimes with proportional electoral rules respond most dramatically to negative economic shocks by increasing government spending, but then do not subsequently scale back when times get better. These regimes are good Keynesians in bad times, but not so good ones in good times. Persson and Tabellini speculate that this is because the coalitions that generally govern in parliamentary and proportional systems have trouble agreeing on a distribution of the fiscal pain that budget cuts would imply.

Presidential regimes, by contrast, are lousy Keynesians, period, actually exhibiting mildly *pro*-cyclical spending patterns. This could be because presidential regimes tend to be found in poorer countries, which face more severe restrictions on financing government debt, and so for whom countercyclical spending is less available as a policy option to begin with. Moreover, whereas parliamentary/proportional regimes tend to have difficulty cutting spending under any circumstances, presidential systems postpone spending cuts until the year after elections, suggesting a brand of manipulation of the political business cycle. Governments in proportional electoral systems, parliamentary and presidential alike, by contrast, raise spending in election years.

Readers interested in institutional design should take these reported results as the tip of the iceberg and read the whole book. *The Economic Effects of Constitutions* advances our understanding of political economy and is certain to become a standard reference in the comparative study of institutions.

Transforming Mozambique: The Politics of Privatization, 1975–2000. By M. Anne Pitcher. New York: Cambridge University Press, 2002. 320p. $60.00.

— Deborah A. Bräutigam, *American University*

Mozambique provides a sharp contrast with transition in much of Eastern Europe, the former Soviet Union, and even Asia. A Marxist movement (Frelimo) took power in 1975, pledging to construct a socialist state. Only 15 years later, a new constitution made no reference to socialism. By 1994, Mozambique was a multiparty democracy and one of the World Bank's "model" market reformers. Yet Frelimo remains in power, presiding over the private economy it once denounced. Why and how did this happen?

There has been no shortage of analysis of Mozambique's complex modern history, but this book stands out for its balance, its focus, and its careful scholarship. M. Anne Pitcher makes two central claims. First, she argues that Frelimo "owned" the transition process. This was not something forced on them by outside powers (the World Bank, for example), nor was Mozambique part of a falling series of dominoes linked to the Soviet Union. What she calls "transformative preservation" helps explain why there remained so much continuity between the socialist regime and the government hailed as a "model" reformer. Second, she argues that the authoritarian, state-directed transition was in fact shaped by continuous bargaining with a dense network of social actors.

In providing the evidence for her case, Pitcher begins her first chapter decades before independence, showing how the Portuguese created a surprisingly developmentalist state. Frelimo thus continued and deepened an already aggressive pattern of state intervention. The author's archival research allows her to paint a detailed portrait of the foreign and local businesses that grew powerful under the colonial state. Although Frelimo later nationalized much of the economy, many of these businesses—some "huge agricultural companies" (p. 63) among them—remained players in the shrunken private sector.

The second chapter covers the early years of Frelimo, making the case that the interventionist state was clearly motivated by socialist ideas, but also by African nationalism, and by a vision of modernity that was not far different from that held by the Portuguese before them. These two competing visions weakened the effort to impose socialism in Mozambique. Additionally, Pitcher argues that every transformative measure put in place by Frelimo met with robust contestation. The inability of the socialist state to transform the economy and the society opened it to bargains with the social actors it relied on for production. These bargains shaped the two periods of transition analyzed in the third chapter: the erosion of the state from below during the intensified civil war (1983 to 1990), and the transition to the market after 1990. Although she acknowledges the role of the war and external pressures, Pitcher shows how the transition was a carefully orchestrated effort by the Frelimo regime to preserve at least some of its authority and power. The outlines of this strategy parallel the gradual turn outward in China, at much the same time (and with no war) but with important differences. For example, in Mozambique, companies such as the British multinational Lonrho gained use rights to large tracts of land (the state retained formal ownership).

The fourth chapter focuses more narrowly on the construction of capitalism in postsocialist Mozambique, while the fifth and sixth chapters contain case studies of manufacturing and agriculture. Here, Pitcher briefly engages some standard Africanist debates in new ways, for example: Is a true domestic capitalist class rising in Mozambique, or simply a dependent "comprador" class? The author's detailed accounting of the new/old "cleavages and commonalities" transform these debates (p. 167). Gender, race, ethnicity, and region are sources of conflict in the new Mozambique and challenges to the formation of anything resembling a unified elite. These chapters also carefully document the overlap between state and business: joint ventures, minority shares and interlocking directorships. They trace the multiple roles taken by government officials, at once parliamentarians and heads of business associations; party members and bank directors. Do state–business relationships represent the kind of "embedded autonomy" Peter Evans praised in East Asia? No, Pitcher responds. These partnerships "compromise" autonomy and actually make it harder for the state to be flexible and innovative, or to demand performance (p. 178). The tight focus on business and the state provides considerable leverage, but this is a book about privatization, and one wonders: What happened to organized labor? Unions make brief appearances now and again, perhaps most notably in the sad case of the collapse of the cashew-processing industry, but they are for the most part simply absent from the story.

Pitcher follows in an honorable tradition of comparativists working in Africa. *Transforming Mozambique* reflects long periods of fieldwork, archival work, extensive interviews, and use of primary documents. There is very much to admire in its complex and rich portrayal of socialist transition. A fine book like this could do even more, however, to bring this case out of the shadows and into the mainstream of comparative theories. Because political scientists working outside of the Africa region rarely include African material and experience in the major debates of comparative politics, it is all the more important for those working on African cases to bring theories to the cases and bring the cases back to the theories. To be sure, Pitcher does use a framework adapted from Peter Evans's *Embedded Autonomy* (1995) in her analysis, but there is little engagement with the more specific literature on transitions. For example, the collapse of the cashew industry can also be analyzed as an almost inevitable result of the kind of partial liberalization described by Joel Hellman ("Winners Take All: The Politics of Partial Reform in Postcommunist Transitions," *World Politics* 50 [1998]: 203–35). One also wishes that Pitcher had put into comparative perspective her conclusion that the Mozambican project "has a great possibility of collapsing at any time" (p. 146) and that the "troubled alliance between state and capital" is "endangering both political stability and economic development" (p. 178). These are important findings, but is this unique to Mozambique, or common elsewhere in socialist transitions? A comparative chapter at the conclusion of this work would have strengthened its claims and made it easier to integrate its many insights back into "the literature." Still, even if those working on issues of transition have to make the comparisons themselves, they will find this excellent book more than ample reward for their efforts.

Learning from Foreign Models in Latin American Policy Reform. Edited by Kurt Weyland. Baltimore: Johns Hopkins University Press, 2004. 320p. $60.00 cloth, $22.95 paper.

— Philip Mauceri, *University of Northern Iowa*

The purpose of this important volume, whose contributors include both policy practitioners and academics, is to "elucidate the cognitive and political processes that shape the diffusion of models in contemporary Latin America" (p. 2). In his introductory chapter, Kurt Weyland lays out a series of questions to help frame an assessment of the impact of foreign models on Latin American social policy reforms during the last two decades. When do policymakers turn to external models? What turns a country's policy practices into a model for others? How is information about a particular model spread abroad? What is the impact of transmitted models on policy decision makers and how, if at all, are they adapted to local conditions? Weyland argues that the concept of "learning" is better suited to addressing these questions than either the traditional rational choice or structuralist approaches alone, since learning creates "important filters between objective reality and actors' attitudes and actions" (p. 6). Various heuristic shortcuts are taken by policymakers that significantly alter the expected cost–benefit calculation that, it is often assumed, decides the diffusion of foreign models. This framework is applied fairly consistently in a series of case study chapters that focus on three social sector reforms carried out in the region: pension systems (Argentina, Brazil), unemployment insurance (Brazil, Chile) and health care (Colombia, Mexico).

Probably no foreign model has been more discussed for its impact than Chile's privatization of its pension system. No fewer than eight countries in the region adopted some variation of this reform. Still, the lessons to be drawn from this process, as Joan Nelson warns in her chapter, are limited, since its perceived advantages have more to do with ancillary benefits, such as strengthening internal capital markets, than with developing a more effective pension system. In fact, much of the promise for pensioners of this privatization has gone unrealized. Where ancillary benefits are more limited, as in health care or education, policymakers are less likely to look abroad for a model. In the spread of reforms such as Chile's pension reform, international financial institutions (IFIs) would appear to be a key player, yet what most chapters in the volume suggest is that the role of IFIs has been exaggerated. While most authors note that IFIs played an important role in putting reforms on the policy agenda, presenting possible reform options and sharing the experiences of other countries, they largely conclude that it would be simplistic to ascribe such reforms to IFI pressure. In many of the case studies, particularly of health care in Colombia and Mexico, it is clear that pressure from organized groups and the patronage interests of political parties carried far more weight in both the domestic policy process and the ultimate outcome of reforms. While the argument that reforms are not imposed by IFIs is convincing, if somewhat obvious, this does not mean we should underestimate the power of IFIs in the diffusion of foreign models. By restricting the agenda of reforms to those that conform to a neoliberal economic model, IFIs have already exercised an enormous amount of power, and it would have been helpful if authors had evaluated the power of IFIs in limiting the discourse over reform more directly.

An important conclusion, especially for those interested in economic development, is that the models that are adopted respond more to functional advantages than to an effort to appear "modern" or to somehow play catch-up with neighbors. It is interesting to note that the volume suggests that governments are more likely to adopt models from similar or nearby countries than to merely imitate policies adopted in advanced industrial nations. The predisposition to learn from regional or similar countries, or what Weyland terms the "availability heuristic," can be more broadly applied to a host of development issues, and it suggests that both policymakers and academics need to focus greater attention on growing regional dynamics when assessing policy reforms. As Latin American integration has accelerated dramatically in the last two decades, there has been a concomitant growth in the regional socialization of business, academic, governmental, and nongovernmental groups. The regional focus of foreign model adoption is therefore not completely surprising. Nonetheless, the experience of many successful late modernizers, particularly in East Asia, is that they have often turned to countries outside their region for foreign models, raising the question of whether Latin America's regional focus will be sufficient to accelerate modernization and solve the region's persistent poverty and equality problems.

If there is a limitation to *Learning from Foreign Models in Latin American Policy Reform,* it is a general lack of historical or comparative perspective. Policy diffusion is by no means a novel phenomenon, as several authors briefly mention. From Bismarck's labor reforms to the agrarian reforms of Latin America in the 1960s, the diffusion of foreign models has a notable track record. It is not clear from this volume, however, whether the learning processes behind the cases examined here can be applied to foreign-inspired reforms in the past or whether there are distinct characteristics to the current period that result from globalization, such as improved communication or financial integration, that make the learning dynamic somehow different. An apparent difference between the current foreign models adopted in the region and those of the past, such as agrarian reform, is the neoliberal underpinning to many of the current models, as well as the supporting role of IFIs in their adoption. It is unclear if the learning process would have worked similarly for models that lacked these two key factors. This is also where a comparative perspective, looking at the same dynamic in other regions of the world, could have been useful as well. Overall however, this book offers a significant contribution to the field by integrating learning into the dynamic of policy choice, and will be invaluable for both policymakers and academics interested in trying to understand the ways by which foreign models of policy reform are adopted and applied.

INTERNATIONAL RELATIONS

From International to World Society? English School Theory and the Social Structure of Globalisation.
By Barry Buzan. New York: Cambridge University Press, 2004. 318p. $70.00 cloth, $25.99 paper.

— Andrew Linklater, *University of Wales, Aberystwyth*

The English School of international relations has been principally concerned with understanding the society of states, but some of its members have commented on its relationship with world society. Hedley Bull and Martin Wight noted that the architects of the modern society of states derived some of their political vocabulary from medieval conceptions of world society. Bull maintained that natural law images of a world society survived in the modern doctrine of universal human rights. Whether a world society was likely to develop in the first universal society of states was a question of great importance to Bull and John Vincent. The former referred to an emerging "cosmopolitan culture of modernity" that could underpin order between radically different societies in "the post-European age"; the latter believed a global culture that established the right of every human being to be free from starvation could perform that function. However, most members of the English School have denied that the modern society of states is likely to be dissolved in a world society. Indeed, Wight argued that from the Reformation, rival conceptions of world society have often had a divisive effect on international relations. Barry Buzan's most recent book is the first work by a member of the school to consider world society since Vincent's call for the more systematic investigation of the concept almost 15 years ago.

Buzan notes that the English School lost the initiative in analyzing world society to members of the Stanford School such as John Meyer and to such systems theorists as Niklas Luhmann. Its failure to theorize world society and its relationship with international society is the "biggest weakness in existing English School theory," one that inhibits its "potential to improve how globalisation can be conceptualised" (p. 2). Buzan criticizes English School theorists, such as Hedley Bull, James Mayall, and Robert Jackson, for their "unwarranted pessimism" regarding the possible "transformation" of world politics, especially within regional subsystems (p. 212ff). How to understand the relationship "between the state and non-state worlds" is the "big political event" in an era moving beyond "a pure Westphalian mode of international relations, in which the key tension is among rival states" (p. 88). In the end, Buzan comes round to the classical English School view of the primacy of states. Given their "dominant command of the instruments of coercion," they "will generally be the dominant actors . . . for some decades to come"; they will shape the lives of transnational actors and individuals to a greater extent than they will shape states (p. 259). Like Bull, Buzan believes that world society is ultimately dependent on order within the international society of states.

From International to World Society? is strong on conceptual analysis. There is a useful discussion of Bull's claim that world society involves "a degree of interaction linking all parts of the human community to one another" and "a sense of common interests and common values on the basis of which common rules and institutions may be built" (p. 37). Buzan argues that Bull did not disentangle these two elements prior to analyzing them carefully. Searching questions are asked about how to move the discussion forward by incorporating insights from constructivist analysis. However, the volume does more to reflect on future intellectual agendas than to break new theoretical ground.

The failure of Bull, Wight, and others to deal with the economic dimensions of world politics is noted (p. 20), but there is surprisingly little discussion—given the concern with globalization—of what English School theory can borrow from international political economy. The significance of capitalism for world society is recognized (p. 83), but the volume repeats the English School's famous neglect of Marx—arguably the first theorist of world society—and the Marxist tradition with its focus on inequalities of material resources and political power. Radical scholarship on, for example, the phenomena of "transnational class formation" and "disciplinary liberalism" deserved consideration.

Buzan rejects approaches to world society such as Vincent's, which allowed "their normative concerns with human rights to distort their theoretical reflections" (p. 11). Rightly he criticizes Vincent's claim that world society includes all groups whose claims are not heard by international society and who are, in consequence, "hostile to it" (p. 41). Such approaches fail to address the "social structural strand" of world society (p. 14). The question is whether highlighting the social structural sphere diverts attention from the dimension of world society captured by Bull's remarks about the "common interests and common values" on which "common rules and institutions might be built." The universal human rights culture and global environment issues do not go unnoticed in this work, but more could have been said about their significance for world society. Buzan's approach largely neglects the literature on media representations of "distant suffering," which has triggered discussions about the rights and wrongs of humanitarian intervention, responsibilities to civilian populations in war, and the future of international criminal law. Such matters are especially germane to his discussion of how a world society is more than a system of interaction but less than a community of "shared identity and we-feeling" (p. 121).

A lengthy conclusion offers "a portrait of contemporary interstate society" that is exceedingly well done and valuable for students, but there is little explicit discussion of world society or the "social structure of globalisation" in the last quarter of the book. In general, the analysis of the English School in this volume is too complex for the beginner. Whether there is enough in the way of a novel development of English School theory for the specialist remains to be seen. The volume does not succeed in providing a clear and systematic argument about how English School theory can improve the conceptualization of globalization by developing a more sophisticated account of world society.

Regions and Powers: The Structure of International Security. By Barry Buzan and Ole Wæver. New York: Cambridge University Press, 2004. 596p. $90.00 cloth, $32.99 paper.

— Douglas Lemke, *Pennsylvania State University*

This book is based on the assumptions that the regional level of security has always been important, has grown in importance over the past seven or so decades, and has emerged as especially prominent with the end of the Cold War. Unfortunately, neorealists are unable to recognize the increasing importance of regional interactions because they focus exclusively on the global level. Equally regrettable is the fact that newer globalization perspectives are unable to capture regional security relations adequately because they, too, severely downgrade the importance of states and/or assume that reactions to forces like globalization do not meaningfully differ across regions. Consequently, these prominent theoretical schools are increasingly unsuccessful at making sense of reality.

To replace them, Barry Buzan and Ole Wæver offer "regional security complex theory" (RSCT), building on materialist concerns akin to those in neorealism as well as constructivist securitization theory (as advanced by Buzan, Wæver, and Jaap de Wilde, *Security: A New Framework for Analysis,* 1998). RSCT distinguishes between global powers able to exert influence across the entire world and regional powers able to exert influence only close to home. Since the projection of power for influence is costly (a materialist influence on their theory), most states' security relations are clustered within their region. RSCT thus focuses attention on security interdependencies within regionally based clusters: regional security complexes. Buzan and Wæver's RSCT offers a conceptual framework to describe each regional security complex according to its type, and thus facilitates regional comparisons while still allowing for global power influence.

Buzan and Wæver define regional security complexes on the basis of their judgment regarding the existence of durable patterns of amity and enmity among proximate states. To the extent that states securitize their relations with neighbors (due to traditional balance-of-power concerns about anarchy, or over differences of opinion about other issues), they comprise a regional security complex. The boundaries of regional security complexes are determined by geographical obstacles to interaction, or by "insulator" states that have little or no security interaction with others. Regional security complexes are classified according to their types. "Standard" RSCs have two or more local powers with a predominantly military-political security agenda. Standard RSCs can be further defined as conflict formations (rivalries and balance-of-power politics predominate), security regimes (alliances mitigate much of the conflict that otherwise would arise), or security communities (à la Karl Deutsch). "Centered" RSCs differ from standard ones in that anarchy no longer prevails. They can be centered by the presence of a superpower or great power that, by consent or coercion, dominates regional relations. They can also be centered by an agreement wherein regional institutions provide an organizing scheme for local relations. Yet more classifications are introduced in that RSCs can combine into larger "super complexes," or can comprise "pre-complexes" and "proto-complexes" (RSCs in the making). Finally, historically there have also been "unstructured areas," regions in which political actors are so primitive that there is insufficient interaction to speak meaningfully of security complexes at all. All states belong to no more than one regional security complex; there can be no overlap.

The bulk of the book is 10 chapters applying RSCT's classificatory scheme to all the different parts of the world. For each region, Buzan and Wæver describe why they believe a given set of states qualifies as a regional security complex, and then discuss that RSC in terms of its type and characteristics. They describe the patterns of hostility or friendship, the distribution of power, efforts by global powers to influence the region, and prospects for regional change. Not all of these applications are equally interesting or convincing, but this reviewer found their discussions of Africa, the Americas, and the post-Soviet space especially valuable. Perhaps this is because these regions are the most challenging for scholars who would analyze all the globe's regions within one theoretical scheme. An enormous amount of research, very well documented in the references, has gone into these 10 chapters, and they repay close reading even given their substantial length. The final two chapters of the book offer comparisons across regions and suggest avenues for further elaboration of RSCT.

As a quantitative researcher who has published a volume offering an alternative analysis of international security from a regional perspective (Douglas Lemke, *Regions of War and Peace,* 2002), I might be forgiven for complaining that *Regions and Powers* pays insufficient attention to establishing rigorous criteria by which the classification of states within specific regional security complexes could be replicated by others. In terms of an operational definition of RSCs, Buzan and Wæver claim: "In order to qualify as an RSC, a group of states or other entities must possess a degree of security interdependence sufficient both to establish them as a linked set and to differentiate them from surrounding security regions" (pp. 47–48). What might qualify as a sufficient degree of security interdependence or differentiation from other regions is never specified. Readers are forced to rely on their judgment. And yet, in spite of epistemological differences, the authors' regional security complexes are consistently similar to "regions" as defined using the (perhaps overly) precise coding rules that scholars of my ilk prefer. More significantly, their findings about the importance of regional rather than global matters in the security calculations of most states, and their claims that security dynamics vary profoundly across regions, are also very similar to those reached by more quantitatively oriented and mathematically inclined investigations. I was repeatedly struck by how complementary our analyses and conclusions are. If triangulation of research findings across different approaches is taken as especially strong evidence of the value of theoretical claims, then this convergence across studies suggests that efforts such as RSCT are very useful avenues for research on international security affairs. And so I strongly recommend *Regions and Powers.*

The Future of Money. By Benjamin J. Cohen. Princeton: Princeton University Press, 2003. 312p. $29.95.

— Jonathan Kirshner, *Cornell University*

Benjamin Cohen has produced yet another excellent volume on the political economy of international monetary affairs, this time with a worthy successor to his previous opus, *The Geography of Money* (1998). Like most of Cohen's scholarship, *The Future of Money* displays the author's remarkable facility with the vast breadth of issues, theories, puzzles, and esoterica associated with monetary matters, as well as a confident command of monetary policy and history. *Future* is perhaps less ambitious than *Geography,* but it is tight, well argued, and not wanting for important claims.

Of the numerous arguments offered in the book, four in particular stand out. First, Cohen attempts effectively, but perhaps with a bit more zeal than is necessary, to disprove the "contraction contention" that "the future [will] see a dramatic reduction in the number of currencies in circulation" (p. xii). Second, he argues neither the euro nor the yen will, in the foreseeable future, challenge the dollar's primacy as an international currency. On the other hand, new currencies—issued by private actors and facilitated by the Internet—will present novel challenges to monetary management. Finally, under pressure from both the proliferation of new monies and powerful market forces, states will need new approaches and techniques to assure the much-needed government of money.

The book's eight chapters are implicitly divided into three parts. The first two set the table, first with an overview of the relevant arguments from *Geography*—and, in particular, the observation that "money's changing geography is diminishing the power of the state" (p. 33)—and then with a consideration of the four strategies that states can employ in response to this challenge: leadership (obviously available only to a few), preservation (efforts to defend monetary autonomy), followership (greater insulation via selective subordination), and alliance (greater autonomy by pooling influence). The next four chapters consider each of these possibilities in turn. Two final chapters are prospective, considering "new frontiers" and offering recommendations on "governing the new geography."

Along the way, Cohen participates in a number of related debates, many of which support his case against the contraction contention, an argument summarized in Chapter 2. One controversy that is engaged quite cautiously is over the prospects for capital controls. *Future* presents the case for both sides of the argument, and in contrast to his relative silence on this issue in *Geography,* the author is ultimately receptive to the place for limited, responsibly managed controls—but (to this reader, at least) he pulls his punches a bit, perhaps with a judicious eye toward picking those fights where more is at stake for the volume's central arguments.

In other arenas, more characteristically, no holds are barred: Marshaling both logic and evidence, Cohen argues that a marked increase in dollarization is unlikely as it "will lack natural appeal to most governments" (p. 140); nor will currency boards proliferate, and "experience to date has not been kind" to their reputation (p. 146). In Chapter 6, "Hanging Together," he methodically surveys the globe and concludes that "predictions of many full new monetary unions . . . appear premature at best" (p. 178). (One weakness in this fine chapter is that the analytical bar had already been set quite high on these issues; see Benjamin Cohen, "Beyond EMU: The Problem of Sustainability," *Economics and Politics* 5 [July 1993]: 187–203.)

Finally, given the endurance of national monies and the new challenges they face, *Future* considers how states can be better equipped to manage their "unavoidable" roles in the governance of money. After discarding a number of options (such as a world central bank), two logical and constructive suggestions are offered as both a counsel for countries and a mediator of coordination between them: the resurrection (and depolitization) of fiscal policy (where governments still enjoy considerable discretion), and a greater role for the International Monetary Fund, though more with regard to its influence than its resources.

Obviously, a book that throws down this many gauntlets will naturally find some of them picked up. For example, in evaluating the prospects for challengers to the dollar, Cohen's argument is to a large extent incrementalist—that is, projecting current trends and prospects, the dollar is likely to retain a dominant position that modestly erodes over time. More likely, however, is that monetary realignment, if it occurs, will take place more suddenly in the wake of a major event, such as a financial crisis involving the dollar. Additionally, sharper competition among the dollar, the euro, and even the yen is likely to be more of a function of the nature of international politics than a play of market forces. If a greater political divide opens up between the United States and Europe, a more overt challenge to the dollar will likely follow.

More generally, many of the critiques of Cohen's vision will follow this political thread. For while *Future* is often and smartly sensitive to the crucial role of politics, it nevertheless averts its eyes from the more rough and tumble rings of the political circus—harder-edged conflicts that feature the oppositional clash of vested interests at both the domestic and international level. This is most evident with regard to the book's proposed reforms, each of which requires the possibility that domestic and international political conflicts can be bracketed off and managed by an apolitical institution. But to search for the end of politics is to chase the horizon: Even the choices of "apolitical" central banks inevitably privilege some interests at the expense of others—this will be more starkly obvious if applied to the naked pie splitting that is fiscal policy—nor do states seem to be up to the task of calibrating their fiscal policies to fine-tune economic management.

But these are the arguments that an important book stimulates. This one would have demanded reading simply on the basis of the author's reputation. Happily, it more than merits this attention, and will be used with great profit by specialists and generalists and in the classroom.

Intervention: Shaping the Global Order. By Karen A. Feste. Westport, CT: Praeger Publishers, 2003. 304p. $64.95.

Humanitarian Intervention: Ethical, Legal, and Political Dilemmas. Edited by J. L. Holzgrefe and Robert O. Keohane. New York: Cambridge University Press, 2003. 362p. $75.00 cloth, $26.99 paper.

— Jennifer M. Welsh, *University of Oxford*

To intervene or not to intervene? This question has dominated global politics over the past five years, in regions as diverse as the former Yugoslavia, Afghanistan, Iraq, and the Sudan. Yet the question is as old as international relations and is a logical extension of the institution of sovereignty. Indeed, it was with the rise of sovereign states that the notion of an exclusive jurisdiction, free from the meddling of outsiders, came to have normative value. Since that time, as Karen Feste notes, patterns of intervention have shaped the international system, "defining contours of connection between its units, guiding interaction, and creating operational expectations" (p. xiii).

Feste defines intervention as "a foreign policy tool of strong states seeking to force their will on weaker ones through temporary, limited acts of defined scope" (p. xiv). But as recent events in Kosovo, East Timor, and Iraq have demonstrated, interventions in this new century have defied the expectations of scholars and practitioners with respect to their duration. In contemporary international relations, the reconstruction of a society's security, legal, and economic infrastructure is viewed as essential if the purposes of the original intervention are to be realized. Some, such as Robert Keohane, even suggest that "[d]ecisions 'before intervention' should depend, to some extent, on prospects for institution-building 'after intervention'" (p. 276).

These two books assume different theoretical and normative postures with respect to the intervention dilemma. For Feste's *Intervention,* the focus is the United States—more specifically, the philosophy that underpins U.S. intervention in the twenty-first century. By describing how the United States has structured its responsibilities through intervention in the past, Feste endeavors to formulate a more coherent doctrine for American policymakers in the present. The main drawback of such an approach (as she herself admits on p. 234) is that the search for a U.S. "intervention doctrine" is ultimately illusive. While the so-called Vietnam Syndrome continues to affect U.S. policymakers, particularly in their choice of military strategy, this hardly constitutes the coherence and consistency that one would expect from a doctrine. The only mantra she uncovers that resembles a guiding philosophy for U.S. intervention is the idea of "selective engagement"—an umbrella concept that can justify just about anything (including inaction). The author also faces obstacles when she attempts to quantify the variables associated with an intervention decision. For example, in Chapter 5, she rates the degree of U.S. national interest at stake in different crises of the last two decades. In the case of Somalia (1992), we are told that the national interest meter read "medium," whereas in Rwanda two years later, it read only "low." But how scientific is this reading, and what is the time horizon for the measurement? At the time, in 1992, it was not at all clear that the United States had pressing national interests in Somalia, yet George H. W. Bush was persuaded that action to halt a pending humanitarian crisis was the "right thing to do." In 1994, still smarting from the very public display of U.S. soldiers being dragged through the streets of Mogadishu, the U.S. government made a different calculation about the imperative to act in Rwanda. It is questionable whether a measurement of national interest can offer a robust explanation of these contrasting responses.

These methodological challenges hint at an additional problem with Feste's work: the lack of a guiding theoretical framework. Her efforts to discuss intervention through the lenses of two main bodies of international relations theory—neorealism and constructivism—yield very little. From the perspective of neorealism, we learn that the United States contributes to the evolving structure of the international system through its intervention policies. But this insight is short on specifics, particularly with respect to what *explains* U.S. behavior. As Feste argues, global structure only "helps shape a superpower's response to world problems once its form is clear, widely accepted, and firmly set" (p. 1). And even then, she rightly points out, unipolarity does not guarantee hegemony.

By contrast, J. L. Holzgrefe and Keohane largely avoid the paradigms that dominate the discipline of international relations in prominent journals of political science, and draw upon well-known scholars from philosophy, ethics, and law. Thus, while they narrow the subject matter by focusing on a particular *kind* of intervention (interventions to prevent or end widespread suffering and/or grave violations of human rights), they offer a wider range of perspectives. Moreover, their driving questions are prescriptive rather than descriptive. Drawing from the words of UN Secretary General Kofi Annan: "On the one hand, is it legitimate for a regional organization to use force without a UN mandate? On the other, is it permissible to let gross and systematic violations of human rights, with grave humanitarian consequences, continue unchecked?" The starting point for all of the contributors to this edited collection is that humanitarian intervention, in some cases, can be justified. In short, sovereignty is not and should not be absolute. But the authors differ with respect to the legality of humanitarian intervention and its desirability as a practice in contemporary international society.

The focal point for analysis throughout *Humanitarian Intervention* is the process of change in international relations—most notably whether and how shifts in norms relating to human rights are changing the institution of sovereignty. For liberal international lawyer Fernando Teson, humanitarian intervention has become both a right and a duty for members of international society when the human rights of individuals, wherever they reside, are being abused. In Teson's view, the principle of nonintervention enshrined in the United Nations Charter and other instruments of international law is a "doctrine of the past" that "feeds on illiberal intellectual traditions," such as relativism, communitarianism, and nationalism (p. 128). If liberals are correct in describing the major purpose of states and governments as the protection and promotion of individual human rights, then the logical corollary, he argues, is that state sovereignty has only instrumental—not intrinsic—value. The ultimate objective, then, is to bring international law in line with an international morality that is giving increasing weight to the condition of individuals (as opposed to sovereign states).

Legal scholars Simon Chesterman and Michael Byers challenge Teson's willingness to override the principle of nonintervention by emphasizing its egalitarian underpinnings and its crucial role protecting weaker states in the international system. Drawing upon key developments in the customary law regulating the use of force, Chesterman and Byers insist that interventions, such as those in Northern Iraq (1991) and Kosovo (1999), can only be justified through a plea to "exceptional legality." For them, Teson's desire to change the content of that law is indicative of an even deeper problem: a movement on the part of a small group of "Anglo-American" lawyers to impose a new legal regime against the opinion of legal scholars in the developing world. This exercise in legal hegemony, they argue, "detracts from, and may undermine, the significant advances over the past half century in the fields of human rights and conflict prevention" (p. 179).

The contribution of philosopher Allen Buchanan occupies an intriguing position between these two opposing positions. On the one hand, Buchanan appears sympathetic to Teson's liberal impulse: Humanitarian intervention can be viewed as legitimate if it meets a "Simple Moral Necessity Justification" (p. 132). Yet the author imposes a further hurdle by arguing that states seeking to change international law, and promote human rights through intervention, must meet a set of legal criteria as well. In other words, there are both justifiable and unjustifiable attempts to reform international law. Buchanan's key example is the Kosovo intervention, where, in his view, NATO countries failed to provide a compelling alternative rule to the existing requirements of Security Council endorsement for military intervention. As a result, he concludes, the intervention was an unjustified effort in legal reform.

As my analysis suggests, the ghost of the Kosovo war hovers above most of the chapters in this volume. While the influence of that controversial intervention leads to stimulating and highly original contributions, one wonders how the authors would respond to the two wars that have followed the terrorist attacks of September 11. In the initial aftermath of 9/11, it appeared as though the divisive issue of humanitarian intervention would fall off the policy agenda as Western states struggled to develop a new strategy to fight terrorism. Nonetheless, as the last two chapters by Keohane and Michael Ignatieff suggest, there are two important ways in which humanitarian intervention connects with the broader war on terrorism.

First, it is clear that post–Cold War changes in the conception of sovereignty and the troubling phenomenon of so-called failed states, both of which fueled the interventions for humanitarian purposes in the 1990s, remain as relevant as ever in a post–9/11 world. The terrorist acts of 2001 brought home to Western states the reality that instability within or collapse of a state anywhere in the world can have implications that reach far wider than that particular region. Second, the debate over humanitarian intervention has revived the just-war discussion about the constraints that should be placed on the use of force in international society. These just-war principles—which include just cause, last resort, and proper authority—animated many of the discussion about the use of force in Afghanistan and Iraq. It was the last of these, authorization, that for many states provided the ultimate test of whether to join the American-led coalition to topple Saddam Hussein.

Yet even before 9/11, it was questionable whether authorization itself was really the key issue in the debates about whether or not to intervene for humanitarian purposes. As Chesterman and Byers remind us, clarity on who can authorize intervention will not make action inevitable: "States are not champing at the bit to intervene in support of human rights around the globe, prevented only by an intransigent Security Council and the absence of clear criteria to intervene without its authority. The problem, instead, is the absence of the will to act at all" (p. 202). As the events of the summer of 2004 in Darfur painfully show, it is that intangible quality, political will, that continues to make all the difference.

Religion, Civilization, and Civil War: 1945 Through the New Millennium. By Jonathan Fox. Lanham, MD: Lexington Books, 2004. 312p. $75.00.

— Andreas Hasenclever, *University of Tuebingen*

Jonathan Fox's book makes a very important contribution to the ongoing debate on the impact of religion on domestic conflict. On the one hand, the author takes issue with Samuel Huntington's clash-of-civilizations thesis. As many others before him, Fox does not find much quantitative support for Huntington's bold expectations: We neither experience a major reorganization of international relations along civilizationally defined fault lines nor observe a significant increase in the number of violent disputes involving parties from different civilizations. On the other hand, he objects to those recent studies according to which the onset of civil wars is determined by political and economic factors, while religion is found to be largely irrelevant (see, for instance, James Fearon and David Laitin, "Ethnicity, Insurgency, and Civil War," *American Political Science Review* 97 [no. 1, 2003]: 75–90, or Errol Henderson and David J. Singer, "Civil War in the Post-Colonial World, 1946–92," *Journal of Peace Research* 37 [no. 3, 2000]: 275–99). By using data both from the Minorities at Risk Project (MAR) and the State Failure Project (SF), *Religion, Civilization, and Civil War* provides strong evidence that religious identity—whether the conflict involves two groups who belong to different religions or to different denominations—and, even more so, religious grievances do influence conflict escalation. This can be best studied when the crude distinctions of Huntington are questioned and religious diversity within civilizations is recognized.

The reader is presented with many interesting findings that cannot be exhaustively addressed in a single review. Most importantly, the author argues that conflicts involving religious issues became increasingly violent during the 1980s and that they are now even more violent than nonreligious disputes. This finding fits nicely with a recent study by Andrej Tusicisny ("Civilizational Conflicts: More Frequent, Longer, and Bloodier?" *Journal of Peace Research* 41 [no. 4, 2004]: 485–98). Although religion never appeared to be the primary cause of conflict, it always operated as an important intervening variable that, especially in combination with separatism, produced consistent results. Second, Fox found no religion or denomination to be particularly aggressive. Or to put it differently, the specific faith of the conflicting parties has no significant influence on the level of violence. This finding points to the important role of political elites within religions that more or less successfully seize upon preexisting traditions. Third, religious institutions matter. Fox found that groups with no formal religious institutions rebel less than do better-organized communities. However, it is not the groups with the most organized religious institutions—those with large-scale formal ecclesiastical networks—who use the most violence in their conflicts, but rather those with comparatively flat and nonhierarchical organizational structures. This observation supports similar findings of the Fundamentalism Project of the American Academy of Arts and Sciences. Fourth, political and military intervention by external powers appears to be more likely when the conflicting parties in the target state differ in their religious identities or when religious issues are at stake.

Finally, Fox reasonably speculates about conflicts involving religious issues that do not involve differences in religious identity. This is a very important step, since most studies searching for religious correlates of civil war still focus on Huntington's crude distinction of seven or eight civilizations. By ignoring religious diversity within civilizations and coding conflicts involving, for instance, secular governments and militant fundamentalists as intrareligious disputes, these studies artificially increase the number of observations of armed conflicts that do not involve religious differences. To illustrate this point, Fox found that during the 1990s, a growing proportion of conflicts within Islamic states involved religious issues and that these conflicts tended to become more and more violent. To consider these conflicts intrareligious would lead to an underestimation of the impact of religion on dispute escalation.

Repeatedly throughout the book, Fox stresses that his study constitutes the most thorough quantitative analysis of the impact of religion on domestic conflict to date. This claim certainly has some merits, and the author indisputably presents an authoritative analysis of MAR and SF data on religion and violence. However, some critical limitations of his study should be mentioned. First of all, the multivariate analyses of violent disputes concentrate on ethnic conflicts and are limited to the 1990s. Whether the patterns found for this particular type of conflict in this particular time period can be generalized has still to be investigated. Additionally, the author does not address explicitly scholarly work on civil wars presenting conflicting evidence to his claims. Most important in this regard, his study fails to include control variables that are widely used in other quantitative studies, such as a country's economic development or the dependency of violent disputes on past conflicts. Whether his findings will survive these tests is still an open question. Second, while Fox discovered a number of interesting empirical patterns, he is reluctant to address them theoretically. What does it mean for our understanding of civil wars that religion independently affects the escalation of conflicts, as the author pretends? Third and closely related to this point, he remains silent on the policy implications of his findings. While we have learned that the infusion of conflicts with religious issues significantly increases the probability of armed confrontations, we still do not know what follows from this information. Is there something that we can do to minimize the impact of religion on conflict behavior?

Overall, *Religion, Civilization, and Civil War* constitutes a significant addition to the literature on the impact of faith on domestic conflicts. It is clearly written, and the presentation of findings is well structured and accessible. The book should be required reading for all students with an interest in the topic, and it is a valuable resource for teaching graduate courses in conflict analysis.

European Conquest and the Rights of Indigenous Peoples: The Moral Backwardness of International Society. By Paul Keal. Cambridge: Cambridge University Press, 2003. 274p. $70.00 cloth, $24.99 paper.

— Edward Keene, *Georgia Institute of Technology*

Paul Keal's study of the position that indigenous peoples have occupied in international society is best read as a critical contribution to the work of the "English School" of international relations theory. Members of this school have long had an interest in relationships between European and non-European peoples, usually understanding them in terms of the expansion of the European society of sovereign states. Keal takes this as his starting point, but he criticizes it as "incomplete" because "it has excluded the story of peoples destroyed and dispossessed in the process of expansion" (p. 36). The purpose of his book is to broaden our understanding of the expansion of international society to include those stories, and so invite us to think more critically about the moral value of the society of states.

Keal's argument contains three distinct strands. First, he wants to incorporate historical, sociological, and anthropological studies of European expansion, such as the work of Anthony Pagden, into the English School's account of the development of modern international society. Second, he wants to explain how indigenous peoples were dispossessed of their rights and how those rights have, or have not, been restored and protected in international law. The third theme is an attempt to open up questions about whether or not the society of states supports a just order in human society as a whole. This reflects a long-standing English School interest in the relationship between order and justice, but Keal wants to expand the scope of the debate here by introducing ideas from communitarian political philosophy and recent work on international ethics.

These are important lines of inquiry, and on each of them considered individually, *European Conquest and the Rights of Indigenous Peoples* has much to recommend it to international relations theorists. Its most valuable contribution is on the rights of indigenous peoples. Keal provides a detailed study of the evolution of international law on this issue, and a thoughtful assessment of strategies for promoting these rights in the future. The analysis of European expansion is somewhat less original, as is also the case with the discussion of contemporary thinking about justice and political community. These are reviews of existing literature, albeit applied to the novel case of the rights of indigenous peoples, rather than new contributions based on independent historical research or moral argument. Moreover, while the coverage of the literature is generally good, there are a few weaknesses. Keal might have made more use of Roger Epp's analysis of diplomatic relations with indigenous peoples, while his treatment of Charles Alexandrowicz's work is disappointingly thin (pp. 110–11), especially in view of its relevance for the study of the international legal framework within which relations between Europeans and non-Europeans were carried on. Another weakness is that the distinction between pluralism and solidarism in international relations theory is not explored in any detail (p. 216), leaving Keal's analysis of the moral value of the society of states constrained by a rather blunt conceptual distinction between international and world order that would have benefited from a more thorough engagement with recent work in the English School tradition by Nicholas Wheeler, among others.

A further worry concerns the links between the various elements contained in the study. Keal recognizes, for example, that there are cases in contemporary international society wherein indigenous peoples have been subjugated by non-European rulers (p. 17). While he may be correct that this does not affect the moral case for promoting their rights, it does pose the question of whether the mistreatment of indigenous peoples is merely a subset of the way in which Europeans have abused non-Europeans in general, or whether it possesses its own logic. They are obviously related, but more should have been done to explain precisely how the categories of non-European and indigenous map on to each other. The link between the analysis of the treatment of indigenous peoples and the criticism of modern international society as morally backward is also suspect. Keal's point is that some states rest on morally questionable foundations because they have denied indigenous peoples' rights; since the purpose of international society is to preserve the states of which it is composed, the legitimacy of international society as a whole is therefore questionable.

There are a couple of problems with this line of argument. First, Keal's recipe for protecting the rights of indigenous peoples is that they should be treated as self-determining subjects of international law. This does not suggest that international society is morally backward; on the contrary, it implies that another round in the "expansion" of international society is required. The author himself suggests that international society may emerge as a moral agent that "can help redress the legacy of its historic expansion" (p. 223). The fault, then, does not lie in international society, but rather in the powerful states that have neglected to follow its principles in their relations with indigenous peoples. Second, Keal's argument effectively ignores a point made at length earlier in the book: International society is concerned not only with preserving the states of which it is composed but also with making sure that they live up to a certain standard of judicial, diplomatic, and social organization, which used to be called "civilization." He touches on this when he asks whether his solution for protecting the rights of indigenous peoples is "anti-pluralist," but does so too briefly in view of how significant a contradiction it represents within his thesis about the link between the Eurocentrism and the moral backwardness of the society of states. The problem here may be that his criticisms of English School theory are largely directed at earlier works on the expansion of international society; it might have been better to have taken more recent work on the pluralist–solidarist distinction as the book's jumping-off point. This would probably have blunted some of his sharper criticisms, but would have made his moral argument more compelling.

How Democracies Lose Small Wars. By Gil Merom. New York: Cambridge University Press, 2003. 310p. $65.00 cloth, $22.99 paper.

— Ivan Arreguín-Toft, *Harvard University*

The essence of Gil Merom's argument in this book is that *democratic* strong actors cannot win small wars because these states are constrained by society to avoid the sacrifice—in both sustained and inflicted casualties—necessary to win: "My argument is that democracies fail in small wars because they find it extremely difficult to escalate the level of violence and brutality to that which can secure victory. They are restricted by their domestic structure, and in particular by the creed of some of their most articulate citizens and the opportunities their institutional makeup presents such citizens. Other states are not prone to lose small wars, and when they do fail in such wars it is mostly for realist reasons" (p. 15). Merom adds that "[e]ssentially, what prevents modern democracies from winning small wars is disagreement between state and society over expedient and moral issues that concern human life and dignity. . . . Achieving a certain balance between . . . the readiness to bear the cost of a war and the readiness to exact a painful toll from others—is a precondition for succeeding in war" (p. 19).

The argument reduces to the claim that in small wars, insensitivity to friendly casualties and a willingness to maximize violence against an opponent are necessary and sufficient conditions for victory; and that modern democracies are structurally constrained in both categories. The particular relationship between society and the state makes democratic strong actors too squeamish to win small wars.

Merom's work is a welcome addition to the three previous central works on asymmetric conflict—Andrew J. R. Mack's "Why Big Nations Lose Small Wars" (*World Politics* 27 [1975]: 175–200); Eliot Cohen's "Constraints on America's Conduct of Small Wars" (*International Security* 9 [1984]: 151–81); and my own "How the Weak Win Wars" (*International Security* 26 [2001]: 93–128). The book also engages a broader debate concerning ethics in international relations and the military and political utility of violations of the laws of war (Alexander Downes, "Targeting Civilians in War: The Starvation Blockades in WWI," and Ivan Arreguín-Toft, "The [F]utility of Barbarism: Assessing the Impact of the Systematic Harm of Non-Combatants as a Strategy in War," papers presented at the 105th Annual Meeting of the American Political Science Association, 2003.)

Mack's argument focused on the paradoxical implications of power on resolve to win: An asymmetry of power implied an inverse asymmetry of political vulnerability (domestic actors capable of thwarting an incumbent government's preferred policies) and that, combined with the assumption that weak actors would invariably use a guerrilla warfare strategy, explained why big nations lost small wars.

My own argument built on Mack's thesis to arrive at a *theory* of asymmetric conflict outcomes that relied on the notion of strategic interaction. I argued that there are two ideal-types of strategic approaches—direct and indirect—and that when these interact one way, wars are short and strong actors win. When they interact another way, however, wars are protracted and strong actors are much more likely to lose. My argument also built on Cohen's insights—now 20 years old—into why a powerful democratic state such as the United States might yet lose a small war. Cohen's biting analysis focused on the special demands of counterinsurgency warfare, and his argument rested on the causal impact—not of social restraint—but of a military's capacity to *learn* from its mistakes and then *institutionalize* important lessons so as to train and equip the sort of forces necessary to win small wars. Merom's argument supplements but in no way supplants this key cause of strong actor failure in small wars.

If my own theory has a weakness, it is that it does not concern itself—except in a peripheral and speculative way—with the important question of *why* actors choose the strategies they do.

Here is where Merom's thesis has the chance to make its strongest contribution to the nascent rebirth of asymmetric conflict research (itself intimately tied to questions of legitimacy and ethics in the use of armed force). Merom offers an explanation that focuses on the domestic-level constraints on actors seeking an ideal strategy, and his argument makes it possible to explain the puzzling case of a strong state pursuing political objectives using the wrong strategy and the wrong mix of forces. But there are three important problems with Merom's argument.

First, we have important examples of democratic strong actors initiating brutal strategies (or prodigiously expending soldiers) and still losing small wars (e.g., the French in Indochina, 1945–54, and the United States in Vietnam, 1965–73), as well as authoritarian regimes doing the same thing and losing (e.g., the Nazis in Yugoslavia, 1941–43, and the Soviet Union in Afghanistan, 1979–89). Merom would likely respond that although in Yugoslavia, Indochina, Vietnam, and Afghanistan the strong actor was in fact brutal, what was needed was pure *genocidal* brutality; and for reasons particular to each case, this did not happen. But this argument ignores three constraints that have nothing to do with societal pressure: the constraints that follow from the necessity of avoiding the risk of escalation to a world war; the constraints that follow from a sense that the utility of further brutality may be either marginal or even negative (it may cause friendly psychological casualties or provoke external intervention); and the constraints that follow from a common response to extreme brutality as a counterinsurgency strategy: moving base areas across interstate boundaries.

A second problem with Merom's argument is its insistence that extreme brutality pays. But the logic of this argument is equivocal, and the record of success in the use of barbarism is mixed at best (Arreguín-Toft, 2003). The best explanation for this recent historical development is something Merom's argument points to but that he overlooks: The same social changes—amounting to an expanding international norm—constraining the use of genocidal brutality by democratic strong actors also affected weak actors, transmuting their resistance against extreme cruelty and hopeless odds into glorious martyrdom.

Moreover, democratic states facing competent insurgencies have another option besides barbarism: conciliation. Britain succeeded with this strategy, which involves using economic incentives and political reforms to address an insurgency's legitimate grievances, in the Malayan Emergency of 1948. Cohen's analysis (1984) and mine (2001) also drive home the point that with the right mix of forces and strategies, democratic strong actors can win small wars without resorting to barbarism and without sustaining high casualties (the real cost of such campaigns is *time*).

Finally, Merom's thesis is not well tested. His research design ignores the success and failure rates of authoritarian states, states less constrained by 1) dependence of the state on society for military forces; 2) a general preference for measures short of war by society; or 3) the ability of society, through charismatic representatives, to alter the state's foreign policy or military strategy. It also cannot help us weigh the causal impact of *other* plausible causes of democratic state restraint in small wars.

These problems aside, Merom's account of the political vulnerability of democratic actors in small wars is both excellent and timely. *How Democracies Lose Small Wars* expands on Mack's description of why and how democratic strong actors are politically vulnerable, and offers a useful explanation of why democratic states choose the strategies they do, and why they stick to failing strategies even after they have received a clear indication that their strategy for victory is in fact failing.

The Structure of Regulatory Competition: Corporations and Public Policies in a Global Economy. By Dale D. Murphy. New York: Oxford University Press, 2004. 336p. $99.00.

— Dorothee Heisenberg, *Johns Hopkins University*

Dale Murphy addresses the study of globalization, specifically global regulatory competition, with the careful empirical work of a former banker and the analytical bite of a political science scholar. The result is a coherent theoretical statement about when, and under what conditions, states will dismantle regulations (the colloquial "race to the bottom," or "competition-in-laxity" as Murphy labels it), harmonize them to the highest level (a trend David Vogel named the "California effect"), or allow heterogeneous regulations to prevail. What sets Murphy's book apart from other political science scholarship in this area is that he abandons the assumption that corporations have monolithic regulatory preferences, and instead unpacks the "black box" of the corporation. He then gives corporate preferences weight in the regulatory process. Thus, the motivations and preferences of corporations are significant drivers of a state's regulations, and determine whether regulations will be harmonized upward or downward in international regulatory competition: "Powerful firms exert influence but they do not seek a monotonic goal; one must identify each firm's different interests. Over time, these preferences will shape state regulations" (p. 9).

Corporations' preferences for or against greater regulation, in turn, stem from three structural factors: the locus of regulations, meaning whether the jurisdiction regulates the production process or market access; the characteristics of that industry or sector's market structure; and the specificity of the corporation's assets. Murphy selected six cases that vary on these structural factors—shipping flags-of-convenience, offshore finance, the Montreal Protocol on CFCs (chlorofluorocarbons), the Basle Accord on Capital Adequacy, Mexican tuna-dolphin, and the Infant Formula Marketing in the United States—to show how these sectoral or industry conditions shape corporations' regulatory preferences. The meat of the book is in these extensive case studies, and they are extremely thorough and wide-ranging. The first two cases show a trend toward lower-common-denominator harmonized regulation, the second two toward higher-common-denominator harmonized regulation, and the final two to a heterogeneous (nonharmonized) regulation. One of the outcomes his typology resolutely ignores is mutual recognition agreements. MRAs are agreements to accept other countries' regulations and tend to put downward pressure on governments' standards. These can be very important (e.g., the European experience), but Murphy does not include a discussion of the corporate preferences that would allow governments to conclude MRA agreements, declaring in a footnote that "MRAs do not obviate the utility of the three analytical trajectories used here . . . just as warm water does not obviate the distinction between cold and hot. They do, however, raise an interesting new question as to what conditions lead to their implementation" (p. 241).

Murphy is careful, in his analysis of the cases, not to be overly deterministic in his assessments, for example, noting that "over the long term, asset specificity may change as firms change their investment strategies" (pp. 226–27), and thus implying a strategic dimension to the business–government interaction. He is similarly open to accepting that the vagaries of fate, as well as the characteristics of leadership, play an important part of the story of corporations' preference: "[A] key element of choice that emerges in the case studies above are [*sic*] the decisions by business strategists and policy entrepreneurs to lead or to lag in inducing regulatory change" (p. 242). Clearly, however, his focus in the entire book is on the corporation's side of the equation, and the transmission of corporate preferences to the government is less explored. To the extent that there are cross-national differences in the degree to which the government's position on regulation may be shaped by other, noncorporate, interests, Murphy's framework and cases may overgeneralize from America's business–government relationship. Would statist governments be less likely to incorporate only the regulatory preferences of businesses? Would the European Commission, which now proposes a majority of European regulation, be as beholden to business interests as national governments? (And if so, which ones?) There is an interesting story to tell about the transmission of preferences from business to government, but as the author correctly points out, there are more studies on that process than on his essential contribution that establishes the origins of the regulatory preferences of businesses.

Murphy has 20 pages of implications for next-generation research on international political economy, as well as detailed observations about what his case studies and variables imply for other theoretical perspectives. In particular, he critiques many of the nonmaterialist motivations often ascribed to the higher-regulation outcomes, such as the Montreal Protocol on CFCs, the ban on dolphin–lethal tuna imports, and the restrictions on infant formula advertising: "[I]t was clear from the case details that corporate interests were critical. . . . [T]o unearth these material aspects takes considerable perseverance, as they are often deliberately concealed" (p. 241). He nevertheless acknowledges a place (albeit secondary) for these other variables in rounding out cases where material motivations are ambiguous.

Perhaps the most engaging segment of *The Structure of Regulatory Competition* is the "practical implications for business and policy leaders" section. Here, Murphy's background in corporations and government shines, and he is able to distill lessons from his research. Moreover, he is quite modest about the predictive claims toward one form of regulation over another. By not overselling his results, he highlights his contribution effectively. His emphasis on differentiating and integrating firm preferences into international relations is welcome and rare. One hopes that other international relations scholars will take the time to open up the black box of "business" and use those insights as effectively as Dale Murphy does.

The Nation-State in Question. Edited by T. V. Paul, G. John Ikenberry, and John Hall. Princeton: Princeton University Press, 2003. 400p. $60.00 cloth, $22.95 paper.

— Martin van Creveld, *The Hebrew University, Jerusalem*

As John A. Hall notes in the introduction, many authors (including the present writer) writing in the 1990s questioned the future of the nation-state, which, to them, was coming under attack both from above—at the hand of global economic forces—and from below, at the hand of various national or ethnic revivals. As he also notes, the purpose of the volume under review is to question that question and see whether there is still some life left in the nation-state. To do so, he and his fellow editors have assembled an impressive battery of American, Canadian, and British scholars, each of whom has something interesting to say. Let us see, then, what they do say.

The book's first part, "National Identities," looks at the link between states and nationalism. As Bernard Yack reminds us, originally nationalism, popular sovereignty, and the liberal democratic state went together; the real problem, present almost from the beginning but made especially acute (for Yack) by recent events in the former Yugoslavia, is whether the world can afford a form of nationalism that is neither liberal-democratic nor based on popular sovereignty. Turning this question on its head, Brendan O'Leary argues that every state needs a state people (he uses the original German *Staatsvolk* here) and that, consequently, the European Community will never become a state. Anatoly M. Khazanov applies this reasoning to the Russian federation, noting that it consists of numerous, often very different, ethnic groups and expressing doubt whether, under such conditions, it can really develop into a liberal-democratic state on the Western model or will revert to authoritarianism. Finally, Peter Baldwin discusses the attempts of many states to use biotechnical means in order to impose much greater control over their populations, say (to mention a case I came across while writing this review), by implanting chips in people so that any disease they may have can quickly be identified. This is indeed a terrifying perspective and one that is relentlessly growing on us. For good or ill, though, the biomedical device that can distinguish, say, an ethnic Serb from an ethic Albanian is not in sight. Hence, I could only wonder why this essay was included in this particular part of the book.

Part II deals with "state security" and itself consists of two essays. In the first, T. V. Paul argues that providing security against other states remains an important function of the post–Cold War state. In this he is right; however, he forgets to mention that since nuclear proliferation has created a situation where six decades have passed since any first- (or even second-) rate states have fought each other in earnest, the meaning of security itself is very different from what it used to be. Next, Jeffrey Herbst argues that the European experience whereby the need to prepare and wage war led to the consolidation of the state, whereas the state in turn used its strength to wage war on other states, will not be repeated in Africa. He, too, may well have right on his side, but he, too, overlooks a very important aspect of the problem by neglecting to say that many African states are so torn by civil war as to barely deserve that name.

Part III looks at "state autonomy" in the international context. In the first essay, Francesco Duina reminds us that "states can always refuse to implement common market law" and that they may "assert their viewpoints and independence in the international arena." True enough, most of us would say, but does not North Korea provide a terrifying example of what going it alone may cost? In the second, Christopher Hood suggests that whereas in the recent past most states have been very good at taxing their citizens, "over time several factors could combine to erode the pillars on which the twentieth-century tax state was built." In the third, John L. Campbell asks "how much truth there is to [the] economic globalization thesis" that seems to undermine state power, and he concludes, a little predictably in view of the editors' overall thesis, that there is less here than meets the eye. Finally, Rudra Sil argues that in spite of the return in many places of what Shimon Peres recently called "pigsty capitalism," the state still has a say in industrial relations; to which one can only say, Amen.

This brings us to the fourth and last part, "state capacity." In what is surely one of the most interesting essays, Grzegorz Ekiert, using Poland as his main example, argues that East European communist states were actually much weaker than their "totalitarian" character suggested; also, that in some ways, the trend since 1991 has been toward more state power, not less, as most of us thought. By calling his essay on China "Rotten from Within," Minxin Pei tells us what he thinks of that country's development over the last 25 years or so. Finally, G. John Ikenberry in the concluding essay devotes some space to discussing the events of 9/11. To him, though, they do not prove that even the geographically most isolated, most powerful state on earth is no longer able to look after its citizens' security as it used to. Rather, he believes that "in the age of catastrophic terrorism, the world will never be completely safe until it is completely filled with sovereign states that have effectively established political control and the rule of law within their territory." Amen, again.

As they say, the editors' purpose was to reexamine the idea that the state is in decline. In their favor it must be said that the volume they have produced provides a much more complex and nuanced perspective. This reviewer cannot agree with everything the authors say, but that does not change the fact that all their essays are thoughtful, most are well written, and some are surprising. Hence, I can only recommend *The Nation-State in Question* to anybody who is interested in the nature of the state, the ways it relates to society, the factors that are causing it to change, and the direction it may take in the future.

When States Fail: Causes and Consequences. Edited by Robert I. Rotberg. Princeton: Princeton University Press, 2003. 336p. $65.00 cloth, $19.95 paper.

— William Reno, *Northwestern University*

Failed states have proliferated in the last 15 years. For much of this time Robert Rotberg has played a major role in defining debates about this development. His latest edited volume is a collection of 14 essays that examine why states fail and what to do about them once they fail. Most of the contributors are political scientists. A few are experts in conflict management and have worked directly in international efforts to remedy state failure.

All of the contributors agree that the essence of state failure lies in the loss of government capacities to control the exercise of coercion in their societies. This creates a distinctive kind of political system. Borrowing concepts from the study of international relations, such as anarchy, security dilemmas, and the evolution of cooperation, the contributors show how politics in failed states really works. For those concerned with remedies, the task is thus defined as coordinating cooperation. While cognizant of individual cases, this approach provides a general framework for comparison. In this pursuit, several of the authors focus on distinctions between organizations that provide public goods and those only serving the private interests of members. This is useful for distinguishing between failure and order, since it connects the exercise of violence to very different sets of social relationships. Such distinctions are critical for some of the chapters on state reconstruction that look beyond the capacity, level of violence, or size of some groups to ask instead whether they are capable of providing social order or whether they serve the interests of a small group of members.

Several of the chapters that focus on the internal politics of failed states borrow directly from international relations theories. The best is Nelson Kasfir's superb essay on cooperation amidst anarchy. Other scholars have referred to security dilemmas to explain why groups assume offensive intent among other groups who simply may be trying to fend off the worst effects of general mayhem. Kasfir observes that for security dilemmas to function, groups have to be fairly coordinated in their actions. But the abundant opportunities for predation in failed states lower the cost of entry into this mini-system, compared to the global system. Thus, leaders of these groups face serious difficulties in controlling followers. This undermines agreements with other groups and promotes fragmentation. Kasfir also recognizes that the groups that emerge out of predation and those emerging out of security dilemmas differ in their internal logics. The former usually pursues private interests and is prone to fragmentation, while the latter tends to provide more public goods.

Most of the eight chapters on responses to state failure focus on strategies to restore order. They prescribe ways to create legal cultures and to disarm combatants once fighting stops. This is useful in many instances, although when states have failed entirely or are very weak, it is difficult for well-meaning foreigners to find any institutional structures on which to build. Several contributors shift the focus to the positive roles that indigenous societal organizations can play. Daniel Posner, however, provides an intelligent caveat. Beware of aid to advocacy groups, he warns, as these are poor at coordinating action among members. Better to assist groups that already furnish services that enfeebled states cannot provide. On the one hand, this is a tricky proposition, as some of these groups are likely to promote distinctly nonliberal ideals, such as armed protection of a cultural or ethnic community. On the other hand, Posner observes that advocacy groups account for the many "fake NGOs" that emerge around the promise of foreign aid and really just provide employment for core organizers who are adept at telling the foreigners what they want to hear in exchange for a salary.

Two controversial and lively chapters are those by Christopher Clapham and Jeffrey Herbst. Clapham offers the unsettling proposition that state failure actually reflects the inability of many societies, especially in Africa, to accommodate statelike political structures. The individualism and clan politics of pastoral societies, for example, is good at dealing with survival and the need for shifting alliances when on the move in sparsely populated areas. It is poorly suited to the large-scale organizations needed to run states. Thus it may be that people in many failed states are reverting to a nonstate form of governance, and that "restoring" effective states requires social revolution and not simply restoring a status quo ante.

Herbst's suggestion that the international community ought to decertify failed states and not attempt to rebuild them in their old form is another variant of the idea that state failure is part of a deeper structural process. An implication of his suggestion is that new states could arise on the ashes of the old. But if Clapham is correct, prolonged statelessness will follow state failure, at least in some places. Add to that Michael Klare's chapter, which tells readers how the presence of small arms and the predatory opportunities in global clandestine trades may increase pressures to fragmentation and extend instability to neighboring regions. Thus, decertification raises the problem of what comes next in a world where stability is desirable. Both chapters throw down interesting challenges for the study of international relations, as they suggest that the structure of the state system itself is significantly frayed on its edges.

In this regard, it is surprising that the issue of protectorate status does not appear more often. Only Jens Meier Henrich argues for international conservatorship to create new administrations. If the problem of state failure is really deeply entrenched in the political and social structures of these countries, the need for semipermanent military and financial oversight may not be far-fetched. Drawn-out interventions in places like Sierra Leone, Congo, Afghanistan, Kosovo, and now (recently broken) Iraq suggests that the quick revival of failed states is overly optimistic. But protectorates—witness Iraq—have their problems, too. Failed states are a problem indeed, and *When States Fail* sheds light on this still understudied subject.

Whose World Order? Russia's Perception of American Ideas After the Cold War. By Andrei P. Tsygankov. Notre Dame: University of Notre Dame Press, 2004. 224p. $45.00 cloth, $22.00 paper.

— William Zimmerman, *University of Michigan*

One consequence of the end of the Cold War has been the muddling of boundaries between Russian and Western, notably American, scholars. In my own experience, this has been all to the good. Social scientists at the University of Michigan have had, for instance, an ongoing relationship with their counterparts at the European University in Saint Petersburg (EUSP) for many years now. In our meetings, the distinctions between the Self and the Other (to use the constructivist jargon favored by Andrei P. Tsygankov in the book under review) have often been rather arbitrary. The intellectual divide we have most frequently encountered has been between those of us, Russians and Americans, who attach relatively greater weight to the role of culture and those for whom institutions matter more, a divide that separates us more by disciplinary background than by citizenship or where we teach.

Tsygankov himself represents an example of such blurring of boundaries. He was trained in Moscow and the United States and now teaches here. But his *Whose World Order?* tells a much different story from that recounted in the previous paragraph. Rather, it is a story in which ethnocentrism, realism, and cosmopolitanism as exemplified by the writings of Francis Fukuyama and Samuel P. Huntington have had profoundly deleterious affects on U.S.–Russian relations. Fukuyama's "end of history" thesis and Huntington's "clash of civilizations" thesis, Tsygankov argues, shaped the Russian elite dialogue about Russia's relations with the West and about its own internal evolution in ways that have had adverse consequences for Russian–American relations and for the flourishing of Western values in Russia. He recognizes there are others (e.g., p. 129) who must share some of the blame for the deterioration of Russian–American relations in the years following those immediately after the collapse of the Soviet Union. Nevertheless, to the stock Russian question, *Kto vinovat?* (Who is to blame?)—in this instance, for the precipitous decline in favorable views about the United States and its foreign policy among Russian elites in the years since the collapse of the Soviet Union—a substantial part of his answer is that Fukuyama and Huntington are the culprits.

Two things are at the basis of this conclusion. The first is his reading of the Russian commentary about both the clash-of-civilizations and the end-of-history arguments. His account of that commentary is plausible enough. But this depiction of the Russian dialogue is then followed by some inferential leaps that are really quite startling and a normative critique of Fukuyama's and Huntington's work that most readers, I suspect, will find ultimately unpersuasive, even if they, like me, find the evidentiary basis for both the clash-of-civilization and end-of-history theses quite dubious. Thus, he asserts that "to the extent that the 'end of history' and 'clash of civilizations' theses were involved in Russia's domestic intellectual developments, their authors are responsible for the rise of Russian discourses of isolation and anti-Western hostility" (p. 129). That is pretty stern stuff, especially if Huntington's and Fukuyama's writings are placed on a scale that includes other real world examples of insensitivity to Russian concerns on the part of U.S. and International Monetary Fund policymakers, along with a litany of ways that Russian economic and political developments in the 1990s contributed to the tarnishing of Western ideas.

The second element underlying Tsygankov's conclusion is his communitarian belief that "the central moral lesson of Fukuyama's and Huntington's engagements with Russia is that an intellectual is responsible for how his or her ideas are perceived outside the idea's immediate cultural context" (p. 129). From this premise, he argues that culturally sensitive scholarship that is multidisciplinary and epistemologically pluralistic rather than privileging a single discipline or epistemology, that is cross-cultural and politically diverse, and that entails "globally oriented" rather than "locally oriented" policy prescriptions is to be preferred (p. 131).

At some level, such propositions are unexceptional; my colleague Scott Page is one of those who has shown most rigorously the gains from diversity ("Problem Solving by Heterogeneous Agents," *Journal of Economic Theory* 97 [2001]: 123–63, with Lu Hong). At the same time, it pays to remember some propositions that may get lost in our quest for cultural sensitivity. Not all epistemologies are created equal; there is a there there. Sometimes multidisciplinarity is a cover for lack of rigor. Occasionally, even, responsible decision makers are morally justified in advocating positions that preference intense local feelings over generalized global beliefs. That being said, it is to Tsygankov's credit that he has reminded us that what we do as scholars sometimes has far-reaching effects and is urging us to be mindful of the consequences of our actions.

Moving Money: Banking and Finance in the Industrialized World. By Daniel Verdier. New York: Cambridge University Press, 2003. 326p. $65.00 cloth, $23.99 paper.

— Timothy J. Sinclair, *University of Warwick*

Although it may seem arcane or prosaic to some, the literature on the political economy of money and finance has burgeoned within political science since the mid-1980s. The two decades after the end of the Bretton Woods system in the early 1970s was a time of considerable macroeconomic volatility and cooperative effort to coordinate suitable policy responses. This stimulated scholarly inquiry. Since then, financial globalization has drawn in those interested in the challenge financial markets seem to pose to the official political order centered on states. This developing political science research tradition has, however, existed on the periphery of two other longer-standing literatures. The first is the economic history of monetary and financial affairs. This tradition has exhaustively investigated the formation, organization, and implications of the classical gold standard and the Bretton Woods order. This research stimulated many of the political science pioneers and remains seminal and vibrant. The other tradition to mention is that impulse in the social sciences to identify and characterize different forms, tendencies, or cycles in capitalism, and link these to distinct political and social orders. This much more controversial way of thinking has its origins in the great nineteenth-century systems of ideas. In more recent times, Karl Polanyi, Joseph A. Schumpeter, Alexander Gerschenkron, John Zysman, and the many authors associated with core-periphery and world systems theory have kept the tradition alive. Although many readers will assume Daniel Verdier's new book is unproblematically part of the political science tradition, it should be read squarely as a product of this last tendency, making use of the established historical work to generate a reinterpretation.

Verdier wants to tell a very big story. *Moving Money* seeks to explain the neglected, but as he sees it, central role of the constitution of states in creating the specific forms finance takes in different societies. Why have some societies been dominated by local banks (United States), while other societies—such as Britain—have been typified by centralized financial structures with just a few large banks? He argues that financial markets are, if allowed to organize things themselves, centralizing, inherently likely to pull money away from the peripheries. Could it be, he suggests, that the centralized and centralizing nature of some states stops local regions from retaining their financial assets, allowing these to be drawn into a financial center where the depth of financial markets offers the greatest returns? Decentralized states, in contrast, allow regions to establish barriers that help them retain local banking systems and thus retain financial resources in the periphery.

The author investigates the tendencies toward centralization and decentralization by considering four apt dimensions that order financial markets: spatial concentration, internationalization, market development, and specialization, in two time periods: 1850–1913 and 1960–2000. The analysis is comparative across the developed states of North America, Europe, and Australasia. The text consists of an interpretation of the existing economic history and political economy of money and finance literatures, to which he adds his own quantitative analysis of broad aggregates, focused on making his point about the impact of the centralization or decentralization of the state on the organization of banking. Verdier's reading of the qualitative literature used is schematic and abstract, and little in the way of historical specificity enters into his analysis. This is not a reading of financial history that most financial historians will appreciate.

It would be inane to celebrate this book as a "return to the state" in the study of the political economy of money and finance. Other authors have made the same ontological commitment in recent years. It would also underestimate the value of Verdier's contribution. Where he goes beyond most of the literature in the field is in being both more political and more financial. By political, I mean that he does not see the role of states as universal or interchangeable, like lightbulbs. What he thinks important is the differing constitutions or organization of states in relation to their societies. Decentralized states are quite different from centralized ones, he argues, with major implications for the possible forms of financial structure that can emerge. He is more financially sensitive than most writers in the field, too, because he understands different financial instruments and the likely impact of different institutional forms within banking.

Verdier's book is mainly a work of reinterpretation, rather than new research. Indeed, his original quantitative work may have less impact than it should. Many readers are likely to find the broad statistical comparisons he offers difficult. The fact that the research technique is so explicit will narrow the readership to established scholars and advanced graduate students. Agency seems to have little role in the account, with structural variables being the key to the pattern of development. Although sensitive to institutions and financial forms, this book is not influenced by constructivist thinking. It is certainly iconoclastic and undoubtedly a challenging read.

Moving Money is a work of great ambition and insight, a wide-ranging inquiry into issues of great import. As a work of political science, it is both strange in its comprehensiveness and narrow in its heavy emphasis on structural factors and neglect of agency. Despite this concern, in focusing squarely on forms of state and forms of finance, Verdier has made a major contribution to the political economy of money and finance, and possibly to the broader literature concerned with changing forms of capitalism and political organization. Whether this book stimulates debate among scholars will depend on their ability to appreciate its uniqueness. The likely benefits make that appreciation worthwhile.

Dangerous Alliances: Proponents of Peace, Weapons of War. By Patricia A. Weitsman. Stanford: Stanford University Press, 2004. 264p. $49.50.

— Jeffrey M. Ritter, *Rutgers University*

Patricia Weitsman sets out to provide a unified theory of alliance politics that explains when states will manage threatening neighbors by allying with them rather than allying against them, when allies will undertake different types of military obligations, when alliances will be more or less cohesive, and how alliances aimed at preserving peace sometimes provoke war. Although she is not entirely successful in fulfilling this extensive agenda, *Dangerous Alliances* is nevertheless a constructive contribution to several ongoing debates about alliance politics.

With a tip of the hat to Stephen Walt's *The Origins of Alliances* (1987), Weitsman accepts the idea that states form alliances in response to threats. She builds upon Walt's argument, however, by characterizing "threat" as a continuous variable and arguing that alliance behavior may vary in response to different levels of threat. Low-level threats encourage "hedging" behavior, an unspecified mix of half-hearted gestures toward both friends and enemies, aimed at preserving flexibility and forestalling trouble. Moderate threats prompt states to form "tethering" alliances with their enemies in hopes of restraining them. States form the balancing alliances described by Walt at higher levels of threat, resorting to traditional "bandwagoning" alliances only when threats become so overwhelming that balancing is impractical.

Weitsman further argues that the type of military obligations incorporated into alliance agreements and the overall cohesiveness of alliances are both a function of the character of the threat to which the allies are responding. For example, "balancing" alliances provoked by severe external threats tend to be cohesive and to involve promises of military assistance, while tethering alliances occasioned by moderate threats by alliance partners tend to be less cohesive and to involve promises of consultation or neutrality. Finally, Weitsman appeals to the security dilemma by suggesting that tethering alliances may provoke hostile reactions from nonmembers, who misunderstand the allies' aims. Although this argument seems to have provided the title for the book, it actually appears as something of an afterthought.

The most striking contribution of the book is the author's effort to sketch the underlying logic of "alliances of restraint" of the sort outlined by Paul Schroeder (see Schroeder, "Alliances 1815–1945: Weapons of Power and Tools of Management," in Klaus Knorr, ed., *Historical Dimensions of National Security Problems,* 1976). An alliance can provide influence over a partner to the extent that it provides a valuable and retractable advantage relative to some third party. I have some misgivings that distinguishing tethering from balancing alliances may tend to obscure the fact that deterrent and offensive alliances also involve compromises between allies in order to secure advantages relative to third parties, but Weitsman is certainly correct that intra-alliance advantages may sometimes be the primary motive, rather than a secondary motive, for states entering into military agreements. On occasion, she argues that tethering alliances keep the peace between allies without providing any advantages against nonmembers, but unfortunately, she does not specify how such alliances provide the partners with any incentives to accommodate each other that did not exist in the absence of the alliance.

The author's ambitious argument about how states' motives translate into the types of promises they make and the reliability of the resulting alliances is perhaps a little too straightforward, omitting any role for strategic behavior or for uncertainty about the true motivations of one's partners or one's enemies. For example, she reasons that because tethered allies are wary of each other, they will promise each other only very limited support, and they are unlikely to honor even these promises if put to the test. If so, then it is not clear why such alliances provide the partners with leverage to restrain each other in the first place: Why would a potential enemy mind its manners in order to preserve a predictably worthless promise?

The major portion of the book is devoted to case studies of the major alliances between the European powers between the Congress of Berlin and the end of the First World War: the two Leagues of the Three Emperors, the Austro-German alliance, the Triple Alliance, the Franco-Russian Alliance, and the Triple Entente. This choice of cases seems a bit unfortunate, because Schroeder covers the same ground in the essay that Weitsman credits with inspiring her hypotheses on tethering alliances in the first place. Weitsman devotes a chapter to the two Leagues of the Three Emperors, which Schroeder ignored, and she rightly treats the Triple Alliance as distinct from the Austro-German alliance rather than subsidiary to it, but the reader might be forgiven a sense of déjà vu at many of her conclusions.

The case studies are well worth reading, and the author deserves credit for doing original archival research rather than relying entirely upon secondary sources. Students of the period will, of course, find points of interpretation to dispute. The studies of the second League of the Three Emperors and the Triple Alliance seem a bit unbalanced without any discussion of the Mediterranean Agreements or of Italy's dealings with France in the early 1900s. It seems to me that Weitsman underplays Austria-Hungary's ambitions to create a sort of continental coalition against Russia in favor of emphasizing its tethering motives. Her decision to follow the Triple Alliance and the Triple Entente through 1918, however, is a nice touch that sets her book apart from the crowd of studies that considers alliances only in the context of extended deterrence.

Weitsman's effort to consider alliances of mutual restraint in a "balance of threat" context is both welcome and long overdue, and her suggestion that states may adopt different alliance policies in response to different levels of threat is worthy of further consideration. *Dangerous Alliances* pushes Walt's balance-of-threat framework in a significant new direction, and it deserves the attention of any serious student of alliance politics.