

BOOK REVIEWS

Political Theory

Culture and Equality: An Egalitarian Critique of Multiculturalism. By Brian Barry. Cambridge, MA: Harvard University Press, 2001. 399p. \$35.00.

Monique Deveaux, *Williams College*

Barry is well known for his hard-nosed, no-nonsense defenses of traditional liberal principles, as evidenced in his *Theories of Justice* (1989) and *Justice as Impartiality* (1995). Here he turns his attention to arguments by proponents of multiculturalism in political life, liberal and nonliberal alike. The book is divided into three main sections: Multiculturalism and Equal Treatment, Multiculturalism and Groups, and Multiculturalism, Universalism, and Egalitarianism. The central argument does not so much unfold gradually as it is asserted at the outset and then defended intermittently thereafter.

Barry's task is twofold: Offer a thorough and critical analysis of recent political theory that advocates cultural pluralism, and defend core liberal principles and a conception of liberalism as equal justice for all against what the author plainly sees as the misguided efforts of multiculturalists. Barry seeks to wrest liberal principles from the hands of so-called liberal proponents of multiculturalism (such as Will Kymlicka and Chandran Kukathas) as well as to show why efforts to discredit liberalism as inadequate to the demands of culturally plural societies are mistaken. Rather than eschew liberal norms or reform them beyond all recognition, Barry argues, we need to seek solutions from within liberalism. Specifically, we need to extend equal rights and treatment to all, and we must rectify the pervasive social and economic disadvantages that multiculturalists wrongly impute to groups' distinctive cultural identities.

Barry makes no attempt to obscure his thoroughly skeptical view of this literature. Indeed, he states at the outset that, in his "naively rationalistic way, I used to believe that multiculturalism was bound sooner or later to sink under the weight of its intellectual weaknesses and that I would therefore be better employed writing about other topics" (p. 6). Only multiculturalism's inexplicable persistence persuaded him to undertake the task of rebutting the arguments of leading proponents. As befits this motivation, his central aim is to demonstrate that much of the normative argumentation underpinning proposals for greater accommodation of cultural minorities in liberal democratic states is fundamentally flawed. Barry also seeks to show that many if not most of the specific proposals advanced by contemporary political theorists for special rights, exemptions, and other arrangements for cultural minorities are poorly defended and potentially disastrous from the vantage point of politics.

Among the thinkers Barry takes on in his thorough and highly critical survey of the literature are Bikhu Parekh, Chandran Kukathas, Will Kymlicka, Charles Taylor, James Tully, and Iris Young. In his discussion of the various arguments of these and other proponents of cultural pluralism, Barry brings his analytical skills to bear in exposing vague, contradictory, and poorly argued aspects of their work, and he employs his political savvy to speculate about the potential effects of concrete proposals for accommodating cultural minorities. At times this strategy is remarkably insightful, as in his exploration of the implications of the suggestion (which he attributes to Iris Young) that liberal cultural minorities should have disproportionate power in shaping the public policies that apply to their own practices (p. 303).

At other times, Barry too readily engages in sarcastic dismissal of the works he discusses. His writing style is highly rhetorical and often witty, but the witticisms frequently come at the expense of authors whose arguments or political visions are forced through Barry's analytical grinder. Examples include Iris Young's *Justice and the Politics of Difference* (1990), which Barry discusses at length and whose central arguments he rejects on the basis of some very persuasive reasons. Nonetheless, punctuating his analysis are derisive asides—Young ultimately advances a "perverse thesis about the assimilationist impulse behind liberalism" (p. 69)—and he twice quotes a passage from Young's book that discusses an idea he finds particularly absurd, namely, the notion of a distinct women's culture as including traditional women's arts and rituals deriving from witchcraft (pp. 94 and 278).

Similarly, it is difficult to avoid the conclusion that Barry is merely poking fun at a central aboriginal image from James Tully's *Strange Multiplicity: Constitutionalism in an Age of Diversity* (1995)—the black canoe, employed by Tully as a metaphor for his vision of constitutionalism amid diversity—even though Barry hastens to assure readers that "there is nothing intrinsically absurd in any of the argumentative strategies that Tully attributes to the mythological bear" (p. 262). References to the Politically Correct Thought Police (p. 271) and the Commissioners of Political Correctness (p. 328) abound, reinforcing Barry's view that cultural pluralism as a political strategy is the concoction of a handful of posturing intellectuals who rely on dogmatism to support their cause.

As ever, Barry's strong suit is his talent for articulating the essence of liberal principles and presenting persuasive (although not, in my view, ultimately convincing) arguments to support his case against self-styled liberal and postliberal theories of multiculturalism. A key strength of the book is Barry's knack for demonstrating that often political theorists who purport to amend flaws within liberal theory simply misconstrue or fail to understand what liberal norms, principles, and conceptual distinctions actually entail. For example, the liberal distinction between public and private has, Barry argues, been profoundly misconstrued by critics. Urging us back to John Stuart Mill (whom he discusses at length), Barry persuasively argues that such a division is indispensable for justice.

Barry also unpacks and holds up to critical scrutiny much of the rhetoric of recent political theory that endorses cultural pluralism, sometimes in quite useful ways. His discussion in the final chapter, "The Politics of Multiculturalism," effectively demonstrates the dangers of the presumption that liberal democracies should move toward a conception of the "special interests" of ethnic and cultural minorities, in part because such a conception obscures the ways in which public institutions and resources (like education) are a matter of critical concern for all citizens, regardless of their ethnicity.

Barry's claims that "a politics of multiculturalism undermines a politics of redistribution" (p. 8) and that "pursuit of the multiculturalist agenda makes the achievement of broadly based egalitarian policies more difficult" by "diverting political effort away from universalistic goals" (p. 325) combine to shape a provocative and sobering thesis. The book is, however, a bit short on solutions to the very social and economic disadvantages and inequalities that are, Barry claims, misattributed (by proponents of multiculturalism) to culture and group identity. It is hoped that a follow-up book is in the offing, in which Barry will address precisely these problems from his trenchant liberal social justice perspective.

The Postmodern Marx. By Terrell Carver. University Park: Pennsylvania State University Press, 1999. 240p. \$55.00 cloth, \$19.95 paper.

Ernesto Laclau, *University of Essex*

Two main features of this remarkable book deserve special mention. The first is its attempt to reflect on Marx's texts in the light of present social and political concerns. This is the reason for "postmodern" in the title. The present is not seen by Carver as organized around a unified perspective but as a true plurality: He does not take refuge in any ready-made coherence or in easy labels. The complexities of the present are projected into the Marxian text—or, rather, rediscovered in it—so that they lead to its authentic deconstruction. One ends with the feeling that there is much more to be found in the texts of Marx, despite their anachronisms and limitations, than conventional wisdom would have us believe.

The second important feature is that the Marx Carver finds at the end of his exploration is also plural. A great deal of literature has tried to discover the "true" Marx, but we get from Carver a fascinating unveiling of the multiplicity of the Marxian text, of the various discursive sequences whose unity results from contingent articulations rather than from any underlying univocal principle. Seen from this angle, the work of Marx appears as a sort of microcosm in which we find, in nuce, all the potential and often contradictory trends of the history of Marxism in the century following the founder's death. Being partly inside and partly outside that history, Carver sees this multiplicity of crossroads already operating in the founding gestures of Marx himself and gives a penetrating analysis of its constitutive dimensions.

In the introduction Carver states the different paradigms, both theoretical and political, that have governed the reading of Marx's texts. Later, he successively analyzes the role of metaphor in theoretical writing, the link between Marx's approach and the idealist philosophy immediately preceding him, the shortcomings of the rational choice reading of his texts, the framework of his political economy approach, his conception of communism, the main dimensions of his political theory and strategy, the interpretative strategies to be found in the translations of his work, his relations with Hegelian philosophy, and even those aspects that Marx did not touch: women and gender.

Carver is very much aware of the main contemporary developments in textual analysis: from rhetoric to deconstruction in its Derridean version and from the hermeneutics of a Gadamer or a Ricoeur to the contextualism of Quentin Skinner and the Cambridge School. The traces of these methodological approaches (or, rather, of these reading practices, given that the notion of "method" would be very much contested by some of these currents) can be found everywhere in Carver's book, and their employment is always careful and rigorous. Carver has written a work that can be considered a model of contemporary textual analysis.

Deliberative Democracy and Beyond: Liberals, Critics, and Contestations. By John S. Dryzek. Oxford: Oxford University Press, 2000. 195p. \$29.95.

Bob Pepperman Taylor, *University of Vermont*

John Dryzek identifies the "deliberative turn" in democratic theory in the past decade and wishes to "establish what a defensible theory of democracy would look like" (p. 2). The fundamental features of the theory he develops are: a critical hostility to what he takes to be liberalism's view of democracy as simple interest aggregation; a subsequent defense of

deliberative democracy's educative and transformative potential; and an argument for distancing deliberative democracy from the state and formal voting mechanisms and emphasizing instead the democratic importance of a plural, politicized, and vocal civil society.

In elaborating these ideas, Dryzek hopes to reinvigorate a radical critical theory that he fears has become too closely allied with liberalism in the past few years. This allows him to distinguish, on the one hand, between what he takes to be the full critical potential of deliberative democracy and, on the other hand, conventional liberalism, rational choice theory, and the work of "difference" democrats. Having carved out the terrain of his own theory, he then applies this theory to the consideration of two interesting and important cases: the democratic potential in international politics and the relationship between democracy and Green politics. Throughout, he is deeply concerned with what he calls democratic "authenticity," or "the degree to which democratic control is engaged through communication that encourages reflection upon preferences without coercion" (p. 8).

There is much to like about and learn from this small but densely argued volume. At the most general level, all scholars interested in the relationship between democracy and voluntary organizations in civil society will find important food for thought. Although Dryzek focuses primarily on politicized organizations in civil society rather than the whole range of voluntary associations (Greenpeace, say, rather than Robert Putnam's bowling leagues and choral societies), he makes a strong case for thinking that many such organizations may make their greatest contributions to democratic deliberation by maintaining their independence from the state, by viewing their contribution to democracy as flowing from the deliberation they stimulate in the polity more generally rather than the degree to which they either share formal political power or aim primarily to influence the outcome of elections.

This idea allows Dryzek to offer a novel argument about the role of an international "civil society" in promoting transnational democracy. The model he has in mind is of networks of organizations, perhaps like those working and demonstrating in recent years against free trade, or the universe of international environmental organizations. In the course of making these arguments, Dryzek discusses social choice theory, the recent liberal turn of critical theory (most notably Habermas), and "difference" theory, all of which will be welcomed by theorists in these fields as well as by those involved with debates about "public reason" and the proper constraints on political debate (Dryzek defends a moderately broad understanding of these constraints).

As in most richly argued and thoughtful books, there is much with which to quarrel. I shall mention three of the main issues that may generate discussion. First, a strong case can be made that Dryzek's portrait of "liberalism" is much too narrow and constrained. We should not forget, for example, that one of the most important "deliberative" democrats was John Dewey, perhaps the dominant liberal voice from the first half of the twentieth century in the United States. It is true that for much liberal political theory democracy is narrowly construed as the aggregation of interests. This by no means exhausts the liberal literature or position, however, as works from John Stuart Mill to Amy Gutmann and Dennis Thompson demonstrate. Nor do all liberals "fail to recognize . . . that getting constitutions and laws right is only half the battle" (p. 21)—one need only think of liberal feminism, such as that developed by Susan Moller Okin, to see how unpersuasive this claim is. Critics will suspect that Dryzek's liberalism comes dangerously close to being a straw man. More generously, others may suggest that his constrained

view of liberalism unnecessarily restricts his access to literatures and arguments that would be most useful to his own thinking.

A second worry concerns the connection Dryzek makes between democracy and civil society. He recognizes that many organizations in civil society are neither internally democratic nor representative of majority public opinion, but he nonetheless believes some of these organizations are democratic because they give voice to wrongly excluded groups or interests. By making our understanding of democracy “discursive rather than electoral” (p. 51), Dryzek severs (or at least seriously weakens) the link that legitimizes democracy through the empirical act of voting. In order to evaluate the democratic integrity of a particular discourse or group, he is left only with arguments about authenticity (he holds that authenticity in deliberation requires no more than that communication be based upon generalizable and non-coercive positions) and the substantive degree to which communication aims toward the liberation of legitimate voices. Many readers will suspect that both of these arguments are more complicated, difficult, and ambiguous than Dryzek appears to believe.

A third and related concern is that although Dryzek never holds that liberal democracy and deliberative democracy are incompatible, he obviously emphasizes the latter and discounts the former in his discussion. Although many (like myself) will be persuaded that democracy cannot be reduced to voting, many (like myself) will also think that Dryzek is in danger of losing sight of the fundamental, even primary, importance of voting and elections in any democratic system.

There are other issues, such as Dryzek’s discussion of Green democracy, that could be the focus of an entire review. Even though this book will raise many questions and criticisms, these will reflect not Dryzek’s failure as a political thinker but his strength as an interesting, provocative, and serious democratic theorist.

Enduring Liberalism: American Political Thought Since the 1960s. By Robert Booth Fowler. Lawrence: University of Kansas Press, 1999. 332p. \$35.00.

Nancy S. Love, *Pennsylvania State University*

Has conflict replaced consensus in American political thought since the 1960s? This is the central question of *Enduring Liberalism*, and Fowler answers it with a carefully nuanced “no.” “To grapple with contemporary interpretations . . . there is no substitute for understanding their predecessors. For contemporary views are in constant dialogue with them” (p. 34). He reconstructs this dialogue between the political thought of post-1960s public intellectuals and the general public, on the one hand, and earlier interpretations of American values, on the other.

The first three chapters move quickly from nineteenth-century classic analyses (de Tocqueville and Turner), to Progressive Era conflict interpretations (Charles Beard and Vernon Parrington), to post-World War II consensus theories (Louis Hartz and Daniel Boorstin), and then to challenges to consensus—from the Left, feminists, conservatives, and postmoderns—during and following the 1960s. Fowler claims these critiques have congealed around an “interpretive orthodoxy” that “a consensus does not exist in the United States and never has. A frequent corollary is that 1950’s consensus advocates were political conservatives devoted to the defense of an elitist, inegalitarian past that was not worth preserving” (p. 36).

In the rest of the book, Fowler contests both claims and

offers his own interpretation of American political thought—scholarly and popular—since the 1960s. Finding “no consensus favoring liberalism among contemporary American intellectuals,” he concludes that “diversity is alive and well in American intellectual life, and it is more and more the reality” (p. 61). Public opinion tells a different story, however: “Substantial differences of opinion on basic (and liberal) political values do not exist within the American public” (p. 62).

Fowler examines debates among public intellectuals on three topics: the meaning of community (chap. 6), the struggle for environmental protection (chap. 7), and the decline of civil society (chaps. 8 and 9). His analysis is comprehensive, ranging far beyond standard Right/Left ideological positions. For example, he discusses proponents of the following versions of community: inclusive, national democratic, participatory democratic, small group and therapeutic, spiritual, and traditional. Throughout the book Fowler identifies doubts about liberalism (at least, when defined as radical individualism, individual rights, and/or market capitalism) shared by public intellectuals, despite their different visions of American democracy.

Fowler sets the intellectual debates against the backdrop of a largely privatized American public, a bittersweet triumph of the core liberal principle of individual freedom (chaps. 4–5). He presents the American public as, in Alan Wolfe’s phrase, “one nation after all,” although a nation that lacks robust concepts of democratic citizenship or public life. Instead, a weak consensus limits even the most conflictual policy debates to “the current liberal framework” (p. 245). Fowler’s discussion of how liberal individualism has pervaded private life, especially family and religion, is fascinating, despite his questionable association of privatization with 1960s-style liberation. Racism, he argues, remains the deepest division among American citizens and the greatest threat to national unity.

As even this brief summary suggests, Fowler provides a comprehensive survey of American political thought, including an invaluable bibliography. Yet, breadth occasionally supersedes analytical clarity. For example, the line between public intellectuals and public opinion is less clear and less clearly drawn than Fowler suggests. He defines the former as “a certain kind of reflective participant in American public life” (p. x). His discussions include Robert Bellah, Jean Bethke Elshtain, Amitai Etzioni, Michael Walzer, William Bennett, Seymour Martin Lipset, William Galston, Stephen Macedo, and many, many others. Unfortunately, Fowler also draws on their ideas and surveys to characterize American public opinion. One is left wondering whether Fowler’s claim that the American public embraces liberal values is itself an artifact of the public intellectuals he studies.

The author’s definition of liberalism is also rather ambiguous. Fowler acknowledges that diversity exists not only outside liberalism but also within it: “Liberalism is a complex outlook, after all, with somewhat porous and highly contested definitional boundaries and many internal tensions” (p. 250). When discussing conservatism, Fowler asks: “How much does it illuminate anything to say ‘They are all liberals?’” Yet, he still tends to interpret distinct intellectual positions as variations on liberal themes (p. 123). For Fowler, most feminisms fall within liberalism (p. 131), environmentalism includes many liberal moments (p. 198), communitarians belong to the liberal tradition they criticize (p. 174), and the decline of neoconservatism is a partial victory, since its proponents endorse the economic values of classical liberals (p. 248). The conclusion of Fowler’s story of American political thought is that “much intellectual criticism of liberal

institutions or even liberal ideology—often in language that quite explicitly repudiates liberalism—amounts to much less than it appears” (p. 247).

Early in *Enduring Liberalism*, Fowler cautions that “telling a story and endorsing it have no necessary connection” (p. 23). He also maintains that “at the heart of the rebirth of liberal thought since the 1960s has been the courage to do liberal theory, rather than only tell its story or cautiously reflect on the ideas of others” (p. 57). This book is best read on both levels, as story and theory. In the conclusion, Fowler identifies his personal politics: “part Enlightenment liberal, part Burkean conservative, part Emersonian anarchist, and part religious existentialist” (p. 243). This is telling. Guiding Fowler’s story of American political thought is a desire to keep hope for an enduring—and evolving—liberalism alive. Of course, the question remains for whom liberalism is the best, last hope. The story Fowler tells shows that to this question there are no easy answers.

Negotiating Postmodernism. By Wayne Gabardi. Minneapolis: University of Minnesota Press, 2001. 192p. \$42.95 cloth, \$16.95 paper.

Leslie Paul Thiele, *University of Florida*

This fast-paced tour of the postmodern condition will repay readers who may not have the time for more leisurely reading and reflection. It supplies both a useful guide to the theoretical terrain and a menu of practical responses to the social, political, and cultural challenges that postmodernism presents.

Wayne Gabardi’s thesis is that the polemics of the 1970s and early 1980s that characterized the initial interaction between those modernists indebted to critical theory (such as Habermas) and those postmodernists indebted to Heidegger and Nietzsche (such as Foucault and Derrida) have given way to a period of “critical postmodernism,” marked by dialogue, accommodation, and synthesis. This thesis is not wholly original: It is discussed, or gestured at, in the works of Stephen White (*Political Theory and Postmodernism*, 1991), Richard Bernstein (*The New Constellation*, 1991), and others. Yet, the continuing failure of many modernists to distinguish between radical and evolved forms of postmodernism, dismissing the second on account of the excesses of the first, and the continuing failure of many postmodernists to integrate modern critical theory into their own intellectual enterprise means that these ships continue to pass unseen in the night. Consequently, the project of critical postmodernism receives less attention than it deserves.

Of course, the distinction between radical and critical postmodernism is somewhat contrived. It cannot be located temporally in any precise fashion. Aspects of radical and critical postmodernism are found in both the early and later works of all but the most extreme postmodernists (such as Jean Baudrillard). Still, a drift toward critical postmodernism is apparent, and a book that systematically charts this drift is well warranted.

Negotiating Postmodernism is part survey text, part synthetic research. Gabardi adopts a “profile and critique” approach. The first half of the book mostly provides well-crafted review summaries of the literature along with insightful, if not sustained, critiques. Spanning the intellectual world of postmodernity in less than 100 pages is a monumental feat, which Gabardi performs with admirable thoroughness and clarity. One marvels at the comprehensiveness. Yet, *Negotiating Postmodernism* provides too cursory an assessment of the literature, pitched at too high a level, to be of much help

to those wholly unfamiliar with it. Those more at home with postmodern thought might sift through the review section on the basis of the promissory note that Gabardi issues in the introduction. He will chart practical options for a democratic politics informed by critical postmodern insights.

Gabardi makes a strong case for critical postmodernism as a theoretical and ideological orientation that best captures the sociocultural dynamics of late-modern times. Moving beyond the modernist pursuit of universal principles, he attends to the complex cultural pluralism and fragmented market globalism of contemporary societies. Moderating radical postmodernism, Gabardi refuses to overplay the technological burdens and aesthetic potentials of postmodern life. He is attentive to the merits of transgression in a world of pastoral power, but he also refrains from the dangerous embrace of anarcho-ethical alternatives. He sees himself as a skeptical moderate. Mixing Montaigne with (the later) Foucault, Gabardi molds a hybrid intellectual hero who deftly, if somewhat reactively, negotiates postmodern life.

The political commitments of the postmodern moderate are fundamentally democratic. Democracy, for Gabardi, is essentially about citizens’ struggle for freedom. Yet, freedom today is not to be won in emancipatory revolutions. Rather, it is to be glimpsed through the cracks of disciplinary power, tasted briefly in performative transgressions, and shared tentatively through efforts to forge new ethical relationships of self and other. Gabardi calls for various forms of creative resistance. These aestheticized acts of refusal and self-invention reveal our individual and collective capacity for freedom within an otherwise enveloping neoliberal world of globalized media and market relations overseen by a “techno-oligarchy.”

Foremost among the conditions that would allow the exercise of such freedom is increased leisure. Leisure allows us to take serious pleasure in the life of the mind, body, soul, and community. It is meant to foster cultural creation and critical engagement in civic life. We are urged not to use our extra free time for the increased consumption of commodities and simulated experiences (e.g., television, video gaming, Internet surfing). Apart from the pedagogical force of “leisure studies” to be added to the college core curriculum, however, we are left without much hope that people will opt for civic agonistics and high cultural aesthetics rather than more frequent trips to the mall. There is little reason for optimism. Perhaps that is why Gabardi acknowledges that those who adopt a critically postmodern approach will necessarily remain peripheral in a lifeworld dominated by production and, increasingly, by consumption.

For those who take on the challenge of critical postmodernism, the goal is to fashion the social, political, economic, and cultural conditions that allow fugitive experiences of freedom to proliferate. Despite all the talk of collective efforts, however, the creative resistance and self-care advocated by Gabardi harbor a narcissistic quality. In this respect Gabardi evidences Foucault’s mentorship. Although in debt to Foucault for his genealogical perspective and strategic overview, Gabardi also relies on Danilo Zolo (*Democracy and Complexity*, 1992) for his politicocultural analysis. He accepts Zolo’s chaste vision of postmodern life. Zolo foresees the “Singapore model” for democracy: a prosperous, efficient society of managed consumers living in an “antipolis.” Theorists such as Gabardi and Zolo may point to Singapore as the vision of things to come. But the threat, or promise, of a Singaporean future may seem strangely out of touch, or out of reach, to the 70% of humankind living in substandard housing and unable to read, the 50% suffering from malnutrition, and the 500 million people currently experiencing the

horrors of war, the loneliness of imprisonment, and the agony of torture. If there is a glaring shortcoming to *Negotiating Postmodernism*, it is that abstract theorization of the most sweeping variety takes place in the complete absence of empirical reference, even when practical solutions are proffered.

Gabardi's encyclopaedic effort provides many succulent bones to chew on. Yet, little is discussed in depth or justified at length. One is tantalized but left rather hungry. For instance, we are at a loss to learn, in light of the dilemmas of postmodern life, why or how Gabardi's economic and political proposals should or could become implemented. Frankly, I believe that most of his practical proposals are good ones. But that only means he is, like me, a social democrat informed by postmodern sensibilities. People with a different ideological bent would find the author's refusal to justify many of his proposals rather irritating. They would be nonplussed by Gabardi's abrupt leap from a theoretical amalgamation of Habermas, Heidegger, Arendt, and Foucault to the practical advocacy of a negative income tax, universal child care, and a 32-hour work week.

I am also troubled by the celebration of Foucaultian-style transgression. The problem is that such performative action is too easily colonized in the postmodern world. One need only think of the performers featured on Jerry Springer and other day-time talk shows who stimulate the public's appetite for transgressive spectacles. Nietzsche said that decadence can be defined as the need for greater and greater stimulation to achieve the same level of satisfaction. Performative transgression is a facet of postmodern decadence that the media techno-oligarchy well exploits. I would think that Foucault's dallying with sadomasochism bears the same danger. Bread, circuses, and sadomasochistic gladiators—all available on pay-per-view!

To his credit, Gabardi recognizes the capacity of the postmodern world to colonize the most creative acts of resistance. Its capacity to exploit efficiently (even our subversive) desires and actions, to integrate minds, bodies, and souls into what Heidegger called the *Bestand* or standing-reserve, is perhaps the chief reason to fight for leisure. But this fight might best be waged not by spending more time free of work, but by learning how not to *spend* time, whether one is engaged in work or play, theory or praxis.

Containing Nationalism. By Michael Hechter. Oxford: Oxford University Press, 2000. 256p. \$29.95.

Russell Hardin, *New York University and Stanford University*

Michael Hechter focuses on three puzzles about nationalism (pp. 3–4). Why is it only a modern phenomenon? Why is it more acute in some countries than in others? And can its dark side of horrendous violence be contained? The title suggests that his principal concern is with the last question, but the bulk of discussion focuses on the second. One can readily answer the first question by saying that nationalism is strictly modern because it is about mass mobilization. Hence, it is kin to democracy, revolution, and socialism, none of which could arise in large countries before mobilization was possible. The democratic revolutions in the United States and France were about changing the locus of sovereignty from a monarch to the people. This is not strictly Hechter's argument, but it is implicit in many of his claims, such as that nationalism requires the existence of organizations that work for the national sovereignty of their subgroup (p. 125).

This answer, of course, merely pushes the question back to ask why mobilization began to be possible little more than

two centuries ago. A quick and probably partial answer is that technology, communication, transportation, and aggregation of workforces enabled mobilization as never before. A second quick answer is that Napoleon changed modern warfare and modern states by organizing vast armies, almost all of them composed of conscripts or volunteers rather than mercenaries. (The Romans, Persians, and others had mobilized large armies, but Napoleon was revolutionary in his era.) Military and factory mobilization both tended to produce people who could speak a uniform national language and thereby de facto created modern nations. The first of these nations were driven by territorially inclusive nationalism, as in the cases of France and the United States. But they provoked nationalisms that were culturally inclusive and therefore exclusionary in other respects (pp. 91–2).

Hechter has a different historical account of the reason nationalism arose only recently in Europe. His specific claim is that the world of local control was displaced by the rise of direct rule, of the intrusive state governing a large population (p. 60). When governance was highly local, the very idea of nationalism could not occur to or motivate anyone—it would have no point. State formation in Europe proceeded by the confederation of distinct solidary groups (p. 42). Later, direct rule from the center of such confederated states broke the connection between the nation and the governance unit. Nationalism was therefore a response to growing state capacity for direct rule. Earlier empires had generally ruled indirectly, with governance structures and policies that varied from one group to another throughout the diverse empire.

Hechter's chief answer to the second question is that states have a limited span of control and need to organize more or less federally to manage large populations. The rise of direct rule in large states led to opposition to the center from culturally peripheral groups. In earlier work, Hechter argues that a multiethnic state would be easier to govern because each ethnic group would enforce some behaviors on its members, who therefore would have less energy to spend on more generally directed efforts that might challenge the state (pp. 156–7). Clearly, there is a risk in this arrangement because the subunit itself may organize against the state, especially if it becomes infected with nationalist fervor. In Yugoslavia, the federal organization of politics into ethnically concentrated regions enabled Croatian and Serbian nationalism. But, Hechter supposes, the greater risk is to attempt direct rule that reduces policies to uniformity across a diverse population. He argues that decentralization of government to a cultural minority may give that minority the resources to mobilize protests. Although this may suggest that the move is destabilizing, it may simultaneously undercut demands for sovereignty and, therefore, nationalist fervor (p. 146).

Despite frequent concern with the possibilities for collective action, both in protest actions and in mobilizing nationalist groups, the argument here is almost entirely at the level of the nation and the governing structure, not at the level of incentives or specific individual motivations. As Hechter notes for particular cases, leaders often have motivations of self-interest in gaining leadership positions in a newly created state that is congruent with some nationality, and therefore they may work for the nationalist cause of separate government. The motivations of others in a nationalist movement are merely a desire for policies that differ from the larger state to which they are subject. Hechter's main addition to this simple account builds on social identity theory, as in the work of Henri Tajfel and others, who attempt to explain the commonly spontaneous creation of exclusionary groups (pp. 99–101).

If we are to identify with a group, that group must already

be defined. Groups tend to form around characteristics that are important to individual welfare or around mere location. In social identity theory, individuals identify readily with high-status groups. If they are in a low-status group, from which they cannot exit, they will tend to revalue the group and will identify strongly with it. Hechter argues that in many multiethnic societies there is a cultural division of labor that contributes in just this way to the heightening of national identity, especially for the group that is lower in the hierarchy of division (chap. 6). Such cultural divisions can be broken down by the conditions of urban life, in which internal enforcement by group members against one another is too weak to sustain the division, which therefore must depend on political enforcement (p. 112).

In sum, nationalism is primarily a result of the irritations of centralized direct rule over cultural minorities who seek autonomy, the kind of autonomy they might have had in the earlier era of indirect rule. They may be placated by grants of partial autonomy, as in various devolutions of governmental authority in recent times. And, if we may read between the lines, they are more likely to be placated if their economic prospects are good enough to displace concern with political status.

Radical Enlightenment: Philosophy and the Making of Modernity 1650–1750. By Jonathan I. Israel. Oxford: Oxford University Press, 2001. 810p. \$45.50.

John Christian Laursen, *University of California, Riverside*

This book offers a major challenge to the academic political theory establishment in the United States and United Kingdom. Instead of Hobbes-Locke-Rousseau, the important story is Spinoza-Bayle-Diderot. If you are not teaching Spinoza and his influence in your surveys of early modern and Enlightenment thought, you should be.

This is an epic drama with a cast of dozens. The story opens with Cartesianism and its spread around Europe, with major implications for society, institutions, women, sexuality, and more. Cartesianism is soon replaced by Spinozism, which pushes philosophical radicalism even farther. Important figures making up a new canon include the previously obscure Van den Enden, the Koerbagh brothers, and Lodewijk Meyer. Benedict de Spinoza is the key figure, largely, as Israel argues, because he systematized the radical philosophy advanced since ancient times by less systematic figures, and because he was both vilified and followed by so many. In a nutshell, revelation, a providential God, freedom of the will, and miracles are ruled out on philosophical grounds, and immortality of the soul is denied by a theory that everything is one substance. Politically, this implies secularization, equality, democracy, freedom of expression, and women's liberation.

None of this was accepted quietly. A three-way battle for the hearts and minds of Europe was waged among conservatives, moderate Enlighteners, and radical Enlighteners. Famous names such as Locke, Newton, and Voltaire are only moderates, in Israel's analysis. The radicals are the Spinozists, such as Adriaan Beverland, Johannes Bredenburg, and Balthasar Bekker.

One of Israel's purposes is to push back the accepted dates for the important developments in early modern philosophy and political thought from the high Enlightenment of 1750–1800 to the early Enlightenment of 1650–1750. By 1750, it is argued, most of the work had been done. In the shadow of Spinozism came numerous controversies, from the brouhaha over Bayle's claim that atheists could be good citizens to

Bredenburg's fight with Limborch over the proper relations between reason and religion; from Fontenelle and Van Dale on oracles as political frauds to Leenhof on universal philosophical religion. Not only conservatives but also such moderates as Locke, Leibniz, Thomasius, and Wolff fought rear-guard battles against the growing influence of Spinozism. "Whig history" is a term that means all historical roads lead to the Whigs; here, all roads lead to Spinoza, so this is presumably Spinozist history.

This is cosmopolitan rather than nationalist history. Defying the trend of studying the Enlightenment in a single national context, the book sweeps back and forth across all of literate Europe: from Ireland to Naples, from Sweden to Portugal. A good part of the radical Enlightenment was underground, spread by clandestine manuscripts written by the likes of Boulainvilliers, Du Marsais, and other deists and Spinozists, most often in French. Radical German enlighteners, such as Tschirnhaus, Stosch, Lau, Schmidt, and Edelmann, receive renewed attention here. Vico, Radicati, and Pietro Giannone prove that some Italians were up to date. And Israel shows that Spinozism played a role even in Spain and Portugal.

This reassessment is on the order of the major works of Peter Gay, Quentin Skinner, John Pocock, and very few others. A few years ago, Steven B. Smith (*Spinoza, Liberalism, and the Question of Jewish Identity*, 1997) gave us a fresh reading of Spinoza's political theory. Israel's book sets that theory in context and spells out its implications for the history of ideas over a century and more.

Repeatedly, Israel takes down the inflated reputations of Hobbes and Locke. He cites dozens of sources from the late seventeenth and eighteenth centuries that claim Spinoza raised the real issues, not the English writers. It is indeed remarkable how long it takes for insular and nationalist canons to be challenged. For example, in France *La lettre clandestine* reached its tenth annual volume without any significant circulation among Anglo-American political theorists. For those of us who have been reading a large body of German, French, and Italian scholarship on these trends in the last decade, it is about time that a book such as Israel's finally is issued by a mainstream English-language publisher. Since prestige is so important in the diffusion of scholarship, Israel's position as professor of History at the Institute for Advanced Study at Princeton should add to the conviction created by his arguments.

For political theorists who have little idea about what is going on in the rest of Europe, this volume is a magnificent opportunity to get up to date. What is at stake is the claim, now widely recognized elsewhere, that we moderns are not the intellectual heirs of the courtier Hobbes or the gentry spokesman Locke but, rather, of the former Jewish lens grinder Spinoza and his radical Dutch, German, and French followers.

As with any wide-reaching synthesis, specialists will have bones to pick. Denis Vairasse's *History of the Sevarambes* is described as a "French Spinozistic novel" and dated to 1677, but it appeared first in English in 1675. Israel asserts several times that Bayle was silent on freedom of the press, but what was his famous "Clarification concerning Obscenities" about? Israel claims that freedom of the press was always and only a radical position, but Elie Luzac's defense of it in 1749 was rather clearly a moderate stance.

Specialists will also want to suggest further evidence. For example, Boureau-Deslandes's *Reflections on the Death of Free-Thinkers* (1713) could have been mentioned on page 298. Something could have been made of Martín Martínez in Spain. Israel has materials on libraries, learned journals,

encyclopedias, book catalogs, and other modes of diffusion of ideas, but there is no survey of the recently growing study of correspondence networks. The coda on Rousseau is a bit underdeveloped; much more has been said elsewhere about Spinoza's reception in the period 1750–1800, and one area for future research would be Kant's Spinozism. But any such matters of detail would only confirm the overall message of this book: Major sectors of English-language political theory and history of political thought have been missing a great deal of what was important in the seventeenth and eighteenth centuries, and it can be found here.

Lincoln's Sacred Effort: Defining Religion's Role in American Self-Government. By Lucas E. Morel. Lanham, MD: Lexington Books, 2000. 251p. \$70.00 cloth, \$23.95 paper.

David F. Ericson, *Wichita State University*

Lucas Morel presents an excellent survey of Abraham Lincoln's frequent use of biblical language and allusions. Yet, Morel fails the significance test he sets for himself (pp. 1–2): Did Lincoln frequently use such language merely because it was the most common vernacular of his time; the vernacular with which his audiences would be most familiar? Or did he also frequently use such language because he thought that the right ordering of the relationship between religion and politics was critical to the maintenance of a democratic regime and that he actually had something important and original to say about that relationship?

I agree with Morel that the latter is probably the correct answer; he does not show that it is the correct answer. This is far from a personal failure on his part, as he probably does the best he can with the available evidence. The problem is that so little evidence is available. In essence, Morel stretches that evidence into a set of arguments that Lincoln *might* have made about the proper relationship between religion and politics. He shows considerable ingenuity in developing these arguments, but it must be emphasized that he is the one who has developed them, not Lincoln.

Perhaps a useful comparison is between Lincoln and Thomas Hobbes. In both cases, scholars have engaged in extensive speculation about their personal religious beliefs and whether they were atheists or, at most, tepid theists. In both cases, a lack of evidence has fueled this speculation. The two cases appear very different, however, once we move beyond the question of personal religious beliefs and begin to look at their views on the relationship between religion and politics. Much more evidence is available for Hobbes than for Lincoln. Morel tries to analyze Lincoln's views on the relationship between religion and politics as if Lincoln had written something equivalent to parts III and IV of *The Leviathan*. But of course he did not.

In making this comparison, my intention is not to stress the difference between analyzing the works of a philosopher and a statesman so much as it is to emphasize the difference between analyzing Morel's chosen topic and other possible topics in Lincoln's works. The writings and speeches of Lincoln can bear a fairly high level of analysis on such subjects as democracy and slavery, as has been shown by, among others, Harry Jaffa, who is mentioned so prominently by Morel (pp. ix, 14). They simply cannot bear the same level of analysis on Morel's chosen topic. There is a very good reason that, as Morel claims (p. 11), such a book has never been written before.

The one possible exception to Lincoln's relative silence on the relationship between religion and politics is his famous Lyceum speech of 1838 (chap. 2). Yet, as Morel emphasizes,

the political religion of that speech is not really a political *religion* but, rather, a civil disposition of obedience to law that religion then might be used to foster (pp. 8–9, 14–5, 31–2). Lincoln understands the relationship between religion and politics in this speech quite narrowly. But Morel is also very interested—and claims Lincoln is as well—in that relationship more broadly defined to include the ways in which politics should accommodate religion (chap. 3), in which religion might be misused politically (chap. 4), and in which religion teaches men the limits of politics as well as of religion itself (chap. 5). It is on these more strictly religious topics that Lincoln says so little and Morel says so much.

This gap is especially yawning in chapter 4, which is the weakest of the book. (Chapter 5, which deftly but still too expansively for my taste analyzes Lincoln's Second Inaugural Address, is the strongest chapter.) In chapter 4, Morel analyzes Lincoln's temperance address of 1842 and elaborates one of the major motifs of his book: The abolitionists were Lincoln's exemplar for the political misuses of religion (pp. 9–10, 26, 125–6, 140). Yet, the abolitionists were not Lincoln's explicit targets in this address; self-righteous temperance reformers were. Furthermore, even when the abolitionists were Lincoln's explicit targets, as in his celebrated 1858 campaign debates with Stephen A. Douglas, his attacks seem grounded much more in political expediency than in personal disdain for either the principles or tactics of the abolitionists. However moderate Lincoln's own antislavery principles and tactics may have been, they eventually coalesced with those of the abolitionists (pp. 175–80). There is a large measure of truth to Wendell Phillips's gloss on Lincoln's victory in the 1860 presidential election: "Lincoln is in *place*, Garrison is in *power*" ("Lincoln's Election," in Wendell Phillips, *Speeches, Lectures, and Letters*, 1864, p. 305; emphasis original).

Where does this leave us? Morel provides some very interesting speculations about Lincoln's views on the proper relationship between religion and politics, but he stretches the evidence beyond what it can bear.

Worlds of Difference: European Discourses of Tolerance, c. 1100–1550. By Cary J. Nederman. University Park: Pennsylvania State University Press, 2000. 157p. \$40.00 cloth, \$18.95 paper.

Preston King, *Birkbeck College, University of London*

This book is novel, attending more to the history than to the logic or morality of tolerance. It propounds, against the popular grain, a significant presence for tolerance in medieval Europe. Cases are made for Abelard, Marsilius, and others as significant exponents. The result provides students with an opportunity briskly to explore work too often ignored. If this study hits methodological sandbanks, it is hoped that will not deter others from voyaging in premodern times and in non-European waters.

Nederman takes aim at two key notions: The doctrine of tolerance is exclusively modern, and, more narrowly, tolerance is the lineal progeny of "liberalism." He is right to target the second, but he has invented the first. He is right to counter the view that "the Christian Middle Ages has [sic] *nothing whatsoever* to contribute to our understanding . . . of tolerance" (p. 3, emphasis added). Except that only one of four whom he "counters" arguably takes this view. A traveller who is construed to claim "there is no water *whatsoever* in the desert," is proved wrong by the little rain that will eventually fall. An observer who claims that no medieval writer can "readily" be conceived to oppose tolerance, or that medieval

religious intolerance was the “norm,” is not to be translated to mean: The Middle Ages have “nothing whatsoever” to contribute to tolerance.

That aside, the substantive question must turn not round whether there is any evidence for tolerance in the medieval past but round the type, volume, and significance of that tolerance. The author dismisses the claim by Ole Grell et al. that “religious intolerance was the norm throughout the Christian Middle Ages” (Ole Grell, Jonathan Israel, and Nicholas Tyacke, eds., *From Persecution to Toleration: The Glorious Revolution and Religion in England*, 1991, p. 3). Yet, he later endorses Moore’s view that intolerance was the norm in the Middle Ages: The “decided trend” was the “enforcement of orthodox faith against a range of medieval dissenters.” The “Roman Church . . . pursued a systematic policy of imposing a unified set of Christian beliefs.” The “Church took direct aim at . . . religious difference, heresy, Judaism, intellectual dispute.” One may add that it is questionable how far this intolerance “culminated in the Fourth Lateran Council of 1215” (p. 11). After all, the *Malleus Maleficarum* (1486) was the decisive, authoritative handbook—commissioned by Rome and used all over Europe by Catholics and Protestants alike—governing the examination, torture, and execution of “witches.”

The author’s counterargument to assertions that support the prevalence of medieval intolerance slips from the idea that proponents like Grell and Tierney are simply wrong to the idea that this intolerance “depicts only part of the terrain” (p. 11). Nederman thus hovers between two claims. First, intolerance was not the norm. Second, intolerance was the norm, but not omnipresent: “Not every medieval thinker was entirely comfortable with repression” (p. 25). It becomes the author’s settled view that not everyone, after about 1100, favored torture, forced conversion, and dogmatic theology, or, after 1492, the abject enslavement of the newly encountered “Americans.” This in turn raises the question: What sense of tolerance is being deployed to make this sort of response relevant?

Nederman’s basic stipulation for “tolerance” is the idea of accepting “a multiplicity of ways of life,” a diversity “of beliefs or doctrines” (pp. 1–2). The larger the net, the bigger the catch. If tolerance equals diversity, we are not likely to run short of tolerance, even in the Middle Ages. Nederman does not dispute the persistence of persecution. He says expressly there was persecution, heaps, but that “persecution did not halt dissent.” Theologians did not stop reading Aristotle when the Church ordered them to (p. 15). “Heretics flourished even in the face of inquisitorial procedures” (p. 16). “Excommunication was unlikely to carry much weight [since] heretics were relatively unconcerned” about whether they were killed or not. It “is simply incorrect” to assume, because the Church made “war on heresy,” that it succeeded in “stifling all religious dissent” (p. 17). “Careful investigation of the historical record shows that forms of religious diversity, at an intellectual as well as a practical level, subsisted throughout medieval Europe” (p. 12).

Nederman plainly assumes that, by locating diversity, he demonstrates tolerance. But, of course, diversity no more establishes tolerance than intolerance; diversity is a formal condition for both. When there is no notable diversity, there is no remarkable Other to despise—or with whom to be tolerantly reconciled. Diversity is consistent with despotism; and the failure to extirpate difference does not demonstrate tolerance.

The author is too little attentive to definitions and time-frame. What is true in general is true as well for the particular figures investigated. Consider two of the least familiar of the

writers Nederman treats. First, in the twelfth century William of Rubruck is sent by the King of France to the Mongol court as a missionary. His job is to win over the alien to the one true faith. Instead, William observes an array of ethnic and religious diversity, fully tolerated by the Great Khan, a toleration which is “in many ways a source of frustration to William.” Why? Because there can be no “inclination toward conversion” where the Khan sanctions diversity (p. 57). One may view William as tolerant because he is intelligent and apprised of the impossibility of securing conversions. But, unlike Nederman, one may also take the view, overall, that William is less a significant tolerator than a supple but failed proselytizer.

Second, Las Casas may be Nederman’s strongest case of a significant tolerator, an exponent of racial (not ideational) tolerance, and an interesting contrast to William. But does Las Casas (d. 1566) really belong in this company? Nederman slots him as a “medieval author” (p. 119), as opposed to Bodin (d. 1596), who is assigned to “the Reformation” (p. 36). In fact, they both belong to the Reformation. Is it just to claim that injustice has been done to “medieval writers” when one cites, as a major exemplar of their virtues, a contemporary of John Hawkins and John Calvin—and so dubiously “medieval”?

As for Abelard and Lull, they are primarily concerned with rational and respectful debate among Jews, Christians, and Muslims. The emphasis is on reason rather than revelation. Although rational dialogue possibly “requires that one respect the integrity” of one’s interlocutor (p. 37), it may not. (Cannot firm enemies join in rational debate, as in courtrooms and boardrooms?) For John of Salisbury, “mortals can know very little” (p. 50). But this makes him more a fallibilist than a true skeptic; what he does not doubt is the existence of God and related notions. Marsilius opposes crusading and denies the church authority to excommunicate. Yet, Catholicism is for him the one true faith, and “heretics and other infidels . . . are to be shunned” (p. 79). The best case for medieval religious tolerance may be Nicholas of Cusa, who seeks respect for divergent faiths. Even he assumes rational argument in the end will reveal the superiority of Christianity over all other faiths (p. 88). There is little in any of these men to match the audacity of Bodin’s *Colloquium Heptaplomeres* (c. 1593).

The author’s task would have been more challenging under a tighter, apter stipulation. For example, if we read “tolerance” as “accepting or putting up with items (behaviors) to which we object (disapprove),” then there are three consequences. First, no agent can tolerate everything. For *A* to tolerate *x* implies (i) *A* has the power not to tolerate *x*, plus (ii) *A* is intolerant of whatever undermines *x*, given (iii) that *A qua* tolerator is to be conceived as having the capacity to resist such undermining, else s/he cannot (properly) be said to tolerate. Second, we should expect to find cases of tolerance, both as thought and practice, in any era or region, including medieval Christendom, given a reasonably extensive body of evidence. It is not conceivable that any people or epoch should altogether exclude the possibility of some actors sometimes putting up with behaviors or beliefs to which they object. Thus, the residual questions that historians must answer relate to type, volume, and significance of tolerance. Third, formulated as above, one does not think to derive tolerance exclusively from “liberalism,” any more than from “socialism,” “anarchism,” “Christianity,” or “Islam” (consistent with the position taken by Nederman).

The Play of Reason: From the Modern to the Postmodern. By Linda Nicholson. Ithaca, NY: Cornell University Press, 1999. 179p. \$47.50 cloth, \$17.95 paper.

Eloise A. Buker, *Denison University*

The book is composed of nine essays; all but one have appeared in earlier publications. They have a timeless quality, however, and even readers familiar with them may find a rereading productive, especially in the context of examining them as a body of work.

Section I, which has five essays, examines "Modernity and the Problem of History." Combining a solid reading of political theory from the seventeenth century to postmodern times, Nicholson traces the development of such key concepts as the family, "race," gender, sex, and the body to show how history has shaped the kind of politics we imagine possible. The first essay, and possibly the least insightful, offers a critical review of the analyses of moral development by both Lawrence Kohlberg and Carol Gilligan. The next examines Marxism and draws on Mary O'Brien and Iris Young to illuminate how Marxism can be adapted to examine women's subordination by reframing the categories of analysis to include child rearing and sexual relationships. Nicholson explores the key concepts of consumerism, family, kinship, production, and reproduction and goes beyond dual systems theory. She argues that both moral development theory and Marxist theory fall short in their ability to develop significant cross-cultural analyses.

The third essay examines the ways in which feminist theory has problematized the private/public distinction. Reflecting on the work of Young and Rosalind Petchesky, Nicholson examines how the personal is political to reveal how the economy and state organize family relationships. She explains how liberal theory created a public arena composed of male household heads that wholly excluded women. Maintaining the value of a private/public distinction, Nicholson sees the changes in this boundary as a political process and not simply a historical fact.

In the fourth essay, "Interpreting 'Gender,'" Nicholson questions the distinction between sex and gender by showing how these concepts are mutually constitutive. She argues that radical feminists like Janice Raymond as well as feminists like Gayle Rubin retain a kind of "biological foundationalism" that has some of the political problems present in biological determinism. She suggests reframing the concept of "woman" as a complex term that can serve a coalition politics based on women's differences.

The final essay in Section I is a historical review of how modernity constructs the family. After demonstrating that the so-called nuclear family is a myth emphasized in the 1950s and 1960s, Nicholson argues that understanding families as living arrangements rather than as kinship networks permits feminists to undo the distinction between the traditional family and the alternative family. She maintains that families provide social insurance and that the variety of family obligations suggested by cross-cultural analyses offers a broader understanding of family. The argument is persuasive, but she fails to show how this offers new ways of viewing the family as a political institution that shapes public life.

Section II, composed of four essays, examines "Postmodernism and the Problem of Connection." The first essay, published in 1988 and coauthored by Nancy Fraser, is philosophically dated. It was important in initiating dialogue about postmodernism among feminists, but the issues raised have been more elaborately developed in subsequent works, including an important book Nicholson edited, *Feminism/*

Postmodernism (1990). For example, postmoderns have become much more articulate about the way in which politics is a part of their analysis; so the point that postmodernism lacks social criticism and is therefore politically "anemic" no longer holds (p. 100). Furthermore, this essay skirts the central epistemological concerns raised by postmoderns, although Nicholson does address some of these in "Bringing It All Back Home." In this essay, the one not previously published, she argues for a context-dependent understanding of reason based on pragmatism. She deals with the problem of relativism and shows the importance of social theory that acknowledges the limits of history and culture. In the conclusion of this essay, however, she compares philosophy to religion and suggests that "salvation does not coexist well with diversity" (p. 128). This seems to be an unnecessarily limited view of salvation, whether it is constituted by either a religious or a philosophical discourse.

The third essay in Section II offers a lucid argument about the limits of Charles Taylor's politics of recognition but acknowledges the strength of his critique of liberalism. The final essay takes on the issue of emotion in public spaces and argues for a balance between emotion and reason as the basis for politics. Emphasizing the work of Freud, Nicholson maintains that the psyche needs to be taken into account as a factor in public life. She does not draw on either Jane Flax's work in the psychoanalytical or Judith Butler's work on the psyche, *The Psychic Life of Power* (1997). Nicholson's work is part of the same conversation, and it might have been useful to draw these thinkers into the discussion (pp. 156–61).

"Bringing It All Back Home" comes closest to fulfilling the promise of the title, "the play of reason," but Nicholson does not discuss "play," and one wonders how this title emerged and what it means to her. Placing these essays in the context of her current thinking would have underscored her point about the importance of paying attention to context and would have offered a view of how she understands the connections and disconnections in these earlier essays.

These important essays would serve well to introduce graduate students to some key issues in political theory that emerged in the United States from 1980 to 1999. The historical insights are especially relevant for those engaged in the history of modern political thought and its connection to postmodern concerns. Because the language of the text draws from the work of key philosophers rather than offering concrete examples, the book may not be useful for undergraduates. Scholars unfamiliar with postmodern political theory will find this a lucid introduction to some of the central issues. Certainly, political theorists will want to have this volume, as it offers ready access to the important contributions Linda Nicholson has made to political theory.

The Values Connection. By A. James Reichley. Lanham, MD: Rowman & Littlefield, 2001. 304p. \$35.00.

Michael P. Federici, *Mercyhurst College*

The connection between morality and politics is the core of political theory. The founders of political philosophy, Plato and Aristotle, demonstrated the indissoluble nexus between the life of the soul and the life of politics. Political theory was engendered as a response to the spiritual and political decline of Athens. Plato and Aristotle diagnosed the spiritual corruption of Athens and provided a prescriptive response to it. Since then, political theorists and social scientists have tried to determine to what extent ethical behavior and moral principles matter to political and social order. Whereas the

ancient and Judeo-Christian tradition recognized the moral foundations of politics, modern thinkers such as Machiavelli and Hobbes depreciated the importance of transcendent moral values to the formation of a just political and social order. With some exceptions, modern political theory has attempted to transform the foundations of politics. Rather than a transcendent foundation for political order, modern thinkers have founded politics on self-interest, power, liberty, and natural rights.

A. James Reichley sides with the ancient and Judeo-Christian tradition in the debate about moral values and politics. He is primarily concerned with determining which moral values are necessary for a free society. He identifies ten essential values for the creation and maintenance of a free society: the unique value and significance of each human life, the rule of law, continuity between individuals and the historical human community, moral equality, social justice, popular sovereignty/majority rule in a constitutional framework, tolerance of different behaviors and beliefs, honor, compassion for the troubled and needy, and moral realism. A free society is impossible unless these values are accepted as moral ideals. He also explains why six particular "isms" (e.g., egoism, collectivism, monism, absolutism, ecstasism, civil/secular humanism) are destructive to a free society. In short, they fail to balance order and liberty. They either represent excessive individual freedom or excessive state authority. Moreover, they do not balance individual rights and social responsibility.

The bulk of the book is expository. It describes the six inimical ideologies in a textbook or encyclopedic fashion and sketches their theoretical development. Whereas these ideological systems are insufficient, Reichley determines that "transcendent idealism," because it embodies the ten essential values, is the appropriate moral foundation for a free society. Transcendent idealism is the "source for moral regeneration and guidance in the twenty-first century" (p. 173). It is a value system primarily shaped by the classical and Judeo-Christian cultural tradition, although its characteristics can be identified in the traditions of Buddhism, Confucianism, and other non-Western cultures. It accepts the existence of original sin and consequently the fallibility of human institutions. Because human institutions are fallible, individuals need freedom to question and challenge them.

The efficacy of transcendent idealism stems, in part, from its continuity with constitutional democracy. Reichley believes that constitutionalism evolves from Judeo-Christian insights that form the foundation for free societies, such as the unique and equal value of each human life and the rule of law. He associates a wide range of thinkers and artists with transcendent idealism, including the Hebrew prophets, Sophocles, Aeschylus, Augustine, Aquinas, Luther, Calvin, Rembrandt, Locke, John Adams, Jefferson, Jane Austen, Matisse, and Walker Percy. Explicitly excluded from the list are Plato, Hobbes, Rousseau, Hegel, Marx, Rorty, and Rawls. The compatibility of ideas represented by Reichley's list of transcendent idealists is not self-evident. He fails to address important differences between such thinkers as Aquinas and Locke on property or Locke/Jefferson and Madison on representation. A broad net is cast in defining transcendent idealism that obscures important theoretical differences between specific thinkers.

Reichley's argument hinges on his definitions of key concepts. What he means by "transcendent" or "justice" matters more than the categories (e.g., transcendent idealism) used to construct his argument. At first glance Reichley, a senior fellow at the Public Policy Institute at Georgetown University and former Brookings Institution fellow, appears to be

making a conservative argument, that is, liberty and justice would be more secure in America if genuine religion and the Judeo-Christian tradition exerted more cultural influence. He is concerned that mainstream religion in America is on the decline and being replaced by secular (civil) humanism. Without connections to religious institutions, Americans will lose a sense of and commitment to the values that keep a free society vibrant. He rejects the moral relativism of the post-modern Left and subscribes to a theory of moral values that recognizes the transcendent as the necessary ordering force for a free society. He argues that transcendent idealism "finds no route to either freedom or justice that does not pass ultimately through reverence for God's grace and love" (p. 175).

Arguments like this are common in recent scholarship. The idea that the transcendent is the source of existential, political, and social order is central to the works of such seminal scholars as Leo Strauss, Eric Voegelin, Irving Babbitt (to name a few), and their followers. The argument for a restoration of genuine religion in American culture has been made by Richard John Neuhaus, James Davison Hunter, William Bennett, and a host of neoconservatives. The argument for a restoration of genuine spirituality as the cultural impetus for a renewal of the American political tradition is commonly found in such journals as *First Things*, *Commentary*, *Modern Age*, and *Humanitas*. A growing and diverse body of scholarship argues in one way or another that a free society depends on attunement to transcendent reality. Upon review of this literature, however, it becomes apparent that what each author means by moral universality and terms like "transcendent" varies significantly. Placing Reichley's conception of universality in one of the variegated schools of thought that argue for a restoration of the classical and Judeo-Christian tradition is not clear-cut. In most instances, however, his argument can be classified as neoconservative as opposed to paleoconservative.

Although Reichley covers significant historical and theoretical material, ultimately his argument is based on social science studies and surveys; he does not make a philosophical argument. An example of his reliance on social science is his use of Guenter Lewy's work to demonstrate an inverse relationship between religious activity and criminal behavior. He recognizes that the social, cultural, and political benefits of Judeo-Christian values to a free society are due to its insights about the nature of reality. But the book neither searches for spiritual insight and growth nor provides a conception of the ethical life.

Reichley tries to convince readers that transcendent idealism is responsible for the formation and maintenance of free societies, but he treats transcendence in a rather abstract way. He is vague about the philosophical basis for ethical responsibility. His argument is justified by social pragmatism. The values of transcendent idealism are best because social science data verify lower rates of crime, divorce, illegitimate births, abortion, and other social maladies among those who accept those values. It does not occur to Reichley that spiritual insights can actually undermine social and political order.

With these reservations aside, *The Values Connection* does address an important problem. It provides a good survey of the classical and Judeo-Christian tradition and uses it to discuss a pressing contemporary problem. Those interested in the contemporary relevance of old ideas and traditions will find Reichley's book useful.

Obligations of Citizenship and Demands of Faith: Religious Accommodation in Pluralist Democracies. Edited by Nancy L. Rosenblum. Princeton, NJ: Princeton University Press, 2000. 438p. \$72.50 cloth, \$19.95 paper.

Surviving Diversity: Religion and Democratic Citizenship. By Jeff Spinner-Halev. Baltimore, MD: Johns Hopkins University Press, 2000. 246p. \$36.50.

Robert K. Fullinwider, *University of Maryland*

These two books—one a collection of essays and the other a sustained treatise—dwell on the problems posed for liberal theory and democratic practice by religious commitment. Both supply a rich menu of arguments and insights.

Let us start with a central dispute. Michael McConnell, in his contribution to *Obligations*, insists that the terms of Supreme Court jurisprudence and the norms of liberal political/legal culture treat Americans with strong religious convictions as second-class citizens. Religiously based arguments are unwelcome in the public square, and religious believers must bend their practices to fit the law, not the other way round. These sentiments echo a common complaint that liberal “secularism” has achieved de facto “establishment” in American law, public institutions, and elite culture. In place of the privatizing imperatives of that establishment, McConnell offers an alternative vision, one of “religious pluralism”: All citizens are free to make, accept, or reject public arguments without limitation (p. 104), and the law bends to accommodate believers’ needs, imposing “the least possible violence” on their religious life (p. 103).

McConnell comes under attack in *Obligations* from two directions. On the one side, Graham Walker derides his pluralism as just another form of liberalism, subject to the same complaints McConnell makes against the secularist version. What is needed, in Walker’s view, is something altogether different from liberalism, namely, some form of open constitutional establishment of church. Such an establishment would be more honest for being above-board and, properly limited, should prove more supportive of real religious diversity than the prevailing covert secularist establishment (pp. 117–21).

From a different direction, Amy Gutmann takes issue with McConnell’s one-sided treatment of religious freedom and argues instead for “two-way protection.” Separation of church and state (not separation of religion and politics) is necessary to protect the church from state interference, to be sure, but equally, she claims, to protect citizen and state from inappropriate political aggrandizement by the church. Furthermore, proper separation “denies religious, anti-religious, and nonreligious citizens alike a *general* right—based on conscientious objection—to disobey laws that serve legitimate public purposes” (p. 143, emphasis original). The state can grant exemptions in cases in which doing so does not create “runaway precedents” that subvert legitimate public law (p. 144), but except in special cases, those in which nonreligious parallels do not exist, exemptions based on conscientious scruples should be extended to the religious and nonreligious alike, writes Gutmann.

Nancy Rosenblum further explores this last issue by considering a piece of important legislation, Title VII of the Civil Rights Act of 1964, which gives “preferential treatment” to religious associations by letting them discriminate in their employment practices (p. 172). The Supreme Court, in *Corporation of Presiding Bishop v. Amos* (1987), construed this exemption broadly to allow a religious organization to condition employment on a religious test no matter how remote the employment from any actual religious function.

Although Rosenblum is keen to protect the autonomy of private associations from demands for “convergence”—demands that their internal organization and norms mirror and support the larger public values of democracy, equality, and nondiscrimination—she sees the *Amos* rule as too generous. Religious associations ought to show some nexus between job and religious function before they are allowed to fire a jobholder on religious grounds (p. 181). Among the reasons against handing them such a blank check is the serious threat posed to the religious freedom of individuals adversely affected by discrimination (pp. 183–6).

Jeff Spinner-Halev, in *Surviving Diversity*, takes as his point of departure multiculturalist arguments for valuing and supporting cultural diversity. Both liberal and “nonliberal” forms of these arguments actually narrow the room for diversity, he contends (the principal targets here are the views of Joseph Raz, Will Kymlicka, Nancy Fraser, and Iris Young). These arguments are especially impervious to the “difference” constituted by conservative religious belief. As an antidote, Spinner-Halev offers a more religion-friendly account of liberalism. The liberal state, he contends, should prove a commodious place for those religious communities that organize themselves around ideas of obedience to authority and submission to revealed truth rather than around ideas of autonomy, individualism, and self-discovery. The liberal state can leave such communities to their own ways because a healthy liberal “mainstream” culture provides an “exit” option. Giving this option substance, however, may require some intervention by the state (pp. 49, 63, 73ff).

A liberal society should do more than tolerate the religious. It should take steps to include them in the public square and make efforts, in schools and in law, to exempt them from requirements of which they conscientiously disapprove of (pp. 107, 136–9). Like Gutmann, Spinner-Halev favors extending accommodations and exemptions, where warranted, to all, not just the religious, who are burdened in conscience by public law or policy (p. 207ff).

This brief survey does not begin to do justice to the depth, subtlety, complexity, and power of the works under review. They belong on the bookshelf of anyone intellectually engaged by the recent renewal of interest in church-state issues. (Add to that shelf, as well, Brian Barry’s new book, *Culture and Equality: An Egalitarian Critique of Multiculturalism*, 2001, whose extensive treatment of accommodation and lengthy reflections on the same court cases discussed by Spinner-Halev and the essayists in *Obligations* make it a forceful and provocative companion.)

These two books show that the broad terms of liberalism do not dictate particular settlements of the religious question. History and circumstance must be given their due, and the “inescapability of judgment” (Rosenblum’s phrase, p. 189) means there are no formal or mechanical solutions to the problems at the heart of church-state relations. A great virtue of *Obligations* is three rich essays whose focus lies outside the United States. Gary Jacobsohn, Yael Tamir, and Martha Nussbaum ask us to consider religious freedom in the context of contemporary India and Israel. Even if we begin with broadly liberal values, the circumstances in these two countries may lead us nevertheless to favor legal restrictions on religious speech and to countenance forms of religious establishment. Indeed, in Western European countries with histories far different from that of the United States, we may find religious establishments conducive to liberty, as Graham Walker hopes (see Nussbaum, p. 361).

Walker puts forward a provocative, self-styled nonliberal ideal of a “mixed constitution” that establishes a church but protects religious liberties. But if a constitution genuinely

protects religious liberties, is it not a liberal one? Moreover, must not the “mixed constitution” ideal attune itself to historical realities just as liberal separationism must? The secularist who insists that the Lutheran establishment in Sweden must go shows no less imagination than the Walk-erite who thinks separation of church and state can be cleared off the decks in the United States and replaced by a religious establishment. Still, as an ideal to toy with intellectually, what is it about the “mixed constitution” that ought to attract the reader to it (besides its openness)? According to Walker, “unlike liberalism, it sanctions truth-seeking . . .; it does not insist that truth-seekers can never find any truth deserving public validation” (p. 121).

This contrast, whether accurate or not, forces some interesting issues onto the table. First, there are questions for critics of the “secular establishment,” like Michael McConnell and Stephen Carter, who would measure U.S. policy by whether it hinders or supports religious pluralism. Why should a particular religious believer, committed to a set of specific theological truths, find religious pluralism in any way attractive? That is, why should he welcome the spread of religious error and theological confusion? What can an orthodox Catholic, say, find attractive about a constitutional order that facilitates the spread of Mormonism, Santería, Bahá’í, the Unification Church, Scientology, the Gospel of Wealth, mushy New Age “spiritualities,” and the theologically anemic but media-savvy nondenominational “Christian” ministries mushrooming everywhere?

Second, there is a question for Walker. If seeking the truth and having it publicly validated is important, can the mixed constitution be indifferent to which church is established? Otherwise, to meet his ideal it would be sufficient for secularism to come out of the closet, divest itself of the garbs of neutrality, announce its own truth, and openly luxuriate in its already existing establishment in the United States.

Third, is liberalism really indifferent to seeking and publicly validating the truth? If so, what accounts for the abundance of public research universities that populate this continent, and the reams of government reports, administrative rules, and legislation that put the public stamp of approval on some views over others? Liberalism is hardly hostile to the truth, although it does dance a fine line about some truths. If we cannot live together as a people under certain descriptions (e.g., “God is one substance, not three”), we had better not stake our constitutional order on them. Still, each of us is committed to the truth or falsity of these descriptions, so must we not find our public order diminished in some way if it cannot acknowledge vital truths? Or is there a higher level truth we might all share that makes such an order morally valuable even as it allows—from our various perspectives—error to rub shoulders on equal terms with truth? If the reader is tired of watching John Rawls’s high-wire act on these matters, she might look, then, at another very different, yet surprisingly congruent, piece of acrobatic artistry, *Dignitatis Humanae* (Holy See, 1965).

Modernity’s Wager: Authority, the Self, and Transcendence.

By Adam B. Seligman. Princeton, NJ: Princeton University Press, 2000. 141p. \$27.95.

John R. Hall, *University of California, Davis*

Who will read Adam Seligman’s important new book, and what will they make of it? The answers to these questions may reflect the very issue that *Modernity’s Wager* examines, modern individualism and its discontents. Seligman writes with great thoughtfulness and erudition, at the borderlands

of social theory, political philosophy, theology, and the history of religion, and he constructs a theoretical lingua franca to draw these disparate discourses into conversation with one another. Yet, he takes the substantial intellectual risk that his argument will not receive due consideration from the very audiences whose attention to it would most advance public discussion: rational choice theorists, Enlightenment liberals, and postmodern relativists, all of whom, in different ways, will question his claim for the importance of external sacred authority. In the end, Seligman proposes skeptical toleration as the most promising basis for restoring a couplet of transcendent authority and authentic (“constitutive”) selfhood that has been lost under conditions of modernity. Yet, for his proposal to gain a hearing will require more than toleration skeptical of its own verities: It will depend upon readers’ willingness to engage ideas far outside—and alien to—their own frames of reference. They will find the effort worthwhile.

Modernity’s wager, as Seligman describes it, is a bet that the autonomous individual of liberal thought can be maintained on the basis of a transcendental philosophy of natural rights, without recourse to transcendental sacred authority, and that the internalized moral selfhood of this autonomous individual, and collectivities of such individuals, will be sufficient for an organization of the social that will not succumb to totalitarianism of either a Jacobean or fundamentalist persuasion. This wager is a risky one, in Seligman’s view, because the moral calculus of liberalism’s autonomous individual cannot be assured in the absence of a well-defined connection of the self to authority that has an external, sacred basis. The wager of modernity stands to lose both the individual self as a fully moral being (rather than merely a utility maximizer) and the possibility of a communal social order that has any moral basis to it beyond what Durkheim called the precontractual principles necessary to maintain a world in which contracts among free individuals undergird social life. The wager, Seligman maintains, morally impoverishes both individuals and the modern social order, and it leads to a paradoxical “politics of recognition,” but in the absence of any moral basis for community that would provide an “authoritative basis of value” on which to base recognition (p. 120). More important, it leaves modernity open to dangerous reversals that potentially threaten the very individual freedoms that liberalism is meant to protect.

In the end, *Modernity’s Wager* must reconcile external sacred authority with the contemporary realities of religious pluralism. To do so, Seligman makes his own wager with the sacred. Recognizing that sacred authority has itself often been used in coercive ways (in effect acknowledging a certain importance of internal freedom of religious conscience that he otherwise declaims as a threat to sacred authority), Seligman shifts from affirming the formal necessity of sacred authority to embracing a particular content of sacred authority that might offer a workable resolution to modernity’s dilemma. This resolution requires modern liberals and humanists to take matters of religion seriously, rather than dismiss all religion as fundamentalist and regressive. (Indeed, as Seligman observes, the resurgence in religious faith of many different types creates facts on the ground that cannot be ignored.) The moral failure of modern individualism, combined with authoritarian and fundamentalist threats, requires proponents of both reason and of faith to act with greater humility and skepticism concerning their own authoritative claims. “What is demanded, then, is a midpoint between nihilism and postmodern relativism, on the one hand, and absolutist claims of both faith and reason, on the other” (p. 129).

The overall argument is developed on multiple and interlocked fronts. In chapter 1, Seligman contends that the social sciences—from rational choice theory to Durkheim, from Hobbes to the symbolic interactionists—believe a (perhaps inescapable) modernist bias in their impoverished conceptions of the self and in their divorce of social order from a theory of community. A richer conception of the self as a moral evaluator rather than simply a utility maximizer can only be achieved on the basis of a transcendent sacred authority (chap. 2). This authority not only substitutes judgments about the deeds that fulfill or fail to fulfill moral intentions in the face of Fortune but also produces community via ritual. By these two developments authority makes possible the symbolic linkage among persons that sustains individual moral effort and collective sentiments of shame and pride, sentiments that reinforce the individual's connection with transcendent moral authority (chap. 3). But a theological and intellectual history is held to demonstrate that the particular trajectory of modern individualism out of the Reformation eclipses external authority and community (chap. 4), and this leads to the challenge of sketching a preliminary alternative consolidation of moral individualism and sacred authority by way of humility, skepticism, and toleration (chap. 5).

This deeply and thoughtfully argued book can easily be read as a jeremiad about the “specialists without spirit, sensualists without heart” (in both life and utilitarian theories) that Max Weber anticipated as the end cultural product of modernity. Seligman follows in a venerable if highly varied tradition of scholars who seek to reckon the fate of community and the prospects of faith, scholars such as Robert Nisbet, Peter Berger, Robert Bellah and his associates, and Robert Wuthnow. Like the best of jeremiads, this one rises above mere nostalgia. Seligman seeks a way out. Readers may disagree with him, and even when they share his conclusions, they may doubt the paths of reasoning by which he reaches them. Yet, arguments and doubts will not undermine the significance of this book. Seligman's achievement is to consider the prospects of individualism and the transcendent sacred through a deep reading of social theory and a charting of theological history over the *longue durée*. He is thereby able to articulate the sources of modern liberalism's crisis in a particularly incisive way.

The challenge that now faces Seligman, and us, is to explore the potential for skeptical toleration (and mutual engagement) of faith and reason. Whether even a fuller explication of this project will offer a way to revive transcendent sacred authority seems to me doubtful. But perhaps the effort itself is more important than achieving its goal. Indeed, the nature of toleration suggests that the goal is unreachable. If we are to have a world with one contradiction, this may be the one worth having.

NOMOS XLII: Designing Democratic Institutions. Edited by Ian Shapiro and Stephen Macedo. New York: New York University Press, 2000. 331p. \$50.00.

Matthew Festenstein, *University of Sheffield*

In keeping with the tradition of this distinguished series, the editors have assembled a team of very fine scholars, representing a range of perspectives from across the discipline and beyond, to explore an important and current aspect of political theory. In the introduction, the editors locate the impulse to think about institutional design against the background of cynicism and disgruntlement about the trajectories of the newer democracies, as the euphoria surrounding

democratic revolutions of the last decade in Europe, Latin America, South Africa, and elsewhere recedes. Yet, there is no “transitology” here (apart from a rude remark from Philippe Schmitter, p. 244), and neither the problems nor the examples picked out by the contributors are peculiarly the property of this wave of democratization. These recent experiences of constitution- (and institution-) building have undoubtedly sharpened the sense that the implementation of democratic values is not merely a straightforward task of “engineering”; other experiences loom larger here, such as worries about campaign finance in the United States, new forms of nationalism, the growth of global and regional forms of governance, and postwar decolonization. Of course the question of institutional design, democratic and otherwise, is very much part of canonical political thought from *The Laws* onward.

The essays in the first part explore some familiar anxieties about the quality of democratic deliberation and decision. Ian Ayres makes an ingenious case for anonymity in political campaign finance in the United States, which is rebutted by Geoffrey Brennan and Alan Hamlin. John Ferejohn discusses the difficulties of institutionalizing deliberation in sociologically plural polities. Philip Pettit is also concerned with the quality of democratic decision making and offers a fresh and very helpful statement of his republican thesis that democracy, when understood as aiming at the articulation of common interests, needs not only to create openings for the expression of interests but also to institute devices to challenge putative expressions of the common interest.

The contributions in the next two parts range more widely. Iris Marion Young builds on a critique of liberal nationalism, as found in the work of David Miller and others, in order to reconcile the emphasis on group identity that has been such a central feature of her own philosophy with an institutional cosmopolitanism. Since the concept of a nation presupposes that of a sovereign state, she argues, nationality cannot be used as a criterion by which to justify a group's claim to statehood. Furthermore, the very principle of state sovereignty lacks legitimacy, and we should create a system of global governance that both supersedes the nation-state and, intriguingly, devolves powers to “self-determining peoples.”

Russell Hardin acerbically argues that this last move reproduces the difficulties with nationalism, which erroneously ascribes interests to groups and is itself a fundamentally “corrupt” ideology: “Liberal nationalism is too good to be true, and ordinary nationalism is too true to be good” (p. 206). Robert Post argues that Young misconceives sovereignty, which should not be understood as a state's capacity for discretionary action but as the authorizing source of collective agency. Transnational institutions do not threaten sovereignty in this sense but may constitute its legitimate embodiment. This is appropriately followed by a contribution from Philippe C. Schmitter on how to remedy the democratic deficit in that *sui generis* institution, the European Union.

Critics of rationalism in politics may wonder if the schemes for the design of democratic institutions are not in any case at the mercy of relatively intractable social forces. The third part of the book consists of a pithy and pessimistic account by Donald L. Horowitz of the prospects for successful constitutional design in ethnically divided societies, with responses from Brooke Ackerly and Philippe Van Parijs. Horowitz offers an excellent summary of his criticisms of the consociational model of ethnic accommodation as well as a summary of his case for constitutions that attempt to lure votes across ethnic boundaries, in effect trying to give an institutional boost to the development of Lipset's cross-cutting cleavages. But this is framed by a pessimistic argument that deep

division militates against the acceptance of any “centripetal” constitutional package; indeed, “substantial compromise on a plan to facilitate interethnic compromise decreases the likelihood of interethnic compromise in the operation of the plan once adopted” (p. 272).

Ackerly responds that neither ethnic cleavage nor institutional design is as important as Horowitz makes out. In particular, she argues (with reference to Nigeria and Malaysia), economic inequality and the immense temptations of corruption are more significant. Van Parijs makes the case that, when the conditions are right (as in the Netherlands and Belgium), a pragmatic mixture of different schemes for ethnic compromise can work. In a rejoinder, Horowitz refuses to take solace. The *ceteris paribus* conditions put forth by Van Parijs in the case of Belgium are just an appropriate set of cross-cutting, nonethnic cleavages that compensate for the feebleness of institutional measures; when these do not exist, as in Northern Ireland, the institutional measures are precarious. (The recent general election results from that province, it should be said, bear out Horowitz’s pessimism about the dynamics of implementing constitutional accommodations.) This is a very illuminating exchange, with (like several contributions to this volume) classical resonance.

Designing Democratic Institutions is a rich and diverse volume. It should be of interest not only to political theorists but also to readers who skip to the reviews on American politics, international relations, or comparative politics. It inevitably offers what Brennan and Hamlin call a piecemeal rather than a synoptic view of its subject, which is vast, and any short statement about democratic institutions is bound to raise a host of vulnerable causal claims and contestable moral assumptions. But this book offers an excellent survey of some current concerns, should encourage concrete thinking from political theorists, and is strongly recommended.

Thomas Hobbes and the Political Philosophy of Glory. By Gabriella Slomp. New York: St. Martin’s, 2000. 194p. \$65.00.

Ted H. Miller, *University of Alabama*

All too often, Thomas Hobbes thought, we are undone by our pride. He used reason, cajoled, and even threatened his audiences to persuade them to accept this sad fact and to accomplish the still harder task of bringing them to swallow the strong political medicine he deemed necessary for correcting our condition. Hobbes himself, however, was not immune from the disease of pride. He noted our inability to acknowledge that others might be wiser than ourselves, but he sardonically recommended we take this inability as evidence of the equality of human wits: “For there is not ordinarily a greater sign of the equal distribution of anything than that every man is contented with his share” (*Leviathan*, ed., Edwin Curley, 1994, p. 75). The problem, of course, is that we demand of others that they see us in the same light as we see ourselves. Readers only vaguely familiar with his mathematical squabbles are aware that Hobbes felt he failed to receive due recognition for his talents from Britain’s mathematical authorities. He protested that these same authorities were all too satisfied with a deficient share of wit. According to Hobbes, we are the inheritors of a mathematical tradition that went mad shortly after it rejected his contribution.

Gabriella Slomp suggests that Hobbes’s views on glory changed between the earlier statements of his political doctrine (*Elements of Law* and *De Cive*) and the later (*Leviathan* and *De Homine*). According to Slomp, the earlier Hobbes

made vainglory (a person’s inflated estimation of self-worth) the root cause of man’s pleasures: Our various passions are rooted in the satisfaction we derive from seeing ourselves honored. By the time he wrote *Leviathan* and *De Homine* (a work rightly emphasized in this analysis), Hobbes no longer gave vainglory all the credit. It emerges as one passionate motivation among many. Glory-related passions remain, but admiration of novelty, compassion, and love are given new emphasis (p. 91). Hobbes also offers greater acknowledgment that some individuals are not driven by vainglory. This is concomitant with another change. Hobbes’s subject, in Slomp’s view, becomes more malleable and educable, although this claim in chapters 7 and 9 and in the conclusion is a bit muddled by other elements of the argument. Slomp suggests that Hobbes may have shifted his views on vainglory to better match the experiences of readers so that they might be educated by the *Leviathan*; she also recognizes that the nonglory seeking individuals were always a part of Hobbes’s world view (pp. 88–90, 145). Slomp further acknowledges that Hobbes does not require all individuals in the state of nature to be vainglorious. Rational actors know that some are so inclined, and this is enough to transform the state of nature into a state of war we would wish to escape.

This is a very wide-ranging book, not always unified by a larger argument or restricted to the topic suggested in its title. It often reads like disparate pieces cobbled together. Slomp begins by defining herself (overzealously) against Cambridge School interpretation, but she nevertheless sustains a largely textualist and analytic treatment by drawing heavily upon sources one might expect to find in a Cambridge School interpretation. Most notably she draws upon Hobbes’s correspondence (particularly Leibniz’s observations) and his unpublished (until 1976) arguments against the scholar and author of *De Mundo*, Thomas White. The arguments against White are nearly the exclusive source for citations to Hobbes’s writings in Slomp’s sprawling last chapter and are used extensively throughout.

The use of neglected sources should be welcome, but one might have hoped for some thoughts from the author concerning the standing of the work on White as a contribution to an interpretation of Hobbes’s contemporaneously published philosophy and a discussion of White’s book. Since one of the most provocative pieces on Hobbes’s views on honor and glory was written by the historian Keith Thomas (in *Hobbes Studies*, ed. K. Brown, 1965), who linked Hobbes with reform-minded aristocrats anxious to check the excesses of their contemporaries, the decision to forgo a historical treatment has its costs. Thomas’s claims are not addressed, although some of the older omnibus targets (such as MacPherson) are singled out for attention. In spite of Slomp’s great emphasis on “political geometry,” no attempt is made to approach Hobbes’s extensive writings on geometry.

The book is divided into two parts. In Part 1, major efforts are devoted to illustrating the centrality of Hobbes’s concerns over glory. Slomp traces the connections between his view of its dangers and Thucydides’ treatment of the subject in *History of the Peloponnesian War* (a work Hobbes translated early in his publishing career) and illustrates the above-mentioned shift. On the way to Thucydides, comparisons are drawn between Hobbes’s views on glory and other well-known sources, including Aristotle’s “honor,” Biblical pride, and Bacon’s *Essays*. Slomp shows parallels between Thucydides’ vainglorious peoples and Hobbes’s vainglorious individuals, but she finds an ultimate divergence: Hobbes is more optimistic about the prospects for using fear to reunite

societies torn apart by the children of pride. Part 1 also includes a criticism of Carole Pateman's reading of Hobbes.

In Part 2, Slomp begins a more analytic or choice-theoretic treatment. Game theoretic approaches to Hobbes by Gauthier, Hampton, and Kavka are criticized, but so is the criticism of such approaches by Patrick Neal. Combining what she takes to be the best from both camps, Slomp redescribes the situation of rational actors in the state of nature: They are not involved in a prisoner's dilemma but are locked in an insoluble game of chicken (Who will veer off the road first?). Slomp's "Chicken with Spices" leaves players paralyzed between the demands of vainglory and the "incommensurably negative value of death."

This is followed by a chapter entitled "Hobbes's Impossibility Theorem." The reference is to Arrow's theorem by the same name, although Slomp's proof, unlike Arrow, involves the strategic interaction of game players. "*In a state of unrestricted liberty (UL), for men who regard death as the greatest evil that might occur to them (S) and know that other people, too, are concerned about their survival but might be glory-seeking (G), it is rational (R) to decide to kill, which decision, because of the equal dangerousness and vulnerability of men, is against reason (non R)*" (p. 147, emphasis original). The only way out is to introduce, *ex machina*, the all-powerful sovereign (nullifying the condition of unrestricted liberty).

Moreover, it is Slomp's innovation to suggest that rational actors living in the state of nature are themselves incapable of resolving the problem. As such, one must stand back from the conflict, in the calm of an already peaceful society where one is able to realize the need for such as sovereign. With this assertion, Slomp joins a growing crowd of Hobbes scholars who argue that the works are best seen as directed toward the minds of persons already within societies, rather than as advice for those living in the state of nature.

Slomp's game-theoretic treatments are constructed from at least one opportunistic reading of Hobbes's work. The assumption of "equal dangerousness" is drawn, in part, from Hobbes's claim that we are all equals primarily because we are capable of killing one another. In Slomp's formulation, however, equality in physical conflict must always conclude with both parties destroying one another (p. 137). Life in the state of nature may be solitary, poor, nasty, brutish, and short, but is it so short that we must assume every physical conflict between every two individuals always results in the death of both? The very idea that some persons seek glory through the subordination of others suggests that Hobbes knows that some individuals walk away the victor from physical conflict, even when the other side resists. Slomp hypothesizes that the state of war in the state of nature may be nothing more than a "war of minds" (p. 146), but that does not solve the problem for her game participants, who must mull the possibility of something more immediately treacherous. Such a fighter's life may not last long in comparison with Hobbes's own, but contra Slomp, it surely need not end as soon as a potentially equal opponent puts up a fight.

The Comedy and Tragedy of Machiavelli: Essays on the Literary Works. Edited by Vickie B. Sullivan. New Haven, CT: Yale University Press, 2000. 246p. \$40.00 cloth, \$18.00 paper.

Markus Fischer, *Georgetown University*

In addition to his well-known political tracts, Machiavelli composed a variety of comedies, poems, and familiar letters.

Literary scholars have studied these works for some time and, more recently, applied their craft to his political writings as well (e.g., Albert Russell Ascoli and Victoria Kahn, eds., *Machiavelli and the Discourse of Literature*, 1993); in their view, Machiavelli took politics to consist of rhetoric and wrote accordingly. The present volume constitutes an important rejoinder to this endeavor, insofar as its more significant essays assume that Machiavelli was a political philosopher and that his literary creations apply his political theory to the private sphere.

More explicitly, the theme that holds this collection together is the question of whether Machiavelli's literary effort tends more to comedy or tragedy. According to Arlene Saxonhouse, comedy breaks down traditional boundaries in order to open the distinct and peculiar to the common and to show the fluidity of all forms (pp. 57–8). Thus, Machiavelli's literary works are comic because they reveal the private life of respectable men and women to be a game for sexual gratification, wealth, and reputation, with success going to the clever—those who know how to assume the most effective speech and guise.

Mandragola, for instance, is a play that celebrates the fraud by which Callimaco exploits the aged Messer Nicia's desire for sons in order to bed his young wife Lucrezia, whose morals are corrupted by her ambitious mother and a churchman who takes the biblical story of Lot's rape by his daughters to imply that good effects excuse evil means, in evident reflection of Machiavelli's infamous advice to princes. Moreover, the fact that Lucrezia is so pleased with Callimaco's embrace that she makes him her lover and gets her grateful husband to offer him a room in their house suggests that a new order, useful and satisfying to all parties, can be constructed by letting go of moral scruples, just as Machiavelli's republic maintains internal stability by abandoning the classical idea of a community of virtue and joining nobles and commoners in the mutually rewarding pursuit of glory and empire by external conquest.

According to Harvey Mansfield, *Mandragola* portrays the moral failing of Lucrezia as the failing of Christian morality in coming to terms with the wicked deeds that many good outcomes in practice require (p. 22). Intriguingly, Mansfield raises the further possibility that Messer Nicia allowed himself to be cuckolded in order to gain respectability as the progenitor of a family, in imitation of Junius Brutus, who played crazy and used the rape of the Roman Lucretia to gain glory as the founder of the Roman republic (p. 28). In the words of Robert Faulkner, Machiavelli replaced classical comedy, which drew ethical lessons from the laughable, with a utilitarian rhetoric that constructs associations for the mutual satisfaction of desire—be they households or modern societies (pp. 53, 56).

The absence of ethical seriousness from Machiavelli's literary oeuvre also disqualifies it as tragedy in the classical sense, as Faulkner further suggests (p. 35). Tragedy signified to the ancient Greeks that the hero's unflinching pursuit of one good inevitably negated another, owing to the fundamental incoherence of reality. In the Renaissance, however, the tragic merely meant that great success was characteristically followed by abject defeat, due to man's subjection to the whims of Fortune, as Ronald Martinez avers (pp. 110–1). Accordingly, the ruin of once glorious Italy by the French and Spanish invasions constituted a tragedy, and recounting it in grave and poetic terms, as Machiavelli did, made the writer a tragedian (pp. 102–3, 116–9). But Martinez's more significant contribution consists of interpreting Machiavelli's comedies as parodies of tragic episodes from antiquity, which, in turn, suggests Italy's calamitous decline. In particular, whereas the

tale of the Roman Lucretia—who committed suicide after being compelled by Sextus Tarquinius to yield her body, which prompted the outraged Romans to overthrow the Tarquins and establish a great republic—qualifies as tragedy, the story of the Italian Lucretia amounts merely to comedy (pp. 105–7). In other words, the tragedy of Italy consists of no longer being capable of the tragic catharsis that alone could renew its body politic.

According to Michael Harvey, a similar mingling of comic and tragic strains is evident from Machiavelli's poem *L'Asino*, a tale of a man's descent into the forest of Circe. Usually, we take Machiavelli's view of sexuality as one in which men of *virtù* subdue women by fraud and force, as expressed in his famous metaphor of the prince who conquers Fortune by beating and striking her down. In *L'Asino*, however, the hero's *virtù*—in particular that of his sexual organ—shrinks before the terrifying power of Circe, revealing the anguish felt by men who must forever prove their manliness in a solitary and agonistic world (p. 133). Even more uncharacteristically, Machiavelli responds to this vulnerability with a tale in which the hero regains his *virtù* through the loving embrace of a kind and understanding woman, which offers a rare glimpse of a world of mutuality and friendship (p. 129). Here, Harvey adds an important nuance to our understanding of Machiavelli's psychology.

Susan Meld Shell rounds off this political interpretation of Machiavelli's literary works by drawing out their propositions on language. Accordingly, linguistic boundaries are shaped by political forces, as conquerors impose their tongues on the provinces they settle (pp. 83, 98). Language itself operates by the principles of politics as understood by Machiavelli. Action words are more "powerful" than articles and nouns (p. 83). Native tongues need to dominate imported words to remain beautiful (p. 87). Italy's common language does not consist of the abstract universals imagined by Dante but of the impure mix of concrete particulars used by the Italians (pp. 86–7). And the value of an idiom depends not on its aptness for expressing the worthiest things but on its practical effectiveness—hence comedy's use of coarse speech to instruct the many in useful things (p. 92).

The contributions by Mansfield, Faulkner, Saxonhouse, Shell, Martinez, and Harvey make this book highly commendable to those seeking to fathom the literary form of Machiavelli's thought. This form consists of both comedy and tragedy, shorn of their traditional moral lessons. As comedy, it reveals the noble to be a mere appearance of the vulgar and applauds the clever manipulation of appearances. As tragedy, it glorifies men who do battle with Fortune even though she will eventually ruin both them and their orders. In the final analysis, Machiavellian comedy and tragedy are but two expressions—light and grave—of a reality devoid of purposive, ethical order. Moreover, the literary form may have lured Machiavelli into giving voice to a dread that this worshipper of manly action would otherwise not admit: that a world of endless strife and contingency is a cause for despair, rather than celebration.

Democracy and Association. By Mark E. Warren. Princeton, NJ: Princeton University Press, 2001. 265p. \$55.00 cloth. \$17.95 paper.

Nancy L. Rosenblum, *Harvard University*

Mark Warren joins the lively discussion on voluntary association with a circumscribed purpose: to identify the specifically democratic effects of associations in the United States and to create a typology of groups. His goal requires him to

perform two preliminary tasks. First, he distinguishes the effects of association from a group's formal purposes and members' intentions; associations formed for a variety of goods may have democratic effects. Second, he sets his project apart from work concerned with the broader moral or socially integrative effects of association, ranging from diffuse social capital to individual virtues, as well as from theories of association that focus on political devolution and subsidiary forms of self-government. It should be said that, as in most of this literature, the political conditions that shape the ecology of associations are eclipsed, and the focus is on the one-way effects of association on political life.

What counts as a "democratic effect," how we value it, and what forms of association contribute to democracy all depend, of course, on underlying democratic theory. Warren adopts a moderate version of Habermas's spheres, distinguishing among government, market, and "life-world," which includes the social structures that support "public spheres" of opinion formation. Consensual associational relations based on neither power nor money can be found in every sphere, but the organizational form, voluntary association, which is based on social attachments and normative resources and is intrinsically communicative, dominates civil society. This framework allows the author to set aside government and business. (And with them the workplace, which is a contentious point, Warren recognizes, since work is arguably the venue that affords most adults the experience of collective action and deliberation under conditions of ethnic and racial heterogeneity.)

The Habermasian framework also emphasizes social differentiation, the "migration" of collective action beyond states and markets, and the multiplication of politically relevant arenas. This underscores Warren's argument that democracy depends on a number of different and independent associational functions that need to be carefully disaggregated; the key is associations' "contributions" to democracy in the plural. The Habermasian framework puts a premium on associations that connect individual life-worlds to public spaces, encourage collective judgments, and create the networks of communication that comprise "public spheres." That said, the usefulness of Warren's typology does not depend on subscribing to Habermas's conceptualization of spheres of collective action, and the author does not employ "public reasoning" or "deliberation" as philosophical terms of art.

Very briefly, Warren identifies three categories of democratic effects. One set constitutes "public spheres" of democratic judgment. Another is personal developmental effects: a sense of efficacy, political skills such as negotiation and coalition-building, civic virtues, and capacities for deliberation, among others. The third category is effects that underwrite democratic institutions, including representation, legitimation, or resistance. Against this background Warren works out a typology of associations based on a number of identifiable factors that tend to produce one or more of these democratic effects.

The principal point flowing from this typology is that trade-offs among democratic effects are inevitable. For example, associations that put a premium on the absence of internal conflict are unlikely to develop members' political skills. Associations from which exit is costless are unlikely to experience pressures from members for "voice" and are "lethal to critical skills." Warren challenges facile assumptions about voluntary associations as sites of unrestricted dialogue. Associations that foster deliberative capacities are less likely to develop the strong consistent public positions necessary for advocacy and other vital contributions to

dialogue in the public sphere. The intimate social context of associations such as universities makes it more difficult for them to address issues of race and gender as compared to market-oriented firms, for whom resolving these issues is instrumental to impersonal goals. One last example: Associations committed to providing the good of “identity” have a low capacity for cooperation and coordination.

The rewards of system are apparent here; Warren’s typology illuminates the terrain of associations in the United States and the limitations of much that is written about it. The costs emerge if we look for substantive insight into the democratic effects of specific associations. Political parties are arguably the most important intermediate democratic association, and Warren types them as vested groups oriented toward “the medium of coercive state power,” indeed, as “arms of state power.” This characterization holds for parties in government but does not point up the face of parties as voluntary associations. Their unique role in accommodating interests, framing issues, altering the parameters of discussion in conjunction with other groups, setting agendas,

and shaping public opinion (to say nothing of their distinctive part in political representation) is given short shrift in this and probably in any typology. Moreover, political parties function both as associations and as forums for other groups. So long as “public sphere” is defined as “institutionally unbound” and without powers of collective action, our understanding of parties will be truncated.

Unquestionably, Warren’s democratic categories and associational types will help structure and guide the work of political theorists and social scientists. His categories are exhaustive but not so detailed as to overwhelm the bounds of useful typology. At the same time, the grain of generalization is not overly coarse, and the result is a nuanced map of the associational terrain. The author’s constructive spirit and attention to the real world of groups are apparent throughout. And Warren’s quiet insistence that against the onslaught of legalism, bureaucracy, and markets associations alone preserve voluntary, social forms of collective action gives this excellent work its moral grounding.

American Politics

In the Web of Politics: Three Decades of the U.S. Federal Executive. By Joel D. Aberbach and Bert A. Rockman. Washington, DC: Brookings Institution Press, 2000. 230p. \$42.95 cloth, \$17.95 paper.

Karen M. Hult, *Virginia Polytechnic Institute and State University*

The legitimacy and the capacity of an administrative state are not easy to demonstrate or justify in the United States, where administration emerged from and is enmeshed in a political system animated by separated powers, checks and balances, individual rights, and skepticism about government. Aberbach and Rockman focus on the “turmoil and controversy” that have swirled around the U.S. national executive branch since 1970, and they sketch the contours of and explore the reasons for the ongoing debate. At issue, they claim, is both a “quiet crisis,” which reflects the allegedly deteriorating quality and morale of federal career executives, and a “noisy crisis,” which involves the uncertain responsiveness of civil servants to the demands of elected officials. More recently, and amid ongoing criticism, the federal government has belatedly joined many states and localities, as well as other countries, in an effort to “reinvent” government. Aberbach and Rockman find scant empirical evidence of either “crisis” or of reinvention-induced declines in careerist morale. Moreover, they contend, much of the persistent concern about the capacity, accountability, and responsiveness of federal administration is better understood as disagreement about the proper scope and activities of government.

The empirical core of *In the Web of Politics* is the analysis of findings from structured interviews with senior careerists and subcabinet-level political appointees in domestic policy agencies during the Nixon (1970), Reagan (1986–87), and first Bush (1991–92) administrations. The three cross-sections permit the authors to examine the extent and nature of change in the backgrounds, attitudes, work experience, and perspectives of these officials across three Republican presidencies as well as before and after the emergence of the Senior Executive Service, which was created by the 1978 Civil Service Reform Act. Aberbach and Rockman also use the

interview data as well as other documentary evidence and material from Office of Personnel Management (OPM) surveys to explore the empirical bases for claims about the representativeness, responsiveness, quality, morale, and adaptability of the federal executive.

The quiet crisis (a term the authors adopt from the 1990 report of the National Commission on the Public Service, popularly called the Volcker Commission) revolves around the ability of the career service to attract and retain qualified individuals and the morale of those officials. Of ultimate concern is the effect of personnel quality and morale on government performance. Aberbach and Rockman find little evidence of a clear drop in competence. Despite the admitted limitations of the indicators of “quality” in their survey (primarily, number and kind of advanced degrees, and the prestige of undergraduate and graduate degree-granting institutions), many may find the favorable comparison between senior governmental and top for-profit sector officials surprising and, perhaps, comforting. At the same time, there has been “a modest overall drop in morale and a more dramatic drop-off in the intensity of [job] satisfaction” among careerists (p. 82). This decline, along with the persistent “guildlike features” (p. 75) of the career service (such as promotion from within, careers within single agencies), may contain warnings about the longer term adaptability of the federal executive to increasingly complex and volatile policy environments.

Far noisier is the purported crisis of unresponsiveness. Both Nixon and Reagan entered office assuming that they confronted a mostly hostile executive branch. Twenty-five years ago, Aberbach and Rockman observed (“Clashing Beliefs within the Executive Branch,” *American Political Science Review* 70 [June 1976]: 456–78) that Nixon probably was correct: Most of the civil servants they interviewed in 1970 reported being both Democrats and “liberal.” Nixon’s response, of course, soon came to be labeled the “administrative presidency,” as he experimented with strategic placement of loyal appointees, reorganization, and impoundment to try to boost bureaucratic responsiveness. Reagan brought many of his predecessor’s objectives to fruition. The administration placed Republicans in top career positions, especially in “controversial” departments, such as Health and

Human Services, Housing and Urban Development, and Education (pp. 107–8). Consistent with a more systematic and centralized personnel process, political appointees were more likely to be Republican and more conservative than they had been in the Nixon era.

Despite George H.W. Bush's less ideological posture, he also strove to shape congressional influence on the executive through, for example, the use of signing statements (p. 38). Meanwhile, senior careerists during the Reagan and Bush years reported having fewer contacts with Congress, interest groups, and the public as well as somewhat less perceived influence (p. 115). Whether the three presidents' efforts to boost responsiveness, the growing conservative "Zeitgeist" (p. 169), or the decreasing attractiveness of federal government service to Democrats and liberals was most responsible for these shifts is impossible to tell from the Aberbach and Rockman data. Nonetheless, overall, the executive has been notably "responsive to a changing political environment and . . . instruments are in place to promote responsiveness" (p. 127).

The relatively small number of executives Aberbach and Rockman were able to interview (never more than 228, in 1986–87) militate against much fine-grained analysis of similarities and differences among the agencies sampled. Even so, some further disaggregation by agency mission or primary unit task (e.g., regulation versus grant oversight) would have been useful. More important, it is not fully clear how far the findings can be generalized. Robert Durant, for instance, calls into question the responsiveness of federal officials who work outside Washington (*The Administrative Presidency Revisited*, 1992). Similarly, those employed in national security or foreign policy departments (such as the Central Intelligence Agency or the departments of State and Defense) or units (e.g., the international trade division in the Department of Commerce) may have quite different profiles from those in more "domestic" agencies.

Aberbach and Rockman conclude by examining the somewhat different issues of responsiveness that emerged from efforts in the Clinton era to "reinvent" government. This time, the demands of "customers" rather than elected officials (or their appointed agents) were to be heeded. *In the Web of Politics* pays special attention to the National Performance Review (NPR) and the Government Performance and Results Act, and it capably surveys a broad range of praise and criticism the initiatives have elicited. Most helpful perhaps is the volume's placement of reinvention efforts in the context of the broader new public management movement, which has been influential in such countries as Australia, New Zealand, and Great Britain. Still, the tendency of many new public management (and reinvention) advocates to rely on an idealized "business model" rather than actual business practice in their analyses might have been noted. Just as earlier efforts to "reform" U.S. national administration spotlighted value conflicts, so, too, Aberbach and Rockman contend, has reinvention. Not only is NPR ambiguous about accountability relationships, but also issues of political responsiveness—"to which principals in a system of separated and divided powers are bureaucratic agents to respond?"—remain "unresolved and contentious" (p. 157).

In the Web of Politics synthesizes important findings on the senior federal executive with other studies of the challenges of governing in the United States and other advanced industrialized countries. The work crystallizes the key normative and analytical dimensions of efforts at administrative redesign and oversight, and it underscores the profoundly political foundations of such initiatives. Throughout, the authors' reflections on continuities and changes in the U.S.

(domestic) executive are nuanced, insightful, and firmly anchored empirically.

Aberbach and Rockman make a persuasive case that reformers' persistent focus on "management improvement" is likely to have "at best marginal effects" on executive branch capacity (p. 176). "If the U.S. system produces complexity, contradiction, bloated or inefficient programs, and unusually high degrees of restriction on managerial latitude, that is primarily the product of politicians" (p. 188). Whether and how that system copes in an era of shrinking federal government employment, expanding shadow government, more complex and challenging tasks, and continuing public demands is a central concern of governance in the twenty-first century.

Campaign Reform: Insights and Evidence. Edited by Larry M. Bartels and Lynn Vavreck. Ann Arbor: University of Michigan Press, 2000. 259p. \$69.50 cloth, \$25.95 paper.

Paul Gronke, *Reed College*

Actors, evidence, and standards are the three watchwords of this volume on campaign reform edited by Bartels and Vavreck. Campaign reform is unlikely (and very possibly unnecessary) unless some shared understanding of each watchword is reached among politicians and policymakers, academic observers, and journalists.

The book is an outgrowth of a Pew-funded task force on campaign reform and contains seven articles along with the task force report. Topics range from the content of campaigns (particularly the consequences of negative advertising), to the nature of media coverage, to voter reactions (learning, turnout, declines in diffuse system support). All authors keep a primary eye on applying political science theories and evidence to practical questions of campaign reform. Does negative campaigning benefit candidates? Should media outlets provide "free time" to competing candidates and conduct "ad watches"? Can campaigns be conducted in such a way so as to reinvigorate, rather than depress, public interest, information, and enthusiasm about politics? Each article can be read as standing alone (perhaps too much so; see below), but each revisits the issues of actors, evidence, and standards.

Public commentary on campaign reform typically focuses on candidates and their financial statements, but this volume identifies two other actors to consider: journalists and the citizenry. Marion Just and her coauthors, for example, show how the objective of candidates (win office) in many ways runs counter to those of the news media (gain viewers, curry influence). Larry Bartels and Lynn Vavreck, in separate articles, note that the desire of voters for easily accessible information, interesting campaigns, and distinctive policy positions can run contrary to the aims of candidates, who may be interested in maximizing votes, blurring distinctions, and otherwise wooing the median voter.

An even larger set of actors could be included. Vavreck speculates how campaigns in general affect the legitimacy of democratic leadership more generally, and Buchanan worries that campaigns may reduce civic engagement by the nonvoting public. Any campaign reform must address the complex interaction of multiple actors, all involved to various degrees in political campaigns.

Another notable contribution in this volume is the careful consideration and presentation of high-quality social scientific evidence. Multiple authors (Bartels, Geer, Vavreck, and Shaw), using separate data sets and different techniques, show that negative advertising has anything but a negative

effect. The verdict is far more mixed. Similarly, although we tend to blame candidates for negative campaigns, the evidence in this volume is consistent and relatively overwhelming: Journalists are far more blameworthy for highlighting attack ads, fostering cynicism, and generally accentuating the negatives (see especially the contribution by Just et al.). This theme runs through many of the essays. The careful use of evidence is what one would expect from these scholars and is a refreshing antidote to emotional commentary on this issue. Also, the presentations are accessible to a wide range of audiences.

Finally and most important, many of the authors agree that the strong reformist movement of the past few decades lacks a clear set of standards. What is wrong with the current political climate? What would constitute successful campaign reform? These are deceptively simple questions, but the devil is in the details.

For example, is it more important that voters become highly informed (see Bartels, Geer, and Vavreck), or is a greater concern the corrosive effect of campaigns on public attitudes about the political system (see Buchanan)? Negative ads seem to help voters discriminate among competing candidates (Geer, Bartels) but also reduce faith in government (Buchanan). Journalists are well positioned to inform the public about misstatements by candidates, but in doing so they may publicize the very negative ads that the “ad watch” is meant to expose (Jamieson and Waldman).

Bartels and Vavreck do a fine job of highlighting conflicting standards. By far the best and most thought-provoking treatment of the various and competing standards for campaign reform, however, is found in the task force recommendations. This chapter should be required reading for any course on campaigns and elections, and it is a useful blueprint for both reformers and scholars.

What is missing in the volume? Most important, it lacks both a historical and political science context. Many of the media and voter trends noted in the book first appeared in the 1960s, accelerated into the 1970s, and flattened out thereafter. This was a time when many large-scale changes occurred in American politics, including the rise of candidate-centered elections. Perhaps all we are observing is a maturation in candidate-centered elections. If so, reforms are virtually impossible without completely changing the political system.

With respect to political science, a few authors note the conflict between the desire for voters to discriminate among candidates and the pressure for candidates to blur differences and master the art of ambiguity. More attention could have been given to models of candidate behavior, especially Downsian convergence and how it relates to campaign reform.

An introductory chapter could have provided a larger context. A unified bibliography might have been desirable, since many authors cite the same works, although the current arrangement allows teachers to use a single article or a few in class. The index could be more extensive. These weaknesses indicate a light hand by the editors but are not serious flaws.

Polarized Politics: Congress and the President in a Partisan Era. Edited by Jon R. Bond and Richard Fleisher. Washington, DC: CQ Press, 2000. 226p. \$36.95 cloth, \$24.95 paper.

Michael L. Mezey, *DePaul University*

In introductory American politics courses, when we get to the section on political parties, we often contrast the American

parties with their more disciplined European counterparts. Under a European model, copartisans unite behind a coherent set of party principles and policy proposals that they present to the electorate, and party leaders in both the executive and the legislature exercise significant authority. In the following class, we then explain why such a system does not obtain in the United States: A federal system produces decentralized party organizations rooted in states and localities; single-member districts; the electoral connection and the localism that comes with it; the committee and seniority systems in Congress; the power of individual U.S. senators; and the constitutional system of separate institutions sharing power.

As often happens, the conventional wisdom that we teach our students is modified by events. In this instance, beginning in the mid-1970s and continuing apace through the end of the century, the political parties in Congress came to take on some of the traits of the unified political parties typical of European parliamentary systems. At least that is what is suggested by rules changes giving more power to congressional party leaders and by rising party unity scores on roll-call votes.

The seven excellent essays in *Polarized Politics* are all directed at the central question of the causes and consequences of the increasing level of party unity in Congress and its implications for presidential-congressional relations, especially in the context of divided government.

Gary Jacobson shows that voters increasingly connect their party identification with their own ideology. As John Aldrich and David Rohde demonstrate, this is reflected in Congress, where a growing ideological consensus within both parties has created the conditions for institutional reforms that facilitate party government. Tim Groeling and Sam Kernell argue that, despite these changes, American political parties remain limited in their ability to communicate a consistent message through their rhetoric. This in turn limits their capacity to deliver on their policy commitments. They conclude that increasing party coherence has been achieved primarily through negative commentary about the opposition rather than through a positive enunciation of the party's positions. This point is reinforced by Kathleen Hall Jamieson and Erika Falk, who document an increase in the level of incivility among members of Congress that coincides with increased partisanship. This incivility reached its peak in the 104th Congress, when the atmosphere was somewhat reminiscent of the more rough and tumble environment of the nineteenth century.

The implications of all this for presidential-congressional relations are a bit ambiguous. George Edwards and Andrew Barrett demonstrate that presidents can get their proposals on the congressional agenda, but final passage is largely determined by whether their party controls Congress. This is nothing new; the size of the president's majority in Congress has always been the best predictor of presidential success. What may be clearer is that in the current era of more intense partisanship, the majority itself, rather than its size, may be the key determinant.

Turning to divided government, Barbara Sinclair shows that such an environment does not preclude presidential success. In the six years of divided government during the Clinton administration, however, the tactics used by the president and by a more empowered set of congressional leaders changed to reflect the higher level of partisanship. In particular, the level of hostility between the two camps, as demonstrated in the Jamieson-Falk chapter, suggests the hard-ball turn that American politics took during that period.

In a concluding essay, the editors point out that the more

homogeneous parties one finds in Congress today are the result of fewer cross-pressured members. This in turn is the result of more homogeneous congressional districts and the party realignment in the South since the advent of voting rights for African Americans. Endorsing the theory of "conditional party government" developed by Aldrich and Rohde, Bond and Fleisher conclude that the current situation may be subject to change if the condition of more homogeneous preferences within the party majority changes. But the higher level of partisanship in Congress has not reduced the responsiveness of legislators to their constituents, or eliminated party mavericks who can swing the outcome of a close vote, or produced gridlock, even in situations of divided government. Although the interchanges may be nastier and the policymaking process more difficult, policy decisions are nonetheless reached.

As is the case with many studies of this sort, the Senate is not seriously considered in most of the pieces, and this is a serious omission as we seek to understand the implications of changes in partisanship for congressional-presidential relations. Left unanswered, as well, is how much of the decline in civility and the intensified partisanship is attributable to the personalities involved. Certainly, the visceral hatred of Bill Clinton that the Republican leadership displayed had as much to do with his personal characteristics as with his essentially centrist ideology.

Anthologies are always chancy, but this one is a must read. The chapters are of high quality, present original data and original arguments, and fit together nicely. As a whole, they demonstrate that changes in partisanship and party organization do matter in Congress, at least in the House, and they provide an important counterpoint to theories that tend to minimize the role of party in favor of an exclusive focus on individual legislators and their preferences.

Advancing Public Management: New Developments in Theory, Methods, and Practice. Edited by Jeffrey L. Brudney, Laurence J. O'Toole, Jr., and Hal G. Rainey. Washington, DC, Georgetown University Press, 2000. 320p. \$65.00.

William Eric Davis, *University of California, Riverside*

What is the role of public managers, and do they matter? How should we study public management? Which strategies for reform and innovation hold potential for improving the success of public agencies? Which frameworks hold the most potential for advancing the field of public management? These four questions represent the organization and themes of the book reviewed here.

If addressed competently, each question can advance public management's ability to deliver on promises, improve service, and better satisfy public demands. More important, we will know whether success is being achieved in those respects. The book's greatest strength lies in its diversity of approaches, methods, and frameworks. The opening chapters are devoted to the role of managers and illustrate the difficulties of measuring the effect of management. The most appealing aspect of Part I is that it seeks a greatly expanded definition of management, which for many years was simply considered something that managers did. This created conceptual and empirical difficulties. Part I also supports the notion that there can be no single recipe for management success. Much depends upon the external and internal contexts.

Laurence O'Toole sees a need for a broader but workable

definition of management. For a broad definition to be workable, we must assume that a manager "does something" that gives an indication of the limits of organizational structure and the limits of employees. Following up on that, Anne Khademan asserts in chapter 3 that cultures tend to be deeply ingrained and difficult to shape, which limits the effect of management. Her suggestion is to acknowledge even more directly that culture is part of an organization's institution, which implies that neoinstitutionalist literature may hold the best promise for future advances. Her chapter reflects the somewhat pessimistic assertion that culture does not so much represent a management tool as offer a way to understand the institutional context. That is, the culture will suggest whether there is a possibility for organizational governance or change.

Thomas Hammond and Jack Knott use spatial theory to specify the conditions under which public agency managers can move policy in their preferred direction. Spatial theory requires simplifying assumptions that open it to attack for being unrealistic, thus impractical. Nonetheless, the goal of Hammond and Knott is to show the conditions under which an independent regulatory agency head can have influence in one or two policy dimensions, given the ideal preference positions of, respectively, the House, Senate, and president. Using their version of McKelvey's "chaos theorem," (Richard D. McKelvey, "Intransitivities in Multidimensional Voting Models and Some Implications for Agenda Control," *Journal of Economic Theory* 12 [1976]: 472-82), they do a good job of illustrating how agency heads, under certain narrow conditions, can achieve policy change closer to their preferred ideal point. They outline the environmental conditions and the particular individual skills that allow such an effect. An agent must be smart enough to know the size, shape, and location of the unbeatable "core" of policy space as well as when it is advantageous to preempt the House, Senate, or president by taking initiative. S/he must be an effective persuader and know how to frame issues to maximize policy advantage. In sum (although the authors do not put it this way), the agency head must know how to manipulate.

Hammond and Knott present a purely formal theory, so they do not indicate the proportion of managers who meet the specified assumptions or do the things they list as important to success. Thus, they do not (and do not seek to) offer a definitive answer regarding whether managers matter. They simply note when the potential exists. Furthermore, since the manager in their example heads an independent regulatory commission with rulemaking power, s/he can act almost unilaterally and force the House, Senate, and president to meet the specified assumptions and seek to persuade the agency head, rather than vice versa. Perhaps a more dynamic model is needed that incorporates the actions and reactions of all four major participants plus some component to represent the limitations on the agency head imposed by other endogenous and exogenous factors (culture, structure, and so on).

Part II is devoted to methodological issues and addresses how we should study public management. Often in statistical analysis we run the models, report the results, and walk away from the project without closer inspection of outliers. The chapter by Kenneth Meier, Jeff Gill, and George Waller asserts that traditional statistical techniques do not serve scholars of public management very well because they are interested in high and poor performers, rather than the "typical" case identified by traditional ordinary least-squares regression. They advocate the use of substantively weighted least squares (SWLS), which weights each case according to substantive performance. High-performing school districts were identified with a studentized residual selection criterion.

For subsequent analysis they were weighted more heavily than typical and poor performers. The authors similarly identified “failures” and analyzed them separately after also weighting them according to performance.

The point of the Meier et al. project was to remove average performers from the analyses entirely. A comparison between the two groups showed that high-performing districts in each category (high optimizers versus failures) were able to get better performance from various resources (increases in state aid, higher teacher salaries and instruction funds) than other districts. This chapter is certainly worth the attention of public management scholars. It might be interesting, however, to run the models (after applying their selection criterion) and weight each case by its standardized residual, rather than the somewhat arbitrary levels they chose. Nonetheless, their contribution supports the book’s theme that there can be no single recipe for improving success.

Chapter 7 represents an interesting qualitative strategy for learning about “street-level” workers in public agencies. Steven Maynard-Moody and Suzanne Leland asked line bureaucrats to tell stories about fairness, working with clients, and life in the agency. The chapter would have been improved if subjects had been asked to tell a story about when they exercised judgment and it led to success, to tell a story about a personal exercise in judgment that led to failure, and to state their beliefs about why. This not only would examine more directly decisions and discretion but also would allow better theorizing about factors that contribute to success. Nonetheless, this omission does not detract from their major premise concerning the utility of stories as a learning tool.

Part III concerns strategies for reform and innovation that hold potential for improving the success of public agencies. It rejects the conventional lay wisdom that public agencies are resistant to change but notes many obstacles to it. The major task for the authors is to make sure change is reasoned. Eugene McGregor offers a new vocabulary and three heuristic devices. He asserts that a common language and conceptualization will improve the odds of bringing about desired change. His chapter is written more for the practical manager than the scholar, but both will find useful items. The other chapters in Part III offer their own frameworks for identifying obstacles to innovation and avenues for change. They mostly agree that the obstacles are formidable. One theme that appears in various places is the importance of culture, which is ignored by managers at the risk/cost of failure.

Part IV emphasizes general frameworks and is the strongest section theoretically. The focus is on frameworks that hold the most potential for advancing knowledge and practice. The chapter by Patricia Ingraham and Amy Kneedler is partly a summary of the literature, but it also offers a “performance model” for describing the key relationships and systems in public agency management. They take us inside the famous, or perhaps infamous, “black box” of public management in an attempt to understand what takes place within.

How competently does the book address the four organizing questions? The question about the role of a public manager and whether managers matter is never really answered. Instead, we learn that managers have the potential to have an effect, notwithstanding certain types of limitations. The contributors do not directly attempt to define that effect, although they intuitively assert that managers matter, but the section is really geared toward explaining why stronger answers have not yet been provided. Since that question is vastly more difficult and complex than the other three, the authors are to be forgiven for emphasizing the “role” of

managers over whether they matter. The other parts of the book come closer to providing answers to their respective questions.

Veto Bargaining: Presidents and the Politics of Negative Power. By Charles M. Cameron. Cambridge: Cambridge University Press, 2000. 292p. \$59.95 cloth, \$18.95 paper.

Charles Tien, *Hunter College, CUNY*

Veto Bargaining is an important book and a pleasure to read. It is important because it takes us a long way in understanding presidential veto politics. The power of the veto is a foundation of the checks-and-balances system and the separation of powers, and it is surprising that so few data have been collected on it. This book is a pleasure to read because it provides a framework for understanding the many absorbing veto cases it includes. For example, Cameron shows that congressional uncertainty about the president’s acceptable policy preferences can help the president wrest policy concessions from Congress. This helps us understand why Truman vetoed Republican tax cuts three times during the 80th Congress. By the time legislators found the right package to attract an override majority, the Truman vetoes had forced Congress to concede 15% off the cut for the wealthiest Americans.

Cameron frames veto bargaining within the context of separation of powers, which summons institutional battles over policy especially during divided government. The main tool at the disposal of presidents during these battles with Congress is the veto. Cameron points out that presidents have been ready and willing to use this tool when government is divided and when the legislative stakes are high. More important, he shows that the veto has been a very effective factor in bargaining with Congress. Its use or threat has allowed presidents to wrestle policy concessions from Congress.

Theoretical work argues that veto power is limited and asymmetric: It allows presidents to get less out of Congress than what Congress wants to give but no more than what Congress wants to give. Cameron builds on earlier models and introduces uncertainty (about the policy preferences of the president and about the pivotal member of Congress whose vote can override a veto) into the veto bargaining game. The result is the view that veto power is “much more consequential than is commonly believed” (p. 26).

There is much to like about the book. The rational choice models are grounded in real-world politics. The research is more than a series of mathematical exercises and increases our understanding of veto bargaining. The applied rational choice models provide new intuition about veto politics between Congress and the president. Furthermore, Cameron’s book should quiet critics who argue that empirical testing of the rational choice models is inadequate. The models developed in chapter 4 are thoroughly tested empirically in chapter 6, with original data, and in chapters 8 and 9, with many succinct case studies.

The amount of original data collected for this book is impressive and informative. Cameron compiled event histories of 434 vetoes from 1945 to 1992. These reveal that most vetoes occur during divided government (71%); very few appropriations bills are vetoed (8%, or 34 total); and a significant number of vetoes occur in chains (41%), that is, either Congress passes another version of the bill, or the president vetoes the bill more than once. Cameron also classifies all bills passed between 1945 and 1994 into one of four categories according to their significance. From these

classifications we learn that (1) the majority of vetoes occur over minor bills (56%); (2) vetoes of landmark and important legislation usually occur in veto chains (65%); and (3) most vetoes of minor legislation are final (70%), that is, there are no other bills or vetoes regarding the matter.

The book is quite comprehensive in its coverage, but there is no mention of the line-item veto. Every president since Jimmy Carter has asked for the authority to single out items for veto on spending bills after signing the other parts of the bill. Congress gave this power to the president in 1996, but in 1998 the Supreme Court ruled it unconstitutional in *Clinton v. City of New York*. The issue is important, as 43 of the 50 governors have line-item veto power.

All students of the presidency and Congress should read this book. The importance of the findings and the breadth of the original data collected and interpreted alone make it worth reading. This should be required reading in graduate courses on the presidency and on legislative behavior. It is also an excellent example of how to do rational choice research and do it well. The book is probably not appropriate for most undergraduates, especially those who have not been exposed to formal theory, although advanced undergraduates would benefit from the findings in chapter 2, based on data from the post-World War II era.

Institutional Constraints and Policy Choice: An Exploration of Local Governance. By James C. Clingermayer and Richard C. Feiock. Albany: State University of New York Press, 2001. 151p. \$17.95 paper.

David R. Elkins, *Cleveland State University*

For more than twenty years, urban scholars have debated whether economic determinism and its fiscal implications trump municipal political action in local government policy choices. In a very real sense, the debate is about whether local politics matters. Somewhat lost in this discussion, although not entirely, is an issue that was once at the forefront of urban scholarship: the role of institutional structures. With the maturing theoretical interest in the new institutionalism, the time is ripe to revisit this area and determine whether institutional structures matter. According to James Clingermayer and Richard Feiock, they do.

The term "exploration" in the subtitle is an apt description of this work. Using seven separate data sets (three are derived from the authors' surveys conducted during the 1980s, three from 1980 Census data, and one from the International City Management Association for 1988), the authors explore issues as diverse as economic development decision making, municipal practices associated with zoning, citizen-initiated contacts and casework activities of council members, contracting out for services, and debt financing decisions. In addition, they explore how external constraints associated with annexation policy, exclusionary zoning practices, and the Tax Reform Act of 1986 affect municipal policymaking. The breadth of the empirical analysis makes a convincing case that urban scholars should seriously consider the role urban institutional structures have in shaping local policy decisions. Indeed, Clingermayer and Feiock hope that their work will reenergize research in this area.

The authors define *institutionalism* as the "formal and informal rules operating within or across organizations" (p. 2). In their analysis of municipal governments, they place great emphasis on the formal rules of institutions. It is argued

that institutions are important because they provide stability, shape choice sets, and structure decision making. Specifically, "electoral rules and constituency boundaries affect policy makers' behaviors by determining what sorts of voters they must please if they are to remain in office or rise to higher office" (p. vii). Although the research is firmly anchored in the new institutionalism, there is also a strong linkage, which the authors readily acknowledge, to the classics in this field.

The effects of institutional features on municipal economic development activities are examined from several angles. First, the authors explore how differences in structural features, particularly those of elected officials, are associated with five separate economic development policies and practices. Second, they ask whether the adoption of comprehensive zoning is attributable to either market failure or distributive politics. Third, they turn to the question of how institutional structures shape development-based constituency-related experiences of council members. The conclusion is that institutional structures do matter with regard to these varied economic development activities, but institutional effects are subtle.

Clingermayer and Feiock also examine the decision-making effects of turnover in leadership and council membership. They propose two general explanations for the influence of turnover on local policymaking. The first suggests that private organizations involved in negotiations with a city that has a high level of turnover may be apprehensive about the city's ability to fulfill long-term obligations, and this political uncertainty drives up the transaction costs associated with negotiations. The second explanation hinges on the attempt by elected officials to avoid blame for potentially controversial decisions or to concentrate benefits to enhance political prospects. Clingermayer and Feiock find support, albeit weak, for both explanations but along an intriguing faultline. They find that administrative turnover is inversely associated with the level of services contracted out and that mayoral turnover is directly associated with contracting out. Also, in cities with council-manager structures, turnover in council membership is inversely associated with various measures of municipal debt.

Finally, the authors explore how state and federal decisions affect municipal policymaking. Their analysis of annexation and exclusionary zoning suggests that state-level attempts to limit the discretionary authority of local officials have largely succeeded. They also find that the Tax Reform Act of 1986 reduced the level of municipal revenue bond debt.

The book is not without flaws. The use of multiple data sets and methods creates a trade-off between breadth of analysis and continuity of argument. The authors remain focused on the exploration of the institutional dynamics of municipal policymaking, but the book seems to lack the unified theme typical of research based on fewer data sources. In some instances it seems this is a collection of discrete articles, with little connection between one chapter or section and another. The book would have benefited from an appendix with details about the various data sets, the construction of variables, and some of the statistical problems associated with the analyses. For instance, regarding citizen-initiated contacts, Clingermayer and Feiock note that "usable responses were received from 234 council members" (p. 39), but the statistical analysis refers to an *N* of 177 (p. 41). Why were so many cases dropped? Although I am inclined to find brevity a virtue, further development of some ideas would have been helpful. For instance, the discussion of time horizons is intriguing and warrants elaboration.

These criticisms should not dissuade urban scholars from

reading this worthwhile contribution. It undoubtedly will be cited among the classics in the institutional-structural explanations of municipal policymaking. Indeed, drawing on this earlier generation of scholarship, Clinger-mayer and Feiock conclude that institutions matter but in ways that “mediate and interact with other factors” to influence outcomes (p. 123). Whether Clinger-mayer and Feiock accomplish their goal of reinvigorating the institutional perspective in urban scholarship remains to be seen, but their exploration is a critical first step.

Empire on the Hudson: Entrepreneurial Vision and Political Power at the Port of New York Authority. By Jameson W. Doig. New York: Columbia University Press, 2001. 620p. \$49.50.

Steven P. Erie, *University of California, San Diego*

Empire on the Hudson is a dual biography of the Port of New York Authority, America’s first semiautonomous, self-financing public development agency, which helped inspire the Tennessee Valley Authority, and of the individuals who shaped it. It is also much more. This case study of local government’s catalytic role in regional economic development is a major contribution to the twentieth-century history of the New York metropolitan region. In an era when bureaucracy has become synonymous with failed government programs, the Port Authority is a valuable reminder of the positive contributions that public agencies can make. It also serves as a useful prism through which the author views and critiques relevant theories of political leadership.

Doig shows that the Port of New York Authority, chartered in 1921 by the states of New York and New Jersey, was a latter-day child of the Progressive reform impulse of the early twentieth century, which sought rational planning by an activist government, purged of corruption and infused with “business-like” efficiency. The initial impetus was the fear that the region’s magnificent waterfront might lose its pre-eminent status to other East Coast ports because of the additional time and costs entailed by docking facilities located mostly in New York but rail terminals located mostly in New Jersey.

The newly created authority did, indeed, have a life of its own. From the 1920s through the 1950s, it evolved from the relatively narrow mission of rail-freight planning to a much broader mandate to build and operate bridges and tunnels, assume the dominant role in regional air transport (at Newark, LaGuardia, and Kennedy), and construct a containerized marine terminal, a gigantic mid-Manhattan bus terminal, and a new arterial highway system.

The financial underpinnings of this “imperial” bureaucracy were the bridge and toll revenues that allowed the authority to self-finance most of its capital projects by issuing revenue bonds rather than depend on local and state governments for funding. In classic survival-of-the-fittest fashion, it successfully fought off challenges from both within and without the region. New York Mayor John F. Hylan in the 1920s mobilized Tammany Hall against the Port Authority, but he was forced into retirement by the authority’s good friend, Governor Al Smith. Robert Moses, the region’s well-connected transportation “power broker,” tried unsuccessfully to block the authority’s new airport management role as well as its Manhattan bus terminal, but he later cooperated with the authority on bridge and highway improvements. Earlier, there was a serious challenge from Washington when the New Deal attempted to end the tax-exempt status of munic-

ipal bonds. This was defeated by a nationwide, ostensibly “grassroots” campaign skillfully orchestrated from the offices of the Port Authority.

Doig quotes Ralph Waldo Emerson’s aphorism that institutions are “the lengthened shadow of one man” but credits three individuals. Julius Henry Cohen, the authority’s general counsel from 1921 until 1942, first emerged into prominence as a garment industry lawyer who promoted the Brandeisian cause of commercial arbitration of labor disputes. He carried his cooperative principles into the public sector and crystallized the idea of bi-state partnership embodied in the 1921 Port Compact. Swiss-born Othmar H. Ammann, the authority’s chief engineer from 1928 to 1939, lobbied for, designed, and supervised the construction of the George Washington Bridge and the Lincoln Tunnel. He returned in the 1950s to design the second deck of that bridge as well as the new Throgs Neck Bridge and the Verrazano-Narrow spans. The third creative bureaucrat was Austin J. Tobin, the authority’s assistant attorney and then its executive director. A graduate of Holy Cross College who lost his interest in theology, he committed himself to the authority as a crusade. He ran the campaign that defeated the aforementioned New Deal policy in the 1930s and then field marshaled the authority’s new initiatives in the 1940s and 1950s as airport manager, bus terminal operator, and highway engineer.

The author impartially documents not only the authority’s successes but also its failures and periods of drift and retrenchment. In the 1920s, the authority was fought to a standstill by the dozen railroads that served the metropolitan region and had no interest in centralizing their competitive operations. Declining revenues during the Depression and the early years of World War II coincided with the ascendancy of Frank C. Ferguson as Port Authority chairman. A conservative investment banker, he focused his tunnel-vision on balance-sheet considerations. Tobin’s career from the late 1950s to his abrupt retirement in 1971 (after alienating governors of both states) was a mixed bag. His successes in constructing new bridges and highways were counterbalanced by a financially disastrous mass transit experiment, the billion-dollar grandiosity of the World Trade Center project, and the defeat of a proposed jetport in northern New Jersey.

Doig’s concluding chapter covers in less detail the embattled post-Tobin era. Since the 1970s, the authority has alternated between “malaise” or retrenchment and controversial new initiatives that the author views as disguised attempts to divert its revenues into nontransportation projects run by local and state politicians. Critics, including New York Mayor Rudolph Guiliani, want to scrap the authority, but Doig hopes for a rebirth of creative leadership in the twenty-first century.

This gracefully written book is a significant contribution to the literature. Doig offers a useful corrective to Robert Caro’s (*The Power Broker*, 1974) understandable overemphasis of the omnipotence of his protagonist, Robert Moses. Doig also provides a sensible critique of James Q. Wilson’s view that public bureaucracies invariably are passive responders, of necessity driven by external forces, rather than proactive movers of events, motivated by creativity and innovation emanating from within. Finally, Doig gives deserved attention to the ethical dimension of public leadership, specifically the need to balance organizational efficiency with democratic accountability. In so doing, *Empire on the Hudson* joins the front rank of scholarship on public enterprise and enterprising public entrepreneurs.

Racist America: Roots, Current Realities, and Future Reparations. By Joe R. Feagin. New York: Routledge, 2000. 311p. \$25.00.

Robert C. Smith, *San Francisco State University*

Joe Feagin is the leading social science authority on racism in the United States. A sociologist, Feagin wrote a series of seminal articles on institutional racism during the 1970s, followed by numerous essays and several books. *Racist America* is his most ambitious work on the subject. Its purpose is to develop an antiracist theory and analysis that will explain the phenomenon but also indicate ways to change and eventually eradicate it. According to Feagin, there are fairly well-developed theories and shared concepts to study systems of class and gender oppression in America, but the social science community has not yet reached a consensus on how to define racial oppression and give it operational meaning, and there is no well-developed theory to account for its origins and persistence.

To remedy these deficiencies Feagin develops a theoretical framework centered on the concept of "systemic racism." Racism is manifested throughout the economy, polity, education, religion, and the family. This theoretical reality is reflected empirically in a complex array of antiblack practices; in the unjustly gained economic resources and political power of whites; and in the ideologies and attitudes of white supremacy that developed in order to rationalize the system. Feagin avers that a pluralistic analysis of oppression in the United States is ultimately necessary because "class structured capitalism, sexism, bureaucratic authoritarianism and homophobia are all important parts of the webbed package of oppressions internal to U.S. society" (p. 6). Yet, such an analysis is, in Feagin's judgment, premature. Therefore, *Racist America* focuses mainly on white-black oppression (with passing references to how other oppressions interact with it) because it is paradigmatic for the other forms of oppression in America.

After sketching out the theoretical perspective, Feagin provides six chapters of historical analysis and empirical data to illustrate the theory's utility. The empirical data include an extensive mining of the social science literature, government and private studies and reports, and results from numerous field research projects Feagin and his associates have conducted in the last decade. These projects include, among other things, hundreds of interviews with blacks from all walks of life about their experiences with racism and with whites about their views of blacks and of racism.

Chapters 1 and 2 are historical narratives about the creation of the systems of racism, sexism, and economic exploitation that are at the foundations of America. Feagin notes that under pressure from the abolitionist and civil rights movements some progress was made in the 1860s and 1960s in modifying some of the cruder manifestations of systemic racism, but the system has remained largely intact. For example, he describes the reforms of the 1960s as modest efforts designed to fit some blacks, women, and other racial minorities into the society without attempting to dismantle the white-male-dominated hierarchical system itself. Thus, he concludes, all the talk about equality since the 1960s is a "ruse and illusion" (p. 36).

Chapter 3 examines the origins and evolution of the ideology of white supremacy; chapter 4 examines antiblack images in the media; chapter 5 analyzes the data on racism in the everyday practices of whites and experiences of blacks, including institutional manifestations in housing, education, employment, business, public accommodations, and criminal justice. The sixth chapter shows how blacks are damaged in

material and psychological wellbeing by racism and how whites are privileged in psychic esteem and material benefits by it. The last chapter presents strategies and policies for the "uprooting and replacement of the existing systems of racialized power" (p. 270), focusing on the need for a multiracial movement of a large scale, with destabilizing protests that will alter the white elite's calculus of the cost of maintaining the system.

Feagin's ambitious effort to place racism in a systems framework is an important contribution that should be widely read and used in research on racism in the United States. It is a refreshingly comprehensive departure from the seemingly omnipresent books since the 1970s that focus on the declining significance of or even the end of racism in America. The work, however, is not without problems that may limit its use both in the research process and in the development of antiracist strategies and movements.

The most basic limitation is the theory of systemic racism itself. As briefly sketched out by Feagin, the theory is ambiguous. It is not clear, for example, whether racism is systemic in the sense of being a core, defining value of America equivalent to capitalism, democracy, and constitutionalism, or whether it is simply systemic in the sense of a complex, interdependent, interactive series of components. A related but distinct ambiguity is whether the theory suggests that racism may not be a core or defining American value but is nevertheless so integral to the operation of the economy and polity that its elimination requires the elimination or at least radical modifications of capitalism and the existing constitutional basis of the democracy. Clarifying work on both of these aspects is necessary before the theory can be used in research and in antiracist activism.

Feagin refers to the concept of oppression throughout the book without ever defining it. This will not do. As he himself writes, "concepts delineating and probing racism need to be clear and honed by everyday experience" (p. 4). This is especially important because the term is used to encapsulate the experiences of women, homosexuals, and recent immigrants as well as African Americans, and, as Feagin knows, the experiences of these groups are not isomorphic with those of blacks. It is useful, therefore, to distinguish between oppression and such related but distinct phenomena as racism, discrimination, prejudice, xenophobia, and stereotypical thinking and behavior. Finally, how does one measure the decline of oppression? If the changes brought about by the civil rights movement of the 1960s were modest and a "ruse and illusion," what would major and real changes look like? Indeed, short of absolute equality in psychic and material wellbeing among all groups, how would we know when systems of oppression have been uprooted and eliminated?

Feagin's work points the study of American racism in the right direction. As the author observes in the first chapter, the social scientific examination of U.S. racism is in the early to middle stages of theoretical and empirical analysis. Lamentably, this is true even after more than a hundred years of studies. Most of the scholarship on race from its beginnings until the 1930s reflected a racist and white supremacist character, and since then the social science community—political scientists especially—has been unwilling to devote the necessary interest and resources to racism in graduate studies, research, and academic journals. Feagin, whatever the limitations of this book, has made an important contribution that will advance the study of the realities of racism in America.

Politicians Don't Pander: Political Manipulation and the Loss of Democratic Responsiveness. By Lawrence R. Jacobs and Robert Y. Shapiro. Chicago: University of Chicago Press, 2000. 425p. \$50.00 cloth, \$17.00 paper.

Jeffrey E. Cohen, *Fordham University*

Many observers of contemporary American politics argue that political leaders are overly sensitive to public opinion, that they pander to the public. Lawrence Jacobs and Robert Shapiro suggest that this view is wrong on two counts. First, at least for the past two decades, politicians have not been very responsive to public opinion. Indeed, they are likely to try to manipulate it. Second, from a democratic theory perspective, Jacobs and Shapiro muse upon the oddity of viewing responsiveness in a democracy as disreputable (p. xiv).

The authors pursue three major tasks. First, they attempt to explain why responsiveness to public opinion has declined over the past two decades. Second, they build a theory of the dynamics of contemporary American politics, which they test on two important cases, the Clinton health care initiative and the Republican Contract with America. Third, they redirect discussion about the role of leadership and responsiveness in democracy and democratic theory. This is a large agenda for any project. The authors succeed quite admirably and have produced one of the most important books on American politics that I have read in years.

In chapter 1, Jacobs and Shapiro critique the idea that politicians pander and discuss the limitations of theory and research on the lack of responsiveness in current American politics. In chapter 2 they lay out their theoretical framework. The next five chapters provide a detailed case study of the Clinton health care initiative. Chapters 3 and 4 focus on the behavior of the protagonists and antagonists in the policy debate. Chapters 5 and 6 present a detailed look at media coverage of the health care debate, and chapter 7 focuses on the public response. Chapter 8 presents a less detailed case study of the Contract with America. The book concludes with two chapters that deal with issues of democratic theory. Chapter 9 discusses the dilemmas of modern democracy, especially the implications of crafted talk and leader nonresponsiveness. Chapter 10 offers suggestions about how to increase responsiveness to public opinion but at the same time provide leadership and improve the quality of democratic deliberation.

Jacobs and Shapiro begin by suggesting that responsiveness has declined and marshal considerable supporting evidence from other scholars. They then address why this decline has occurred and point to five major factors: polarization of the political parties, the rise of individual independence in Congress, the effects of increased incumbency, the proliferation of interest groups, and increasingly divisive interbranch relations. Along the way, they also critique the two major theories that explain the behavior of politicians: electoral incentive models, which emphasize responsiveness to the median voter, and policy motivation models. They do not eschew either, but they forcefully argue that each is incomplete.

Jacobs and Shapiro then move to their second task, building a theory of contemporary American politics. Their framework is extremely rich and subtle and cannot be easily summarized here. Their theory deals with the interconnections among politicians, the mass media, and public opinion.

Politicians, according to Jacobs and Shapiro, currently have strong incentives to pursue their policy preferences rather than respond to public opinion, but they also have strong incentives to appear responsive, which Jacobs and

Shapiro term "simulated responsiveness." This is done by "crafting talk," which is a major conceptual contribution of this work. That is, politicians track public opinion to identify the words, arguments, and symbols that the public finds most appealing with regard to particular issues. These are then used to prime the public, not so much to persuade, but to raise the issue to the top of the agenda. Policy advocates tend to prime the public to the benefits of a policy and avoid discussing the costs; opponents prime the costs, encouraging the public to view proposed reforms as threatening.

The media pick up on this policy debate, and Jacobs and Shapiro make an important argument in this regard: The heightened attention in the media to political conflict may reflect reality and is not merely the imposition of media practices on political discourse. Still, the media amplify the conflict. As a result of the behavior of competing politicians and media coverage, public uncertainty about the benefits and costs of policies is elevated. Opponents are often advantaged, because managing uncertainty is easier than keeping the public focused on potential benefits. As a consequence, it is hard to implement reforms. Furthermore, the emphasis on crafted talk and the atmosphere of conflict breeds public disaffection with politics and politicians. This brief summary does not do justice to the richness and sweep of the theory, which deals essentially with the core operation of the current political system.

The empirical aspects of this study are no less ambitious. To test the theory requires data on the behavior of politicians, media news coverage, and the reactions of the public. Jacobs and Shapiro collect a wide variety of data, including content analyses of politicians' public statements, interviews with politicians and other participants, content analyses of the news media, and public opinion polls.

Jacobs and Shapiro also address important themes in democratic theory. They try to find a balance between leadership and responsiveness, which they term "responsive leadership." Their discussion blends normative political theory with empiricism and bridges the divide between the two approaches, which is rarely done.

There is little to criticize in this important book, although more cases to test the theory would have been desirable. The Clinton health care initiative and the Contract with America were not fully enacted. The theory predicts that, outside election periods, it will be difficult to enact major reforms. Yet, some important policy changes were implemented in the 1990s, such as NAFTA, and a major tax reform was passed in 2001. What accounts for the success of these major policy efforts? How would the theory take account of these cases? Jacobs and Shapiro provide a fertile agenda for future research.

We apply too readily the label "instant classic." This book not only deserves that label but also will redirect thinking about American politics and will be read for years to come.

Presidents, Parties, and the State: A Party System Perspective on Democratic Regulatory Choice, 1884–1936. By Scott C. James. Cambridge: Cambridge University Press, 2000. 318p. \$59.95.

Marc Allen Eisner, *Wesleyan University*

Following a narrow victory, a president seeking to strengthen his party's coalition begins to embrace regulatory initiatives that appear to be at odds with the commitments of his party and the demands of core coalition members. Yet, if these efforts secure the support of pivotal voting blocs and ensure future victories in the Electoral College, many loyalists would

be satisfied. After all, control of the presidency is necessary if one wants to control the policy agenda, and that control is impossible without reelection.

This drama, played out in the months following the 2000 presidential election, is as old as the regulatory state itself, according to *Presidents, Parties, and the State*. Although Scott James focuses on three earlier episodes in American political development, he offers important insights into the evolution of the regulatory state more generally. At first glance, one would not expect the current occupant of the White House to embrace new initiatives in environmental protection. But who would have expected an antistatist Democratic Party to promote the Interstate Commerce Act? Who would have anticipated that a party strongly opposed to the rule of reason would advocate a Federal Trade Commission along the lines suggested by Teddy Roosevelt?

The core argument is that pressure-group and district-level models of legislative choice do not provide adequate accounts of regulatory choice during 1884–1936. A more satisfactory explanation is the party system perspective, which places national party competition for the presidency at the heart of regulatory choice. “In confronting the problems of industrial capitalism, Democrats were forced to choose between the short-term desire to enact policies consistent with the long-standing party commitments, or jettison these commitments in deference to the preferences of electoral groups whose political support was pivotal to the consolidation of governing power. It was a Hobson’s choice because in the world of democratic politics, the stabilization of a winning electoral coalition was a precondition for anything else the Democratic party might reasonably hope to accomplish in office” (p. 268).

After framing his party system perspective, James presents three case studies: the Interstate Commerce Act of 1887, the Federal Trade Commission Act of 1914, and the Public Utility Holding Act of 1935. Each explores the incentives created by the Electoral College and party system, the regulatory preferences of the voting blocs deemed essential to victory, and the historical and political contexts that shaped policy alternatives and the intervention of party leaders. James provides a lively but theoretically informed account of legislative action and coalition building that integrates careful historiography with quantitative analyses of congressional voting behavior. Each case study is situated within the larger dynamic of evolving relationships among the parties, the president, and Congress; the decline in congressional party government; and the emergence of a president-centered polity.

In the case of the Interstate Commerce Act, deeply seated antimonopoly and antistatist impulses within the Democratic Party generated strong opposition to the creation of a railroad commission staffed with experts and charged with the task of administering the industry. Yet, business Mugwumps had assumed the status of a swing group in the presidential election of 1884. Grover Cleveland and other party leaders needed to obtain their allegiance if they wanted to increase the chances of future electoral success. This required the passage of the Interstate Commerce Act over the objections of agrarian Democrats. The demands of coalition maintenance and the incentives created by the Electoral College trumped the historic commitments of the party.

The Federal Trade Commission Act involved many of the same issues. The Democratic Party’s antitrust program was strongly opposed to the Supreme Court’s rule of reason, corporate consolidation, and any efforts to manage this consolidation through the creation of expert commissions. The party demanded instead that new laws be passed to force

deconcentration and unrestricted market competition. By 1914, an impending depression led many to counsel Woodrow Wilson to suspend the pursuit of new antitrust legislation. Teddy Roosevelt and the Progressive Party threatened to dominate the reform debates and link Democratic policy failures to the growing economic malaise. Understanding the potentially disastrous electoral consequences for the midterm elections and his own reelection, Wilson embraced the Federal Trade Commission Act. Its passage would allow the party to appeal to Progressives and organized business, thereby strengthening the Democratic coalition. As in the earlier episode, coalition building would carry a price: the alienation of agrarian Democrats.

The case of the Public Utilities Holding Company Act (PUHCA) of 1935 returns to the theme of coalition maintenance. Franklin Roosevelt’s Democratic Party had yet to consolidate its hold on power. This may have been impossible without the support of midwestern Republicans, who had become disenchanted with the collectivism of the New Deal and the lack of structural reform. Roosevelt met the demands for reform with PUHCA. It was a death sentence for utility holding companies and struck at the heart of the so-called Power Trust, thereby exhibiting the Democratic Party’s commitment to reform. Ironically, the demands of coalition maintenance created incentives for Roosevelt to move to the left of congressional Democrats and take up the antimonopoly impulses of earlier generations of agrarian Democrats, who had been sacrificed previously in the name of electoral victory.

The final chapter of *Presidents, Parties, and the State* explores a number of provocative questions. For example, why was the Democratic Party—but not the GOP—forced repeatedly to choose between well-established party commitments and the demands of electoral blocs whose political support was pivotal to coalition building? Given the compromises involved in building and preserving coalitions, what was the regulatory legacy of Democratic agrarians? These are important questions for anyone interested in American political development and policy history. Although one might wish for some sustained discussion of the implications of the party system perspective for regulatory choice in the contemporary period, James has accomplished much with this volume. It makes a significant contribution and should have an effect on future discussions of the role of parties, elections, and presidents in the construction of the regulatory state.

Race, Neighborhoods, and Community Power: Buffalo Politics, 1934–1997. By Neil Kraus. Albany: State University of New York Press, 2000. 294p. \$19.95 paper.

David L. Imbroscio, *University of Louisville*

This book grapples with what are perhaps the two central questions preoccupying the academic study of cities in the postwar era, questions that are quite familiar to political (and other social) scientists. The first asks who (or what) governs cities, that is, the community power question. The second examines why (or how) cities became marked by—to use Jonathan Kozol’s powerful phrase—savage inequalities, especially along racial lines. The first concerns process: how political issues are resolved and the relative power and influence of actors and structural forces in their resolution. The second involves outcomes, specifically, the pervasiveness of deeply entrenched poverty spatially concentrated in the older neighborhoods of the urban core.

Through an examination of the political processes and resulting outcomes in one American city, Buffalo, New York, Neil Kraus admirably aspires to address both questions concurrently. Based on his study of housing, redevelopment, and education during more than a half-century, Kraus offers provocative contentions regarding both questions, as he challenges to some degree the conventional wisdom surrounding each.

First, Kraus postulates that the strong focus on the influential role of private sector elites who act largely behind the scenes—whether expressed through the current dominant paradigm in community power studies, regime theory, or early elitist theories—is “somewhat misplaced” (p. 11), at least in regard to issues that affect residential patterns and neighborhoods (as opposed to downtown redevelopment). Instead, he revives and embraces some of the key tenets of pluralism; that is, the nature of community power can be understood by examining overt, clearly visible, and publicly debated decisions. The core of Kraus’s story is about race, location, and racism, as whites in Buffalo used their numerical majority to limit residential choice and economic opportunity for African Americans via the traditional and straightforward processes of elections and political representation.

As for the second question, Kraus contends that concentrated urban poverty can be strongly linked causally to politics, as opposed to economic or demographic trends, because “political structures have played a critical role in ghetto formation” (p. 11). This stands in opposition to the standard explanation, which focuses largely on the timing of southern black migration to northern cities and the contemporaneous white flight to suburbia, coupled with the effects of postwar economic restructuring and deindustrialization.

Most interesting is the way Kraus brings both questions together. He not only argues for the significance of politics in explaining the formation and durability of the American ghetto but also emphasizes “the decisive influence of *local* politics,” that is, the struggle over community power (p. 7, emphasis added). Rather than focus solely on macrolevel processes, Kraus attempts to demonstrate that concrete local public policy decisions must be included in any explanation of ghetto development in Buffalo and the plight of its African American poor. In this sense the book can be read as yet another work in the literature stream that emphasizes “local politics matters” in shaping urban outcomes.

By advancing this claim Kraus astutely draws attention to a causal relationship often overlooked in studies of urban poverty. For this reason alone, his book makes a positive contribution, although his analysis could have been more penetrating. For example, Kraus makes the “local politics matters” argument by attempting to show that deindustrialization was not a significant factor in creating concentrated poverty in Buffalo because African Americans never had access to good industrial jobs in the first place (pp. 6–7, 38–41, 221). But Kraus fails to take his localism argument far enough. Work on cities such as Baltimore and Chicago reveals that deindustrialization was not simply the result of macroeconomic change; it also was driven in part by local policy decisions to pursue economic strategies built around downtown real estate development and tourism rather than preservation of the manufacturing base. Therefore, one can argue that local decisions mattered deeply in worsening the problem of concentrated poverty and acknowledge that deindustrialization was a significant cause of concentrated poverty. But doing so requires an appreciation of how urban economic processes are far from beyond the influence and control of local politics.

Chapters 4 through 8 provide a highly detailed account of

Buffalo politics from the mid-1930s to the late 1990s. It is presented in a chronological, event-by-event fashion, and serves as the empirical data for the study. As a telling of local history, this section of the book (roughly two-thirds of the total pages) works reasonably well. Those with little interest in Buffalo politics may lack the patience for the level of detail presented, however. In addition, the careful description of historical events could have been placed in a much tighter analytical framework and tied much more closely and rigorously to Kraus’s theoretical propositions regarding community power and the development of the ghetto.

In the final chapter, Kraus discusses the evils of segregation and concentrated poverty and makes some gestures toward policy solutions. He also reiterates many of his claims regarding the nature of community power, such as the continuing importance of pluralist theory for understanding city politics. In both discussions Kraus nicely draws on the Buffalo case to illustrate his main analytic points, but these concluding sections would have benefited considerably from a more thorough and systematic treatment. Especially needed is a fuller demonstration of how the empirical case supports the validity of his rather provocative theoretical propositions. This chapter is a mere ten pages and could have been extended and deepened to strengthen the book’s overall significance and contribution.

Despite many flaws, this book is well written and addresses meaningful urban issues. Moreover, unlike numerous other American cities of similar size, such as Cleveland, Atlanta, Pittsburgh, St. Louis, and New Haven, the Buffalo political story has “not really been told” (p. xi). This book is a welcome addition to the academic literature on the politics of American cities.

Crafting Law on the Supreme Court. By Forrest Maltzman, James R. Spriggs II, and Paul J. Wahlbeck. Cambridge: Cambridge University Press, 2000. 206p. \$49.95 cloth, \$17.95 paper.

Cornell W. Clayton, *Washington State University*

Important contributions in the social sciences are usually made in one of two ways. Either they tell us something we previously did not know, or they verify, in a systematic way, something that we did. *Crafting Law on the Supreme Court* falls into the latter category. The premise of this book is remarkably simple. The authors argue that decision making on the Supreme Court is interactive; justices consider the views of their colleagues when writing their opinions. Justices engage in the “collegial game” of bargaining and compromise because they are constrained from unilaterally writing their individual policy preferences into law by a set of institutional rules, such as the need to attract five votes for a majority, which make the Court a collegial institution.

The collegial character of the Court is well known. The authors quote Chief Justice Rehnquist, who candidly admits that, in the process of producing opinions, “give and take is inevitable, and doctrinal purity may be muddied in the process” (p. 152). Indeed, even among political scientists, who usually are slow to recognize the obvious, the interactive nature of opinion writing was thoroughly discussed nearly forty years ago in Walter Murphy’s classic, *Elements of Judicial Strategy* (1964). What makes this book a crucial contribution to the literature is not its premise but its use of a sophisticated method and innovative database to test a multivariate model of interactive decision making. Specifi-

cally, the authors seek to identify systematically “what factors affect whether, when, and to what extent justices decide to bargain, negotiate, or compromise in the process of writing opinions” (p. 25).

In the first chapter the authors present a model of decision making based on two central postulates: (1) Justices prefer legal rules that comport with their policy goals, and (2) given the Court’s institutional rules, justices often act strategically to secure their policy preferences (p. 17). The first postulate draws on the work of attitudinal scholarship, especially that of Jeffrey Segal and Harold Spaeth (*The Supreme Court and the Attitudinal Model*, 1993); the second builds on scholarship in the positive theory of institutions, or the “strategic choice” tradition, especially the work of Walter Murphy as well as more recent scholars, such as Lee Epstein and Jack Knight (*The Choices Justices Make*, 1998). To test their postulates, the authors created a database of all cases that received full disposition opinions during the Burger Court (1969–86). Using the files of Justice William Brennan, the authors culled data from assignment sheets, docket sheets, and circulation records (such as draft opinions and memoranda distributed to the justices generally) about how the justices interact when drafting and circulating opinions.

In the four chapters at the heart of the book the authors test hypotheses drawn from the two central postulates and related to four distinct stages of the process: the initial assignment of the majority opinion, the response to draft opinions, the author’s subsequent reaction to those responses, and the decision whether to join an opinion (i.e., the politics of coalition formation). Their analysis finds strong support for the strategic, interactive model of decision making during each stage. Justices constantly engage in bargaining and compromise over the final content of the Court’s opinions. In a short concluding chapter the authors summarize their findings and comment briefly on the scientific study of courts.

Few of the substantive findings in this study are intended to challenge conventional wisdom. For example, the authors find that justices are less willing to accommodate the views of potential joiners to an opinion once they have secured a majority. Likewise, the Chief Justice, who has institutional responsibilities for the Court as a whole, appears to be more sensitive to questions of equitable work load and Court efficiency when assigning opinions than are assigning associate justices. There are no major surprises here. The significance of this book lies in its “scientific” or quantitative testing of the interactive model of decision making. The methodological work is first rate. The hypothesized relationships are clearly specified and conceptualized. The development and use of the data are ingenious. The analysis is thorough and rigorous. The empirical findings are robust and conclusive with respect to what they measure.

One minor quibble has to do with the types of research design compromises that data constraints impose on this type of study. Specifically, in order to understand how institutional structures shape judicial behavior, it would be useful to have more variation of those structures across time. For example, how do we know that Burger’s assigning patterns stem from his institutional role as Chief Justice, as the authors assume, rather than his own idiosyncratic or attitudinal concerns about Court efficiency? It is possible that Burger would have demonstrated similar assigning patterns even if he were not Chief Justice. Of course, the authors’ assumption is perfectly plausible and in fact quite likely. The point is that we cannot know with an n of 1. Still, this criticism, which relates to broader problems of how quantitative political science has come to conceptualize the relationship between research methods and problems, should not distract from what the

authors have accomplished with the data available. They have told us more about the systematic dynamics of opinion writing on the Court than any previous study.

The book stands either as a formidable challenge to or a major bridge between two separate strands of research on Supreme Court decision making. Attitudinal scholars have long claimed that the process is driven by the justices’ individual policy preferences or “attitudes” alone. Most in this camp, however, only claim that individual preferences determine the final vote on a case’s disposition, and that nonattitudinal variables may operate in other areas of Supreme Court decision making (see Harold Spaeth, “The Attitudinal Model,” in Lee Epstein, ed., *Contemplating Courts*, 1995, p. 314). The authors view their research as a bridge to attitudinal scholarship because they find abundant evidence that individual “preferences shape behavior beyond the final vote” (p. 151). Nevertheless, their powerful empirical evidence that institutional variables condition Supreme Court behavior generally is a challenge to many of the theoretical assumptions that underlie attitudinal research.

The authors also argue that their approach is a bridge to historical-institutional or “traditional legal scholarship” on Supreme Court decision making (p. 151). This literature tends to focus on the historically evolving normative institutions and symbols that structure judicial decision making. Legal doctrines, judicial role conceptions, and other normative institutions that define “good judging” are thought to have a constitutive influence on judges, shaping their values and attitudes (Howard Gillman and Cornell Clayton, eds., *The Supreme Court in American Politics: New Institutional Interpretations*, 1999). The authors of *Crafting Law* present strong empirical evidence that justices are concerned about the content of legal opinions, not just the disposition or outcome of the case, as generally suggested by attitudinalists. This is an important finding for historical-institutionalists and supports the proposition that legal doctrines matter very much to the justices. Yet, the authors argue that justices care about the content of opinions for strategic reasons only, which rejects the core historical-institutionalist position that judges are often normatively driven actors and favors the position that judges are simple policy maximizers self-consciously using legal doctrines instrumentally. Whether this primary assumption can be tested using positivist methods is the subject of intense debate within the field (see “Symposium: The Supreme Court and the Attitudinal Model,” *Law and Courts Newsletter* 4 [Spring 1994]: 3–12). As in Segal and Spaeth’s landmark study on Court decision making, many of the findings here with regard to the ideological motivation of the justices, not to mention the central conclusion that justices care about the content of Court opinions, may be perfectly consistent with a “legal” or normatively driven institutional explanation of their behavior. The authors appear to reject this possibility in order to adopt the more narrow attitudinalist explanation for judicial motivation. Consequently, this study has the potential to be a valuable link to broader institutionalist work, but its key theoretical and methodological assumptions probably serve as a surer bridge to traditional behavioralist research on the Court.

This book is a major contribution to our understanding of Supreme Court politics. It is a significant expansion of empirical-quantitative studies of the Court, moving beyond the typically narrow focus on judicial voting patterns. Simultaneously, it is a major step toward systematically verifying the important role that institutional structures play in shaping judicial politics generally. There is enough empirical fodder here to provoke and console scholars of all theoretical persuasions. The authors do a superb job of drawing insights

from disparate theoretical traditions and melding them into a conceptually sound, parsimonious, and rigorous model. Their book is an important milestone in research on judicial decision making and commands the attention of all serious students of Supreme Court politics.

Affective Intelligence and Political Judgment. By George E. Marcus, W. Russell Neuman, and Michael MacKuen. Chicago: University of Chicago Press, 2000. 199p. \$42.00 cloth, \$15.00 paper.

Mark R. Joslyn, *University of Kansas*

Perhaps it is ironic that a book about rational judgment is entitled *Affective Intelligence*. This is a rousing and provoking effort, with an unmistakable double agenda. One objective is to dispute conventional wisdom regarding the relationship between affect and reason, especially the rational choice vision of political judgment. A second is to establish a more prominent role for emotions in models of political behavior: "The scientific community might fruitfully devote more attention to the idea that emotions are centrally important to political behavior" (p. 129). The two objectives are linked. By demonstrating that emotions influence how and about what we think, and what we do, the authors effectively challenge a prevailing disposition of the research community to set aside the complexities of emotional dynamics or to conceive of emotions as distorting agents in judgmental processes. Rational choice is the proverbial straw man in this regard, because it generally defines human affect as endogenous. Marcus, Neuman, and MacKuen argue persuasively that emotional engagement motivates individuals to think more deeply about political affairs and thus enhances rational choice.

The first half of the book is devoted to the first objective, which details the traditional understanding of emotions: We think first and then feel. Moreover, emotion and cognition are thought to work in opposition. A passionate citizen is unlikely to be a deliberate one. Emotions appeal to the heart, not the mind. Against this backdrop of scholarly consensus, the authors posit that emotions activate reasoned consideration. Emotions draw greater attention to environmental circumstances, which allows people to process information required for a rational choice. The authors invoke Herbert Simon: "The human organism living in a demanding environment requires an interrupt mechanism to redirect human attention to higher priority real-time needs" (p. 7). This "interrupt mechanism" is key to *Affective Intelligence*.

Drawing heavily from recent literature in the neurosciences, the authors advance a surprisingly straightforward picture of affect-driven judgments. We possess two emotional subsystems that enable us to rely on habit and to activate reasoned consideration, each in the appropriate circumstances. These subsystems are labeled disposition and surveillance; the former manages our reliance on habits, and the latter identifies circumstances that require a shift from habit to reasoned consideration. The disposition system monitors the relationship between our behavior and its expected execution. If behavior matches expectations, emotions of satisfaction and enthusiasm are forthcoming, and the behavior is reinforced and ultimately becomes habitual. By contrast, the surveillance system monitors the environment for novel or threatening stimuli. When such a stimulus appears, emotions of anxiety and unease arise, dependence upon habitual routines declines, and individuals are more likely to attend to the relevant stimulus. Anxiety, like Simon's interrupt mechanism, triggers human attention to threatening

elements in the environment. Both subsystems are subconscious emotional responses, and surveillance has clear importance for evolutionary survival.

Various political events and symbols trigger the surveillance system, but an obvious threat comes from the economy. The authors demonstrate that during hard economic times it is supportive partisans who are likely to feel anxiety about their president's poor economic record. A poor record threatens their habitual partisan views, and their surveillance system responds with anxiety. By contrast, opponents are unmoved, as they are neither threatened nor necessarily surprised by events. Anxious citizens are more likely to show interest in campaigns, acquire knowledge about politics, and participate in politics beyond voting. More profoundly, the authors demonstrate that anxiety about candidates leads individuals to deviate from their partisan habits and rely more heavily on policy proposals and personal qualities of candidates. This effect of anxiety is neither trivial nor exclusive to the domain of voting. The authors offer confirming evidence with judgments about the Gulf War and NAFTA. In each case a perceived threat in the political environment alters the nature of people's attention to that environment; consequently, habitual judgments are subject to change. Emotions, especially anxiety, "enhance rationality because they allow citizens to condition their political judgment to fit the circumstances" (p. 124).

Affective Intelligence is a compelling and challenging work, but there are several points with which one may quibble. First, the interpretation of what is rational may be called into question. Although rationality may assume various forms, often at the behest of the researcher, reliance on partisan cues can in fact be considered a rational strategy. Starting from Anthony Downs's classic work, a large and significant literature has demonstrated the "rationality" of cue-taking, information short-cuts, or the use of heuristics, in which partisan identification plays a key role (see Arthur Lupia and Mathew McCubbins, *The Democratic Dilemma*, 1998). By contrast, *Affective Intelligence* defines rationality as a departure from partisan tendencies. Is it necessarily more rational to use issue stances and personal qualities rather than party identification as criteria for candidate choice? Among anxious voters, *Affective Intelligence* says "yes."

The reader also may be struck by the causal connections crucial to the affective intelligence thesis. Notwithstanding the forceful arguments, it is plausible that cognitive awareness of campaign dynamics, perceived differences with one's partisan choice, or simple acquisition of knowledge may trigger the surveillance system. As in the case of a threatening economy (p. 79), anxiety may be a consequence rather than an antecedent of cognitive awareness. We thus may first consider the economy and then respond with an appropriate emotion. The authors' reliance on pooled cross-sectional ANES data does little to clarify such important issues of causal inference. Furthermore, when dynamics are explored using the 1980 ANES panel, the authors' findings are less persuasive.

I ponder the authors' assumption of linearity between anxiety and the various dependent variables used throughout the book: campaign interest, political knowledge, and the like. Without a defense of this assumption, a nonlinear hypothesis appears equally reasonable. As anxiety increases, the capacity to attend and learn may diminish. Similarly, moderate levels of anxiety may produce participatory activity, but high anxiety may well demobilize. One only needs to observe undergraduates at semester's end to find supporting anecdotal evidence. Yet, considerable detail on measurement

strategy is found in the two appendices, which will undoubtedly be important for replication and future research.

I am impressed by the magnitude of the argument and the force with which it is presented. *Affective Intelligence* speaks to a broad cross-section of political scientists; it has implications not only for campaigns, voting, and political learning but also for the myriad political topics that potentially address human emotions. But the implications for political learning are perhaps the most compelling. If, in fact, danger, threat, and novelty move people to learn and think more critically about political choices that confront them, then the character and intensity of present-day political communications, and indeed campaigns, is perhaps not surprising. Inflammatory media, negative ads, and alarmist appeals made by various interests will continue to draw the ire of reform-minded citizens and elites alike, but *Affective Intelligence* offers a clear motive for their frequency and occasional success. In spite of what we may initially believe, the evidence in this book suggests that a more threatening and dangerous political information climate will produce an electorate much closer to the ideals of citizenship—in terms of interest, knowledge, and participation—embodied in classic democratic theory.

Although confident in their thesis, the authors are nevertheless cautious and strike an appropriate balance. The fine distinction between conviction and prudence makes this book a pleasure to read and a must for those seeking the cutting edge in political behavior. The mechanistic models of human cognition have clearly influenced the field of public opinion, but it now appears that motivated cognition must be recognized in order to grasp fully the complexities of political choice. When the rational choice school responds to this book, I suspect it will be because *Affective Intelligence* awakened an emotion within.

Governing Race: Policy, Process, and the Politics of Race. By Nina M. Moore. Westport, CT: Praeger, 2000. 248p. \$65.00.

Francine Sanders Romero, *University of Texas at San Antonio*

Scholars who study civil rights policies face an inherent disadvantage. This chapter of American politics is very complex, involving elements of legislative and judicial behavior, federalism, public opinion, social movements, and so on, and works on this topic are often expected to cover too much ground. The most feasible way to approach the subject is one piece at a time, and Nina Moore ably explores one crucial component of this complex story. *Governing Race* examines the divisive effects of race on U.S. Senate consideration of key civil rights bills and on the substance of the laws ultimately passed.

Moore's premise is that issues of race are essentially ungovernable. Specifically, normal parliamentary procedures have not prevailed in the consideration of civil rights legislation by the modern Senate. The process is marked by abnormal procedures: obstructionism, Byzantine parliamentary maneuvers, and eventual concessions. All this is due to deep regional and partisan divisions over the need to create and preserve civil rights protections.

The book is clearly written, nicely organized, and easy to follow. At its core are four similarly structured chapters that trace three continuing threads—politics, procedures, and policy outcomes—from the late 1950s through the early

1990s. The concluding chapter offers additional empirical support for the assertions regarding each theme. The overall story is that the politics of race has been and continues to be divisive, on both a regional (strong at the beginning and weaker over time) and partisan (weaker at the beginning and stronger over time) basis. As a result, Senate deliberations have been uniquely abnormal. Civil rights advocates were fought at every turn by opponents and had to compromise in order to gain the support of uncommitted senators (usually moderate Republicans). These concessions were of necessity significant and resulted in policy outcomes far weaker than originally intended.

The strength of *Governing Race* is its cogent portrayal of civil rights legislative battles not as historical anecdotes but as a continuing process marked by notable consistencies. The material may not be new, but it is presented in a manner that makes the broad picture more apparent than previously. For example, the crucial role played by Republican moderates, the importance of the cloture rule, and the frequent bargaining down of enforcement mechanisms from the administrative to the judicial level (what Moore terms “constitutional redundancy”) emerge as constant and meaningful characteristics of this issue and venue over time. Moore provides an accurate and useful narrative framework for the study of civil rights deliberation in the U.S. Senate. On this level, the book is quite effective and will be a boon to professors interested in teaching a civil rights policy class from an actual political science perspective, as opposed to the largely historical orientation of most works on this topic.

Moore makes broader claims for the analysis, and on this plane she is less successful. In particular, the assertion that race is an ungovernable issue is not fully sustained. First, the argument that this analysis of thirteen civil rights bills in the U.S. Senate is generalizable to the overall governance of racial matters is unconvincing. Despite her declarations to the contrary, the Senate is too distinct for this claim to be made. Other institutions do manage nonconsensual issues in a more efficient way. The author's fall back position, that what happens in the Senate is meaningful simply because that body must approve all potential laws, is much more reasonable, and the entire book would have benefited from relying on that more moderate justification for the importance of this setting.

A second concern is a lack of convincing evidence that the procedures associated with civil rights bills in the Senate are in fact “abnormal.” Not enough attention is paid to such works as Barbara Sinclair's *Unorthodox Lawmaking* (1997), which suggests that putatively unusual procedures have become much more commonplace. In addition, the data mustered in support of the central argument are weak. For example, the final chapter presents figures to demonstrate that typical civil rights bills are more likely than average bills to be marked by numerous quorum and roll calls, longer debates, and so on. It is not clear what is counted as an average bill, but given the argument that civil rights deliberation is unique, a better comparison would have been to other contentious issues. That is, “average” bills may include a large number of fairly inconsequential measures and thus may be skewed toward a nonrepresentative level of consensus. Further confusing this argument is the imprecise definition of abnormal procedures. At one point, for instance, weakening amendments are referred to as an example of traditional, normal deliberation. Later, the large number of substantive amendments associated with civil rights bills are cited as evidence of abnormal processes.

The purported divisiveness of the race issue, although plausible, is not fully explored. For example, there is no

discussion of meaningful variation within the civil rights policy realm. Certainly, race is a divisive issue, but some questions are far more controversial than others, and Moore ignores the literature that suggests this. In addition, Moore never comes to terms with an ever-present foil to this argument. Despite her insistence on some endemic feature of race as the key to dissent in the Senate, she cites several examples in which uncommitted senators insisted on substantial concessions in return for their support, not because they objected to civil rights gains per se, but because of the increased federal role the original bills represented. Thus, the asserted ubiquitous lack of consensus on racial issues never becomes fully apparent.

Overall, *Governing Race* is a useful approach, with a political science orientation, to one significant aspect of civil rights policymaking in the United States. It does not completely achieve its stated goals, but scholars and students alike will learn from it and will gain a new understanding of civil rights policy creation as a systematically difficult process.

Before Roe: Abortion Policy in the States. By Rosemary Nossiff. Philadelphia, PA: Temple University Press, 2001. 195p. \$69.50 cloth, \$21.95 paper.

Raymond Tatalovich, *Loyola University Chicago*

Twenty years ago, when I coauthored a case study of the abortion controversy (Raymond Tatalovich and Byron W. Daynes, *The Politics of Abortion*, 1981), few books tackled that subject, but in the years since there has been an overabundance of books on abortion. What immediately comes to mind is: Why yet another one? Detailed case studies of pre-*Roe* abortion politics have been published on five states (California, Georgia, Hawaii, North Carolina, and Texas), and adding New York and Pennsylvania hardly seems enough justification for this book. The more important rationale for Nossiff is that these neighboring states are similar demographically (except for the Jewish presence in New York), but their abortion policies before the 1973 landmark ruling diverged sharply. New York enacted the most permissive abortion-on-demand statute in the period before 1973, and Pennsylvania tightened a restrictive but somewhat ambiguous statute.

Nossiff's objective is to analyze the interactions among the Catholic conferences, political parties, and interest groups at the mass and elite levels, based on the framing strategies by ideological adversaries, group mobilization, the antagonists' political resources, and most important the permeability of the party system. To veteran observers of abortion politics, her chronicle of events will be less interesting than her theoretical packaging of this political story. Although prochoice won in New York and prolife prevailed in Pennsylvania, Nossiff finds parallels insofar as the victors in both states fully exploited their political advantages.

Discourse looms large as an explanatory factor. For example, New York advocates shifted the framing of abortion from a medical problem to one of women's rights and, in the process, became one with the newly emerging women's movement. The Catholic opposition remained rigidly anti-abortion in its framing of the issue, with the result that in New York it could not embrace therapeutic reforms to head off demands for repeal. One troubling aspect of viewing discourse in purely instrumental terms is that the reader may forget that, for single-issue activists and most assuredly

people who are religious, ideas have worth, and they bring an intensity of commitment to moral conflicts that sets issues like abortion apart from the rest.

Why did Catholic intransigence pay off in Pennsylvania but falter in New York? The answer is the party system—Nossiff's critical variable—or more precisely the Democratic Party. Abortion pitted reform against machine Democrats in both states. But the Pennsylvania machine Democrats, in league with the Catholic Church, regained their political hegemony against reformers who operated in the elitist tradition of old Philadelphia, whereas in New York City a grassroots slugfest between factions gave rise to a strong reformist contingent within the statewide Democratic Party. Borrowing from social movement theorists, Nossiff argues that the changed political opportunity structure, manifested as intraparty rivalry, allowed the proabortionists to gain policy leverage and ultimately prevail within the New York legislature.

Nossiff states that recent cross-sectional analysis of pre-*Roe* abortion policies in the fifty states neglects intraparty competition (that empirical analysis indicates party competition was negatively related to state abortion reform). Yet, I wonder whether New York and Pennsylvania were not both outliers in the politics of abortion at the time. Most of the early abortion reform states were southern, and conservative Democrats still reigned unchallenged, although they saw fit to enact therapeutic reforms at a time when most northern two-party states did not. Furthermore, just how many states, North or South, were characterized by machine versus reform cleavages within one of the major political parties? In other words, Nossiff gives detailed accounts of what transpired in these two jurisdictions, but her key insight has limited relevance to the dynamics of abortion politics among the fourteen reformed states (ignored in her count is Mississippi, perhaps because its reform only extended to abortions for rape) and the four more states, including New York, that repealed antiabortion statutes.

The final chapter on the aftermath of *Roe* is a disappointment. We all know that Pennsylvania is a leader among the antiabortion states, so presumably Nossiff could have applied the same four theoretical prisms to explaining why each of the several post-*Roe* restrictive abortion laws by Pennsylvania found its way to the Supreme Court. One instance would be especially telling, namely, the fact that prochoice activists pressed their appeal in *Casey vs. Planned Parenthood of Southeastern Pennsylvania* (1992) for reasons (I believe) more related to their own mobilization needs than to any dispassionate assessment of their odds of judicial victory. Had Nossiff pursued this line of inquiry, she would have explored an important caveat to this literature: There are trade-offs between the choices defined by the political opportunity structure and the organizational needs of interests that pursue essentially altruistic ends. Absent that perspective, we are left with basically two more case studies in our inventory of pre-*Roe* abortion policies.

Nonetheless, this nicely crafted and extremely readable book should be considered for courses on morality policy, women's issues, and even (state) party politics.

Filled with Spirit and Power: Protestant Clergy in Politics.

By Laura R. Olson. Albany: State University of New York Press, 2000. 174p. \$57.50 cloth, \$18.95 paper.

Paul J. Weber, *University of Louisville*

Laura Olson is one of a small but energetic and influential group of Christian political scientists determined to bring

rigorous quantitative and qualitative methods to their study of religion and politics. This book fits well within that genre. It is based on interviews with forty-six Protestant pastors in the Milwaukee area between June 10 and August 16, 1994. An equal number of mainline, evangelical, and African American clergy were interviewed, and Olson provides an intimate study of how each views his or her social or political ministry.

In one sense this is a very simple, straightforward book. It explores who among these pastors gets involved in politics, how they get involved, and why. But the simplicity is deceptive. Olson writes clearly and often allows the pastors to speak for themselves, faithfully recording their insights and opinions. She then ties their views into more traditional theories of political participation and the broader religion and politics literature. The result is a readable, insightful analysis that fills a gap in that literature. Some common sense beliefs about clergy in politics are confirmed; others are contradicted.

Olson divides the respondents into three categories based on their attitudes toward the involvement of clergy in politics, that is, how they see political activism as part of their institutional role. The disengaged are deliberately apolitical; agenda-setters set the tone for their congregation and make suggestions about pertinent topics without getting personally involved; and political leaders are politically active in one form or another and see that engagement as an integral part of their religious role.

The comparison of the three types provides the bulk of the insights. The central finding is that "in an urban setting the choices pastors make about political involvement are shaped in a profound way by the socioeconomic characteristics of the neighborhood in which they serve" (p. 3). Those in depressed areas with high crime, unemployment, and need for social services are far more likely to be politically active on behalf of their parishioners and neighbors than are those in more secure and stable neighborhoods.

Some of the findings are predictable. For example, "personal resources" count somewhat; that is, pastors whose theology is influenced by the social gospel strain in American Protestantism and who feel a sense of personal efficacy in the political arena are more likely to be active. Those whose theology is more focused on personal salvation or who feel less efficacious are less active. I was surprised by two findings. Despite widespread media attention to political activism of the religious Right, evangelical clergy are less likely to be politically active than their mainline and African American colleagues. Also, denominational affiliation (and presumably related theological stances) is far less important as a predictor than the socioeconomic need of the communities served by the pastors.

Abortion is one area in which denominational affiliation is very significant. Mainline pastors are overwhelmingly prochoice, whereas evangelical and African American clergy are clearly prolife. Still, despite its enduring legacy as a politically divisive issue, a majority of respondents did not rank abortion among their most salient political concerns. On the issues of crime and violence there is a clear split among types. Pastors who are political leaders consider this a highly salient issue; the disengaged and agenda-setters do not. The disengaged are likely to point to societal causes of crime, and agenda-setters tend to attribute crime and violence to personal failure.

When asked whether the "family values" debate is an important political issue or simply a rhetorical wedge, the clergy expressed some sharp views. Those who do not con-

sider the debate politically legitimate called it either racist or sexist. Yet, somewhat surprisingly, African American pastors held the most consistently conservative views on family values, although they also saw the connections among crime, violence, and the deterioration of the family.

Within the author's intentionally limited scope, this is an excellent study, but one should be cautious about generalizing. Forty-six clergy do not create a critical mass, and the author's liberal use of percentages, gammas, and chi-squares can give a false impression of statistical significance. Also, Milwaukee is a heavily Catholic city with a significant Jewish presence as well, and the exclusion of these groups makes this a less than comprehensive study of clerical activism even in Milwaukee. But Olson makes no such pretension, choosing in-depth interviews rather than broad coverage. Taken on its own terms, this is a valuable contribution to religion and politics literature.

The Politics of Gay Rights. Edited by Craig A. Rimmerman, Kenneth D. Wald, and Clyde Wilcox. Chicago: University of Chicago Press, 2000. 460p. \$68.00 cloth, \$19.00 paper.

Charles W. Gossett, *Georgia Southern University*

It is a rare book that educates even when unopened. *The Politics of Gay Rights* is one of them. For a small lesson in attitudes toward lesbians and gay men in America, I recommend reading this book on an airplane, in a coffee shop, or in some other public setting. The reader may not encounter any direct comments from others, but there will be the occasional raised eyebrow or knowing smile; these and the reader's internalized debate about whether to turn the cover up or down when setting the book aside make one acutely aware of the controversial nature of the subject.

Kenneth Wald's introductory chapter helps place the study of lesbian and gay politics in the broader context of political science as a whole, and the subsequent chapters use a wide variety of analytic tools and approaches that reinforce the introductory chapter. The editors separate the essays into four sections.

Section I, "The Gay Movement," provides an historical overview of the development of gay politics (by historian John D'Emilio) and political organizations (Craig Rimmerman), primarily since World War II, as well as an interpretation of the political movement from the viewpoint of African Americans (Keith Boykin, formerly executive director of the National Black Gay and Lesbian Leadership Forum) and women (Jean Reith Schroedel and Pamela Fiber).

Section II, "The Opposition," looks at the religious Right's opposition to "the gay agenda" in a nuanced analysis of the political motivations and strategies used (John Green) and in a polemical assault on the religious foundations of gay rights opponents (Didi Hermann). A third chapter in this section examines the role played by citizen initiatives and popular referenda and concludes that, for the most part, such activities serve to preserve the status quo rather than cause dramatic change in either a pro- or antigay direction (Todd Donovan, Jim Wenzel, and Shaun Bowler).

Section III, "The Issues," looks at three contemporary controversies in which the role of gay men and lesbians is central. The debates over same-sex marriages and employment nondiscrimination are analyzed in terms of congressional responsiveness to public opinion (Gregory B. Lewis and Jonathon L. Edelson); the effect of AIDS on political participation, political institutions, and public policymaking

as well as on the pattern of gay politics is presented (Mark Carl Rom); and struggle over the issue of lesbians and gay men serving in the U.S. military is carefully explicated (Francine D'Amico).

Section IV, "The Arenas," examines politics at the different levels and in the different branches of American government. There are chapters on local government (James W. Button, Barbara A. Rienzo, and Kenneth D. Wald), state government (Donald Haider-Markel), Congress (Colton C. Campbell and Roger H. Davidson), the Supreme Court (Sarah Brewer, David Kaib, and Karen O'Connor), and on public opinion more broadly (Clyde Wilcox and Robin Wolpert).

The editors have put together a useful collection that analyzes lesbian and gay rights in the United States from the angle of most political science subfields. The book probably will be most useful for an undergraduate class, because many of the articles are essentially summaries of longer works by the various contributors (updated to 1999), as opposed to original research. The contributors include both academics and activists, but all can be considered advocates for advancing gay rights. Due to the quality of previous work of several authors and the relatively small number of scholars working in this area, earlier works by fellow contributors are often cited for ideas and support of the arguments.

Several essays are updated summaries of works already on their way to being classics (e.g., the pieces by D'Emilio; Boykin; Hermann; Green; Donovan, Wenzel, and Bowler; D'Amico; and Button, Rienzo, and Wald). Others are extensions of work or part of a series of studies recently begun that may become books at some point in the future (e.g., the contributions by Rimmerman; Lewis and Edelson; Haider-Markel; and Wilcox and Wolpert). As a result, most of this work has been extensively reviewed and defended in other forums.

In several instances, the authors could have made better use of the ideas expressed in their colleagues' contributions to this volume. For example, John Green carefully distinguishes among different strategies used by the Right, whereas Didi Hermann describes the Right as fairly monolithic. Likewise, Rimmerman's article on strategies employed by lesbian and gay political organizations might have included some comparison to the Right's strategies identified in other chapters. It would have been interesting to have Keith Boykin's response to John D'Emilio's questioning of the usefulness of "community building" as a strategy for advancing gay rights in the twenty-first century. Could it be that the black lesbian and gay movement is on its own trajectory?

A couple of the articles are frustrating because it seems the authors do not have a good understanding of some basic political facts of life. For example, Schroedel and Fiber take an interesting approach to surveying the policy priorities of lesbian and gay public officials in an effort to identify gender-based differences, but they neglect to control for the level of government or the nature of the political office occupied. Also, the collection would have benefited from at least two more chapters, one that places gay politics in the United States in a comparative international context, in Section I, and a chapter in Section IV that examines the effect of this issue on executive leadership and the bureaucracy.

If you want students to gain a solid understanding of the current state of lesbian and gay politics, this book is a good choice. And they can learn almost as much just from carrying it around.

Theories of the Policy Process. Edited by Paul A. Sabatier. Boulder, CO: Westview, 1999. 289p. \$70.00 cloth, \$29.00 paper.

Stella Z. Theodoulou, *California State University, Northridge*

Paul A. Sabatier contends that it is vital to find some way to simplify the policy process in order to understand fully the complexities of policymaking and the wide plethora of actors involved. He advocates a "multiple lens strategy" (p. 6). Such an approach requires researchers to apply several theoretical frameworks rather than only one. This allows a situation to be looked at from several different perspectives. Seven of what Sabatier terms the most "promising" theoretical frameworks (p. 7) developed over the last fifteen years are presented in this volume by some of their leading proponents. Sabatier charged these authors with the task of synthesizing and evaluating a particular theoretical perspective that they believe best explains how public policy is made. Overall, the intention is to provide readers with better theories to understand the policy process in contemporary societies.

The seven frameworks are organized in four sections around certain themes. The first section comprises an introductory chapter by Sabatier and a second chapter by Peter deLeon that reviews the literature on the stages heuristic. Such a perspective explores the policy process by dividing it into consecutive stages, such as agenda setting, formulation, and implementation. The second section explores the role and effect of rationality in the policy process. Specifically, the institutional rational choice framework, as conceptualized by Elinor Ostrom, focuses on how institutional rules affect the behavior of assumed rational individuals motivated by self-interest. In one chapter Nikolaos Zachariadis examines policymaking through a multiple streams and garbage can approach, that is, three streams of actors and processes and the garbage can model of organizational behavior.

The third section looks at policy change over time. In their discussion of punctuated equilibrium theory, James L. True, Bryan D. Jones, and Frank R. Baumgartner argue that policymaking is characterized by long periods of incremental change interspersed with brief periods of comprehensive and dramatic change. In the following chapter Sabatier and Hank Jenkins Smith argue that policy change is best explained through an advocacy coalition model. This approach focuses on advocacy coalitions who share common beliefs and undertake some level of coordinated activity over time.

In the fourth section a comparative perspective is provided by Frances Stokes Berry and William D. Berry and by William Blomquist. Berry and Berry discuss the policy innovation and diffusion framework, which explains policy variations across political entities through analysis of political system characteristics and different diffusion processes. Blomquist examines the funnel of causality and large *N* comparative studies. This approach explains policy variances across states through focusing on such factors as public opinion, socioeconomic variables, and political institutions.

The concluding section has two chapters, by Edella Schlayer and Sabatier, that draw the preceding discussions together. Schlayer does a good job of comparing all the frameworks and highlights their essential differences. Sabatier discusses several strategies for advancing the state of policy theory.

This collection is a challenging read and is for the more advanced student of the policy process. The discussion is too abstract and theoretical for readers without a reasonable level of knowledge of the field, but each chapter is well written and clearly structured. All the authors do a good job of presenting the evolution of each framework and discussing

its underlying principles and propositions. Recent empirical evidence and reasoning are also covered. The collection provides a serious examination of the strengths and weaknesses of each framework and offers suggestions for future developments.

The greatest contribution of this volume is that it compiles in one place some of the most interesting theoretical frameworks in the policy process literature. Some might disagree with the choice of frameworks, but Sabatier summarizes several others that were judged less promising because they are not as complete as the seven selected. The strengths and limitations of each framework are specified, so readers can decide which "repertoire" of two or three frameworks that they want to employ in their work (p. 13). The volume gives a broadly based perspective of the policy process that allows for consideration of a multitude of ideas and issues.

The volume's shortcoming is its bias toward North American literature and examples. Some noteworthy European research has been done over the same period. This criticism is a minor quibble, however, and without doubt this is an indispensable guide for those interested in the policy process and current theoretical developments. The volume is well organized and could be used to supplement a general public policy text in upper division and graduate courses. The essays are stimulating and useful and should be widely read by policy scientists. There is much in this volume to debate, which is clearly the editor's mission in producing it.

The Missing Middle: Working Families and the Future of American Social Policy. By Theda Skocpol. New York: W.W. Norton, 2000. 207p. \$25.95.

Benjamin I. Page, *Northwestern University*

Theda Skocpol's work on post-Civil-War pensions, programs for mothers and children, the New Deal, the G.I. Bill, and the Clinton health plan have convinced her that it is possible to enact broad, generous social policies in the United States, but not on a European model. Her formula for success involves social alliances across class, race, and generation; the linking of benefits with contributions, especially work; central roles for voluntary associations; and the availability of reliable and expanding public revenues.

In *The Missing Middle*, Skocpol articulately addresses contemporary policy issues concerning the plight of low- to middle-income working parents and their children. She describes the new world of highly stressed families with a single parent or two working parents, and she argues that U.S. government policies do reasonably well for the elderly but are quite incomplete or out of date with respect to parents and children. Skocpol offers some thoughts about why this is so and suggests policy changes to remedy the situation.

Chapter 2 outlines cases in which broad social alliances and voluntary associations have achieved generous, inclusive social policies. It declares that we have veered off course by trying to help only the elderly and the very poor, which has alienated the middle of society and permits antigovernment forces to promote generational warfare, demonize "welfare," and slash taxes so as to squeeze social spending. Chapter 3 lauds Social Security and Medicare as inclusive and highly effective programs, notes the still-precarious situation of many oldsters, and maintains that nationally shared values and interests, rather than sheer "gray power," explain the potency of these programs.

Particularly useful is chapter 4, which offers a concise but well-documented introduction to the stark social realities of many U.S. children and their overstretched parents, who earn

low wages, have inadequate day care or health coverage, and can find scant time for parenting. It argues that U.S. tax, labor, and social policies not only offer little help but also are designed as if every family still can and should include a spouse staying at home.

Skocpol offers a series of policy suggestions for a "family-friendly" America, including G.I. Bill type education and training loans from Social Security surpluses; assured child support (paid by absent parents plus general tax revenues); extension of Medicare to younger and younger groups; and expansions in earned income tax credits, the minimum wage, family leave, and day care. To fund this she would make the income tax more progressive, contrary to the recent regressive cuts, and remove the "cap" that exempts high incomes from payroll taxes. She maintains that Americans will support and enact such a program if a family-oriented moral vision is forwarded in national debates, if the Democratic Party is revitalized from the grassroots, and if broadened membership groups of working parents pressure politicians.

Skocpol's diagnoses of problems are quite compelling, as are her policy proposals. Some progressives may dislike the critiques of liberal advocacy groups and the nods to conservatives, especially the assessment that single-parenthood tends to be bad for children (tempered, to be sure, by vigorous refutation of right-wing notions concerning just why U.S. families are fragile and what should be done about it). But her centrist, inclusive rhetoric is likely to be more effective in the United States than, say, overtly class-based appeals would be.

The sketchiest sections of the book are those dealing with the political causes of present U.S. social policies and with political strategies for the future. The crucial role of racism as a tool for dividing working Americans is barely mentioned (e.g., pp. 51, 109). Oddly absent is the part played by sexism in policies unfriendly to single parents. More than in her previous work, Skocpol acknowledges the powerful role in American politics of big money, a "shrinking, class-tilted electorate," and an "increasingly class-biased political universe" (pp. 7-8, 54). But this analysis is not connected with specific policies or carried through into the discussion of political strategy.

How, exactly, can grassroots movements (pp. 167-9) be formed, and how can they counter the major investors who fund, and often steer, the Democratic Party? Perhaps campaign finance reform deserves discussion. How, exactly, can "actual groups" of busy parents involved in day care centers, church groups, and after-school sports (p. 170) be brought together into a politically potent national civic association? And how can low-income citizens be mobilized to vote?

Of course, we should not expect everything from this slim volume, which does accomplish a great deal. Its moral vision and policy ideas deserve a broad audience among the general public as well as political scientists and policy experts.

Contemporary Controversies and the American Racial Divide. By Robert C. Smith and Richard Seltzer. Lanham, MD: Rowman & Littlefield, 2000. 157p. \$65.00 cloth, \$23.95 paper.

Theodore J. Davis, *University of Delaware*

Race continues to be a source for major cleavages in American politics. Robert Smith and Richard Seltzer, two of the prolific scholars in the study of African Americans' public opinion, have continually worked to enhance understanding of race, class, and culture as factors influencing that public opinion. In *Contemporary Controversies and the American*

Racial Divide they continue that pursuit by examining race-based differences in public opinion.

Smith and Seltzer make use of forty-seven surveys and polls in addition to the 1996 General Social Survey to analyze such topics as attitudes toward the Persian Gulf War, conspiracy theories, several high-profile court cases involving prominent blacks, and individual perceptions of several leading African Americans. To ascertain the magnitude of difference in the opinions of blacks and whites, they describe variations in terms of gap (10–20%), gulf (20–40%), and chasm (plus 40%).

The authors suggest that a full understanding of differences in the public opinion of blacks and whites requires recognition of different histories, cultures, and material circumstances between the two groups. They point out that the historical considerations essentially revolve around unequal treatment by the Constitution and by statutory law at all levels of government. Cultural considerations are based on partially distinctive attributes that constitute a black culture. They identify specific attributes that influence black public opinion: shared values among blacks, religiosity, cynicism, suspiciousness, and alienation. They note that one must keep in mind that culture influences black-on-black as well as black-white interaction. They also identify several social factors that define black culture, such as education, employment, income, poverty, wealth, and residence.

Chapter 2 focuses on politics, ideology, and partisanship. Smith and Seltzer indicate that race matters on racial issues, but they emphasize that significant differences among blacks on these issues should not be ignored. They found that differences between blacks and whites on nonpolitical issues are modest and sometimes nonexistent. The greatest difference concerns ideology and partisanship.

Chapter 2 examines the racial divide in public opinion on foreign policy matters. Smith and Seltzer note that little is known about African American attitudes on foreign policy, which they attribute to the neglect of blacks' role in international affairs. They establish that significant differences between the races on foreign policy issues are not as large as the differences on domestic issues. Overall, they conclude that structural and historical factors provide African Americans with different perspectives from which to frame attitudes on foreign policy and military matters.

Chapter 4 probes views of blacks and whites toward three very different black leaders (Louis Farrakhan, Colin Powell, and Clarence Thomas). The aim is to examine how blacks and whites evaluate these men, who represent divergent

ideological and institutional roles in society and in the black community. The objective is to go beyond the traditional examination of issues and ideology to explore perspectives on leadership. This is a particularly interesting approach in light of what Seltzer and Smith describe as the conundrum of post-civil rights era black leadership.

Chapter 5 deals with racial differences in attitudes and opinions related to conspiracies and rumors of conspiracies. Smith and Seltzer justify this examination by noting that blacks tend to attribute much of their subordinate position in society to a white conspiracy. They conclude that blacks are more likely to believe in rumors and conspiracies than are whites.

In chapter 6, Smith and Seltzer look at opinions of several high-profile cases and conclude that blacks and whites differ in their views of the justice system depending on their opinion about continuing racism within the system.

Smith and Seltzer give those who are interested in studying the public opinion of African Americans a place to begin. There are no new revelations about black public opinion and the differences between the races, but the authors effectively bring together various aspects of the literature and provide a common explanation. In doing so they conclude that the formation of black political opinion must be understood in terms of the historical roots of slavery and its psychological, cultural, economic, and political manifestations in late-twentieth-century America. Furthermore, they note considerable consensus among blacks and whites on many issues, whatever the differences between the two groups as to the causes and solutions to racial inequality.

Smith and Seltzer assert that their methods and data nicely address the framing problem because their study does not involve a single survey at a single point in time. Yet, the weakness of the study is the use of more than forty different surveys to analyze race opinion. Furthermore, the vast majority were not designed to tap differences in the public opinion of blacks and whites. These studies enable the authors to ascertain differences in opinion but do little to advance Smith and Seltzer's explanation of why such differences exist. The samples were small in many instances, and the issues had greater local than national significance. This study illustrates the need for more comprehensive surveys specifically designed to probe the extent to which factors such as historical events, religion and culture, and perceptions of racism explain the American racial divide. Despite these shortfalls, the book is an excellent introduction to the study of the racial divide in public opinion.

Comparative Politics

Renovating Politics in Contemporary Vietnam. By Zachary Abuza. Boulder, CO: Lynne Rienner, 2001. 271p. \$52.00.

David Elliott, *Pomona College*

Zachary Abuza has written a valuable and timely study of Vietnamese politics. The book is rather narrowly centered around an examination of dissent, but that has important implications for the larger political sphere. Although the focus is on contemporary Vietnam, the author maintains that today's dissidents cannot be properly understood without considering their historical antecedents: the protest of intellectuals in the mid- and late 1950s against party domination of literature and the arts, as well as the mid-1960s split within

the party about the relationship between socialism and the war, the so-called Hoang Minh Chinh affair. The first, according to the author, not only provided a model and precedent for later dissidents but also established the subservience of all intellectual life to the dictates of the communist party. The second ended "democratic centralism" and tolerance for debate even within the party, although the question of inner-party democracy resurfaced in the 1990s.

Abuza details the various forms of contemporary dissidence in separate chapters on democratization and legalization, intellectual freedom and freedom of the press, the internal dissent of former southern guerrillas in the late 1980s (the Club of Former Resistance Fighters) against the policies of a northern-dominated party central leadership, religious freedom, and internal dissent within the party. He

concludes that the short-term effect of these various dissident voices is limited.

Furthermore, most dissidents do not challenge the fundamental structure of the regime but instead want more pluralism and debate within the party itself. The problem, Abuza argues, is that the party is unable to “renovate,” a term that became the watchword of the economic reform process started in 1986 but was never really applied to the political sphere. The author ascribes this immobilism, in part, to the fact that the party is structured by clientalism but operates on the principle of consensus. Moreover, the party’s continued monopoly on information has prevented accountability (p. 238). This fosters a vicious cycle: Without being checked by a relatively independent press, corruption grows, which undermines the legitimacy of the party built up at such cost during its long independence struggle.

Abuza sees some response from the Vietnamese authorities to external pressure on human rights, but “despite foreign pressure for political reform and a loosening of the VCP’s [Vietnamese Communist Party] hold on power, the pressure for democratization and political reform has come from within. Exogenous forces will continue to be more of an annoyance to the regime than a force for change. Pressure for political liberalization has to come from within the party itself” (p. 231). His conclusion is that “the dissident community will remain small and besieged by a hostile regime until civil society becomes more developed. Until then, the VCP will not face any credible threat to its power” (p. 239).

These are informed and acute observations, and I generally agree with the analysis. I also am impressed by the compilation of supporting material. At the same time, this important book should be an occasion for those who study the Vietnamese political system to reflect on the both the inherent and self-imposed limitations of this work. It is extremely difficult to do extensive first-hand research on most sensitive areas of Vietnamese politics, so Abuza, like most others in this rather small field, relies almost exclusively on secondary material, typically newspaper accounts, and dissident publications outside Vietnam. He also employs revealing primary sources to good effect, but these are mainly produced by a handful of the dissidents who are the main focus of this book. And the volume of dissident writings that reach the outside world is not large. The author counts a total of about thirty dissidents, of whom fifteen were party members at one time. Only eleven of the secular dissidents were sentenced and imprisoned (p. 215).

Primarily a factual narrative, the book is organized around different segments of the dissident community. (It would not be accurate to call them groups; with the significant exceptions of religious dissidents and the defunct Club of Former Resistance Fighters, they are mostly isolated individuals.) Underlying concepts and assumptions could be more fully developed. For example: “Much of the problem is the fault of the Vietnamese political system. Vietnamese politics are highly factional, yet still operate on the basis of consensual decision making” (p. 242). This important insight helps explain both the frustration of the dissidents with the system and the limits of their ability to influence it. It is so central to the book that one wishes the author had developed this analysis. Another issue that might have been explored further is the religious dissidents, who are quite different in character and effect from the intellectual dissidents—so much so that it would be helpful to know more about what these differences imply. The analysis tends to lump all who challenge the status quo into one large group, consisting of separate but related subspecies.

What sets the dissidents apart from more mainstream critics is that they extend their challenge to the power holders

beyond disagreements about strategies of economic renovation into the political sphere, and it would be helpful to have more discussion of how the dissidents view the connection between the two. There is some coverage of this (e.g., pp. 90–2), but the argument is not pursued in depth. Furthermore, an important issue raised by the author is why the dissidents have emerged now. His explanation somewhat undercuts a basic premise of the book, that there is a linear connection between dissent in the 1950s and 1960s and in the 1990s. He implies that there is now a “critical mass” of dissidents (p. 29), although the numbers cited above do not seem to bear this out.

More fundamentally, Abuza notes the importance of the collapse of communism, the emergence of the “Asian tigers” as alternative models of development, the slowdown of Vietnam’s economic progress in the late 1990s, abuses of power dramatically revealed by the Thai Binh peasant protests, and the widespread reaction against corruption in official circles. None of these developments would have been conceivable in the 1950s and 1960s, and the political agenda is very different now, which seems to bring into question the idea of tracing the roots of today’s dissidents to the earlier periods. The case of Tran Do, expelled from the party for his blunt criticism of its power monopoly, is one illustration of the pitfalls of the continuity argument. During the war he was a committed military leader and intolerant of any deviation from strategy devised by the top levels of the party. In the early 1980s he wrote books and articles on “decadent culture” during his stint as minister of culture. Still, Abuza makes a good case for the argument that nothing is ever forgotten in Vietnamese political life, and old issues do have a way of reemerging in slightly different guise.

The comprehensiveness suggested by the sweeping title of the book is not achieved, but this is an important contribution on its own terms. Yet, a better understanding of the mainstream of Vietnamese politics would make the position of those on the periphery even clearer. Perhaps a sharper distinction should have been drawn between sanctioned dissidents and those “beyond the pale,” along with a more sustained explanation of how and why this line is drawn. Nonetheless, Abuza has produced a very significant study, and it adds to our understanding of the murky terrain of Vietnamese politics. Although the approach is not comparative (it would be helpful to examine the similarities between the clientalist-consensus model of Vietnam and similar systems in other Asian states), the book is well suited for courses on Asian politics. We should be thankful to Lynne Rienner Publishers for making this work available, although the quantity of minor typographical errors is irritating and, on some occasions, sows confusion about Vietnamese names.

To Vote or Not to Vote: The Merits and Limits of Rational Choice Theory. By André Blais. Pittsburgh: University of Pittsburgh Press, 2000. 242p. \$45.00 cloth, \$19.95 paper.

Carole Jean Uhlaner, *University of California, Irvine*

André Blais takes aim at the disjunction between the prediction from rational choice theory that few people will vote and the fact that many do. He makes a major contribution to the debate over the appropriate use of rational choice theory in the study of political behavior by carefully applying evidence, including some from very clever original studies, to this voting paradox. The book is organized around the common expected utility form of the rational choice model: A rational person will vote if the benefit from having the preferred candidate win (B) times the probability that the person’s vote

changes the outcome (P) is greater than the cost (C) of voting. Blais considers alternative formulations, notably min-max regret, and dismisses them on theoretical and empirical grounds. He pays considerable attention to sense of duty, D , which often appears as a fourth variable. Blais argues, however, that this and other consumption benefits “fall outside the rational choice model” (p. 5); “that some people vote out of a sense of duty does not support or invalidate the model” (p. 5). He does not address other forms of collective action, about which rational choice may tell us more. As the author acknowledges, “voting is a tough test for rational choice,” since “voting is a low-stake decision” (p. 140).

Blais concludes that “rational choice can contribute to understanding why people vote or abstain but its contribution is quite limited . . . in three ways” (p. 137), which reflect Blais’s empirical findings. First, the model does not apply to persons (half of the electorate, he estimates) who have a strong sense of duty; these people ignore benefits and costs and vote out of moral obligation. Second, among the rest, he argues that although P , B , and C have the predicted effects, the magnitudes are small. Third, people use P in ways inconsistent with the calculation of expected benefits. Yet, the model has theoretical utility, especially since it receives partial empirical confirmation. “The bottom line is that I cannot personally make sense of why people vote without taking into account the rational choice model” (p. 140). In particular, the model pushes us to examine why people vote by showing why voting is problematic. Blais places duty foremost among these reasons. “The major motivation that leads most people to vote is the feeling that if one truly believes in democracy one has a moral obligation to vote. Oddly enough, rational choice may have facilitated the due recognition of this quintessentially irrational factor” (p. 140).

Blais builds his argument by examining “whether each of B , P , and C has independent effect on the decision to vote” (p. 15). His first steps are interesting detours from this effort. He begins with an explanation of cross-national variations in turnout that identifies the usual socioeconomic, institutional, and party factors, but he adds to the literature by doing so across a set of countries much broader than that examined by others. His comparison of turnout fluctuations by type of election (local, national, presidential, legislative, referenda) enters the main argument to provide weak support for a B effect. This section of the book is the least convincing in light of the large number of uncontrolled factors.

The next brief chapter uses cross-national survey data to recap typical conclusions on the demographic characteristics that distinguish voters from nonvoters. Blais then moves into the heart of his analysis, with one chapter each devoted to the evidence that P or C (or subjective perceptions of P or C) affects the decision to vote. Another chapter assesses the extent and effects of D and its interaction with perceived costs and benefits. In the penultimate chapter, Blais extends the generalizability of his findings by placing them in the context of the literature on the logic and incidence of free-riding. (In the context of voting, an abstainer is a free-rider.)

The middle chapters are built around two nonrandom studies and three representative sample surveys conducted by Blais and his associates. Blais and Robert Young administered questionnaires to 1,459 social science students at the Université de Montréal and the University of Western Ontario after (and, for half the sample, also during) the 1993 Canadian federal election. In a second study, 125 Université de Montréal students were interviewed in early 1995 with special attention to their estimates of P and its importance. The two major random surveys, with about 1,000 respondents each, sampled the Quebec and British Columbia electorates,

the former during and after the October 1995 sovereignty referendum and the latter in the last week of the May 1996 provincial election campaign. Finally, in spring 1996 a small (108) sample of regular voters in the Montreal area were probed on their reasons for voting.

These studies provide richer measures than previously available of the perceived cost of voting (C) and of the perceived probability of changing the outcome and related probability variables (P). Blais convincingly shows that cost does affect turnout, but the small effect is further reduced because people perceive very low costs. Turning to probability, he concludes that direct estimates of P are only weakly related to respondents’ perceptions of the election’s closeness. Moreover, only perceived closeness relates to turnout. “People . . . rely on the vague notion that their vote might count more in a close election rather than on an estimate of the probability that their vote could be decisive” (p. 78).

As Blais acknowledges, however, the circumstances of the 1995 Quebec sovereignty referendum may have misleadingly depressed the apparent effect of P . First, the outcome was expected to be and was in fact extraordinarily close (50.6% “no” to 49.4% “yes”). Second, the stakes were extraordinarily high (high B); had the results been “yes,” Premier Jacques Parizeau would have immediately moved the province toward separation. Of course, these are circumstances under which the rational choice model predicts high turnout. In fact, the turnout was extremely high (93.5%), so there was little variance to explain.

The circumstances in Quebec in 1995 also affect Blais’s later findings on the importance of duty. The stakes and the closeness of the referendum guaranteed that everyone from political leaders down to friends and neighbors would invoke a heightened sense of duty to vote. The conclusion that this overwhelmed other components of a rational model for the 1995 referendum might not generalize well elsewhere. And in Blais’s data, the vote decisions of even those 1996 British Columbians with a high sense of duty responded to B and C , as the model predicts (although not to P , which mattered only for those with a low level of duty). In the 1993 student data, B , P , and C matter for those with a low level of duty and not for those with a high level, but also the Quebec students were more likely in either group to vote than were those from Ontario. That geographical difference is left unexplained by Blais. It would be useful to see the entire model estimated separately for the Quebec and the Ontario students, as duty, reinforced by exhortation, might matter more for the former than for the latter.

The rational choice model does explain turnout decisions at the margin, especially among those with a weak sense of duty. As Blais notes, the model then points us to fruitful areas for further inquiry. He urges deeper investigation of the sense of civic duty, including qualitative research and panel studies, and replication of his surveys in other political contexts and institutional settings. For example, does P work similarly in proportional representation electoral systems? Perhaps most intriguing, the analysis implies a need to investigate which factors cause the level of duty and its effects to rise and fall across time and space. For example, Blais accounts for the low turnout in municipal elections by arguing that his data suggest “the moral obligation to vote is believed to apply to all elections in which the stakes are deemed to be relatively important” (p. 110). But circumstances, including deliberate actions by leaders, can affect the perception of importance and the salience of duty itself.

As Blais writes, further research would be useful. In contrast to many such exhortations, his call comes attached to a blueprint. That specificity is especially important in this

field, where even scholars of an empirical bent have found theorizing far easier than testing. Blais has made an important contribution in demonstrating how to do this research and in clarifying which questions are worth addressing.

Engendering Citizenship in Egypt. By Selma Botman. New York: Columbia University Press, 1999. 141p. \$47.50 cloth, \$18.50 paper.

Diane Singerman, *American University*

In the last decade scholarship on gender and politics in the Middle East has mushroomed and includes many good monographs on these questions for particular eras and in particular places. Botman paints a broad picture of social, historical, cultural, and political changes that have influenced the struggle of Egyptian women for voice and representation throughout the twentieth century. In particular, she adds more detail about the participation of leftist and communist women in Egypt's political life than is available elsewhere.

The author adopts a constructivist position, seeking to understand the experience of citizenship for women in the context of Egypt's socioeconomic and political system and the patriarchal nature of family and public life. Herein lies part of the tension within this book. It tries to bring together a constructivist approach and a political culture orientation that is shaped by modernization theory. The constructivist voice examines the ways in which three different political eras—"postharem" nationalist and semiliberal constitutional politics, the state-run socialist Nasserist period, and the years of Sadat's and Mubarak's open door economic policy—affected women's experience of citizenship. Botman presents important information about how changing national priorities and statist ideologies, as well as the larger regional political context, shaped the direction of the women's movement and gender relations in Egypt.

Yet, the modernization approach entails a rigidly dichotomous interpretation of public and private realms. Botman places women firmly in the private realm and argues that this explains their subordination and lack of full citizenship rights. Egyptian men, she asserts, are the undisputed household head, and women's "natural" primary identities are as wives and mothers. At times she argues this is due to the Egyptian state's adoption of liberal politics and refers to Carole Pateman's arguments about women's exclusion from the liberal notion of citizen. At other times, however, Botman adopts a third political culture approach and identifies Islam as a source for women's subordination through its legal statutes and religiously defined gender norms. She suggests that only by democratizing the family and jettisoning conventional social and cultural (read: Islamic) beliefs will gender relations become more egalitarian and will women become full citizens. Eclectic explanations for complex problems are warranted at times, but here they confuse the analysis.

Botman insists throughout her analysis on a wide gulf between public and private spheres in Egypt. Yet, it is quite commonly accepted that Islam does not replicate the liberal dichotomy between public and private. As the author herself points out, Islam is concerned with the maintenance of social order and gender relations and has many laws that delineate proper relations of the most intimate nature between men and women. The writ of the state in an Islamic society does not stop at the family as it does in a liberal order. Indeed, the stability of the family and gender relations are deeply important to the Islamic state. Although nation-states such as Egypt have largely secularized their legal systems, which borrow heavily from colonial powers, statutes that regulate

marriage, divorce, child custody, and inheritance are still largely based on Islamic law.

Certainly, it is possible and essential to recognize the importance of Islamic law and gendered norms in the struggle for women's equality, but not because of the vast gulf between public and private realms. Botman even mentions in passing some of the many Muslim women and various organizations that have used new legal and historical scholarship to create the grounds to contest Islamic law from within its epistemological foundations. Scholars such as Amina Wadud, Fatima Mernissi, and Azizah al-Hibri and activists with transnational networks from diverse ideological, disciplinary, and religious perspectives have been reinterpreting religious and legal texts to defend and promote women's rights.

Botman misses a critical point that surrounds this debate. Yes, men have been activists and politicians in the Muslim Brothers and the Communist Party and have served as officials of the Egyptian monarchy as well as the Nasserist and post-Nasserist states. Men continue to rule Egypt, but they are also quite far from being citizens with full rights to political representation, voice, and access to civil society. Is the male exclusion from full citizenship throughout different forms of rule in Egypt also due to culture, Islam, and liberal ideology? Only in the last few years, have Egyptians won small steps in their courageous struggle to achieve political voice and representation. The women's movement, as Botman points out, has met with only marginal success, but one can make the same argument about the Muslim Brothers, the communists, and even the wealthy entrepreneurs of the Mubarak years. The power of the state to constrain political life remains formidable. As political opportunities arise, which they do periodically in contested parliamentary elections and elsewhere, female candidates and elite women make gains in promoting equality and legal reforms. Certainly, Egyptian women are constrained by patriarchy, and men are in positions of power across the political, economic, and religious spectrum, but the agency of women and the strategies they have used and continue to use to promote their interests, whether grounded in Islamic, liberal, or developmentalist discourse, deserve more space in this book.

Elections in Australia, Ireland, and Malta under the Single Transferable Vote: Reflections on an Embedded Institution. Edited by Shaun Bowler and Bernard Grofman. Ann Arbor: University of Michigan Press, 2000. 293p. \$59.50.

Richard L. Engstrom, *University of New Orleans*

Shaun Bowler and Bernard Grofman remind us that electoral systems are "just one piece of a wider democratic puzzle" (p. 270). The theme of their edited volume is a well-known message: Election systems are embedded in much larger social and political contexts, and their effects on election outcomes are "conditional and contingent" (p. 3). This is a message too often forgotten by political scientists. It is still common to see features of electoral systems inserted into analytic models as if they are separate and distinct independent variables with the same direct, uniform effect across diverse sociopolitical settings. Contributors to this volume show that such models usually need respecification.

The election system examined is the single transferable vote (STV), often called "preference voting" by American electoral reformers. The book itself is organized according to a "most similar systems" design. The topics are the use of

STV, or some variant of it, to elect members of the Irish Dail (by three Michaels at Trinity College Dublin—Gallagher, Marsh, and Laver); the Maltese House of Representatives (by Wolfgang Hirczy de Mino and John C. Lane, as well as Gallagher); the Australian Senate and some state legislative chambers in Australia (by Colin A. Hughes, Ben Reilly, and Michael Maley); and in the past some municipal governing bodies in Canada (by J. Paul Johnson and Miriam Koene). In addition, contributions by David M. Farrell and Ian McAllister and by Neal G. Jesse are not country-specific but contain information about some of these settings. The idea behind the volume is to begin searching for “other causal factors” that might explain the differences in the experience with STV across these settings, particularly the way in which political parties have adapted to this system (Bowler and Grofman, p. 14).

The chapters, however, are not a series of comparative case studies that examine, in the same systematic manner, how any of these “other causal factors” operate across settings. No structured research design was imposed on the contributors, who use different methods to focus on different dimensions of the experience with STV. The result is eclectic. Some chapters are very historical in focus and rich in detail (e.g., Hughes). Some provide empirical tests of theoretically derived hypotheses (e.g., Gallagher). Others demonstrate that, given some specific assumptions about the information voters have and their subsequent behavior, election results are consistent with expectations derived through formal theory (e.g., Laver). This diversity in approach seems well justified, given the state of our knowledge about these systems and settings, but the search for the other causal variables, from setting to setting, is less than directly comparative. The introductory and concluding chapters by Bowler and Grofman, however, do provide excellent overviews.

Many readers may be surprised by the variation in the way STV has been implemented. Farrell and McAllister provide an overview of these variations, and many of the other chapters show that the details make a difference. Across the settings examined, the STV system is itself, in application, a variable rather than a constant, and this has an important effect on experiences with the system. The STV arrangement in Ireland usually serves as the textbook example, but it is far from representative of the others. Different district magnitudes (the number of seats to be filled) can have an effect, of course, on the translation of votes into seats, and the various rules governing how the voters may express their preferences also can have an important influence on outcomes. An intriguing case in this respect is Australia, which in 1983 added “above the line” voting (also known as ticket voting) to the STV arrangement used to elect the federal Senate. This provides the option to vote above a heavy black line for a particular political party and thereby adopt the preference ordering determined by that party. The alternative is to rank order virtually all the candidates for all the parties, listed below the line, so it is not surprising that almost all voters vote above the line. STV in Australia has been converted, in effect, into a list system of proportional representation (see the contributions of Farrell and McAllister, Reilly and Maley, and Hughes, at pp. 25, 50–3, and 162–3, respectively).

The contributions do reveal what the editors describe as a “sizable gap” between theory and practice (p. 10). This is certainly the case for the often-repeated expectation that STV will result in multiparty systems. The experience with STV has been starkly different. Neal Jesse describes STV, in the context of Duverger’s theoretical formulation about the relationship between election systems and party systems, as an “outlier” empirically (p. 59). Farrell and McAllister

identify ten distinct STV systems in use, over time, in Ireland, Malta, Australia, and two Australian states. The average number of “effective” elective parties within these systems exceeded three only once, in Ireland (it was 3.1 from 1928 through 1989). The average number of effective parliamentary parties was closer to two than three in eight of the ten cases. The exceptions occurred in that same time period in Ireland and in Malta from 1947–55, both of which were scored at 2.79. Malta, it must be noted, is now said to have “one of the purest [two-party systems] in the world” (Gallagher, p. 87; see also Hirczy de Mino and John C. Lane, p. 194).

The outlier status of STV is attributed, primarily, to the adaptation of large parties to the system, by adopting nomination and vote management procedures as well as formal systemic rules, such as ballot structures and campaign expenditure regulations, that counter any tendency toward fractionalization (Bowler and Grofman, pp. 10–11). Jesse further suggests that sophisticated voting within the electorate also dampens any such tendency.

STV, and alternative systems such as cumulative voting and limited voting, has received increased attention in the United States since the Supreme Court, in *Shaw v. Reno* (1993) and its progeny, placed new constraints on the creation of majority-minority districts (in terms of racial and language minorities protected by the Voting Rights Act). The debate over whether to adopt such alternatives could be greatly informed by this volume. The fact that the consequences of electoral system choices are “conditional and contingent rather than categorical and mechanical” (Bowler and Grofman, p. 3) is often ignored in this debate.

A quintessential example occurred in the September 1999 hearings held by the Subcommittee on the Constitution, Judiciary Committee, U.S. House of Representatives, on Rep. Mel Watt’s proposed States’ Choice of Voting Systems Act. This bill would allow alternative election systems, such as STV, to be used for elections to the U.S. House. A political scientist testifying against the bill confidently stated that proportional representation (PR), or even cumulative voting, would lead to the fractionalized politics seen in Israel. The United States “will end up with a David Duke Party, a Pat Buchanan Party, an Al Sharpton Party, you name it.” No mention was made of any other countries that employ a PR arrangement, and there was no acknowledgement that Israel is an extreme case, even among countries using a list system of PR, as does Israel. There were no references to Ireland, Malta, or Australia, despite the fact that STV is far more likely to be adopted in the United States than any variant of the party-centered list system, especially the version used in Israel.

Election systems should serve as conditional or specifying variables that affect how other variables relate to various dimensions of election outcomes. This very useful volume reinforces, once again, the need for that conceptual specification.

Between the State and Islam. Edited by Charles E. Butterworth and I. William Zartman. Washington, DC, and Cambridge: Woodrow Wilson Center Press and Cambridge University Press, 2001. 256p. \$49.95 cloth, \$18.95 paper.

Rex Brynen, *McGill University*

As the title suggests, this edited volume explores the socio-political terrain between the state and Islam, emphasizing “the forces of skepticism, liberty, and creativity that struggle

against Islamic conformism and state hegemony.” As the editors note, “however influential religion appears in word and deed, however evident the trappings of state authority, people do come into being, thrive, marry, raise families, think, laugh, and cry without regard to—indeed, sometimes in utter defiance of—the strictures of religious or state authority” (p. 4).

Set in this way, the volume positions itself to make a significant contribution to the literature on Middle East politics, which often appears to allocate excessive explanatory effect to either political culture or authoritarian state power.

As with all edited works, there are variations in how well the authors adhere to this theme and to what degree they offer additional analytical insight. In the first section of the volume, four contributors address the nineteenth century. Butterworth reviews paradigms of the state and of Islam. This is a useful first chapter, but it is neither detailed nor deep enough to add substantially to debates over how best to comprehend “Islam.” Antoine Zahlan explores technological change in Egypt. His survey is insightful, clear, and very engaging but, ironically, is perhaps the chapter least well tied to the overall theme. Serif Mardin looks at Islam and identity among the Volga Tartars. The treatment, although empirically rich, could do more to conceptualize and generalize the findings outside the case study. Finally, Said Bensaid Alaoui presents a solid overview of nineteenth-century Muslim opposition thinkers.

In the second section, the remaining six contributors explore the twentieth century. As’ad Abukhalil offers a characteristically frank and very useful examination of anti-conformist tendencies in Arab/Islamic thought, pausing on thinkers who have gone beyond traditional reformism to offer radical reinterpretations of Quranic text. Iliya Harik surveys democratic thought in the Arab world, identifying both thinkers and key (human rights and other) institutions. His critical assessment of these actors (and the potential effect of their ideas) is thought-provoking, even if one does not fully accept all his conclusions. Harik’s discussion also assumes particular importance in the wake of the arrest and conviction of Egyptian-American sociologist and democratic activist Saad Eddin Ibrahim, a regressive move clearly intended by the Egyptian authorities to intimidate human rights organizations and civil society more generally.

Ibrahim Karawan discusses the maneuvering of independent political parties in Egypt, Jordan, and Tunisia between the twin poles of an authoritarian state and an Islamist opposition, which is an important and largely unexplored process. Timothy Piro’s chapter on liberal professionals also examines an important understudied topic, although its value might have been enhanced if the scope of the treatment (which includes business associations, professional syndicates, labor unions, the press, and think tanks) had been narrowed. Piro’s discussion also could have been strengthened by discussion of the varied ideological trends within the sectors of civil society, not all of which are very “liberal.”

Jean Leca, Meriem Vergès, and Mouina Bennani-Chraïbi revisit the work of Daniel Lerner to offer an impressionistic analysis of the effect of audiovisual media in North Africa. They offer some insights, but the methodological foundations of the piece could be stronger, and the treatment itself is only weakly tied to the main focus of the book. In the concluding chapter, Zartman wrestles with the compatibility of political religion and secular democracy, and he finds such a reconciliation “impossible” (p. 243). The dualism implicit in this formulation seems to run counter to much of the spirit of the volume, his prediction—that the resulting tensions will only

be assuaged through processes of conflict, interaction, and negotiation—seems unassailable.

In view of its title and purpose, the book generally fails to engage a fairly substantial and growing literature by anthropologists and political sociologists that explores how everyday social action is shaped by folk and “formal” beliefs, institutions, and state power. This literature is particularly rich and highlights family and gender politics; Diane Singerman’s *Avenues of Participation: Family, Politics, and Networks in Urban Quarters of Cairo* (1995) is just one of many possible exemplars. Instead, despite the editors’ apparent willingness in the introduction to privilege a micropolitical focus on how people individually and collectively live their everyday lives, most of the chapters assume a macropolitical perspective that emphasizes major thinkers, institutions, and political movements.

Between the State and Islam contains much of interest. It is recommended for regional specialists and others interested in Islam or religion and politics, and it has several chapters that will be of use in graduate seminars on these topics.

Politics on the Fringe: The People, Policies, and Organizations of the French National Front. By Edward G. DeClair. Durham, NC: Duke University Press, 1999. 261p. \$49.95 cloth, \$18.95 paper.

Andrew Appleton, *Washington State University*

“The charm of history and its enigmatic lesson,” wrote Aldous Huxley, “consist in the fact that, from age to age, nothing changes and yet everything is completely different.” (*The Devils of Loudon*, 1952) Surely, Huxley would have had much to say about the role played by the *Front National* (FN) in the recent history of France. As René Rémond has reminded us over the years, the FN is but the modern incarnation of a traditional strand of the French Right. Indeed, Rémond is one of the most powerful voices behind the cultural approach to analyzing the contemporary extreme Right, arguing that any organizational manifestation of it can only be understood with reference to the historical and philosophical antecedents. Much of the scholarship on the FN either implicitly or explicitly uses this approach as a point of departure.

As the extreme Right presence has made itself felt in the politics of other European countries since the decline of the postwar consensus (such as Italy, Germany, Austria, and Belgium, to name but a few of the more obvious cases), other scholars have begun to make cross-national comparisons. In this approach, the FN is no longer simply a continuation of a peculiar French history but the logical outcome of a set of political and economic factors that affect all postindustrial nations to one degree or another. In the work of Hans-Georg Betz, for example, the FN is one extreme Right party among others; to be sure, some of the variance may be explained by system-specific phenomena, but the underlying logic flows across cases.

Into this debate comes *Politics on the Fringe*, a book that has its roots in Edward DeClair’s dissertation research conducted in France. One strength of the book is that the author acknowledges the debate over “exceptionalism” versus comparative analysis; thus, he promises to place what is essentially an interpretive case study into a comparative context by examining the FN alongside some of its European counterparts. Unfortunately, one weakness of the book is that he fails in this endeavor, despite some brief portraits of the extreme Right in Austria (two pages), Belgium (two pages), Germany (one page), and Italy (two pages). A modest attempt to draw some comparisons between the themes

promulgated by these movements is too cursory (to state that the “national identity crisis,” immigration, and the decline of the welfare state are connected with the extreme Right phenomenon is not particularly illuminating). The main contribution of the book stands in its appeal to those interested in the specific case of France.

Politics on the Fringe loosely adopts the “life-cycle” model of party development, and this leads the author to adopt two important research strategies. First, the book essentially pursues a historical-narrative thread, tracing the rise and consolidation of the FN as an electoral force. Second, the stress in the work is upon party organization as the dependent variable. Methodologically, DeClair writes in the historical-interpretivist tradition, which again injects a set of both strengths and weaknesses. Through a series of elite interviews with the top party leadership, the author aims to let the organizers of the FN speak “in their own voice.” Researchers of extreme Right politics in France may find this the most valuable contribution, as it skillfully permits the reader to gain a greater insight into the motivations and strategies of those directing the extreme Right resurgence of the 1980s and 1990s.

The drawbacks of this theoretical and methodological posture, however, are those that often blight single-case analysis. The theoretical model is never fully specified (I suspect that this was a casualty in the transition from dissertation to book), which leaves us with the bland assertion that “uncovering the variable influences of the different facilitators at each level of the development process provides important information about political party development. The National Front is a compelling test case for this model of party development” (p. 50). Yet, absent a set of logically derived, falsifiable, empirically testable propositions, the narrative does little except lead the author to conclude that the FN has indeed passed through the critical three stages of the life cycle (origins, success, and durability), and this was primarily due to Le Pen’s leadership.

Most unfortunately for DeClair, the historical-interpretive narrative of *Politics on the Fringe* becomes a prisoner of history itself. Just as the book was published, the FN was torn apart by internal struggles generated by challenges both to the leadership of Le Pen and the ideological and tactical posture of the movement. Little in the book prepares the reader for this turn of events: “Ominously, the future does indeed belong to the National Front” (p. 224). In an afterword we are informed that the FN is now in a fratricidal and potentially suicidal struggle. Although the author intelligently explains this turn of events in a few pages, the point is that nothing in the preceding pages predicts them. One is tempted to ask whether the life-cycle theory is inadequate. Should we consider stages beyond “durability”? Or perhaps the party never really achieved durability? Could other theoretical approaches provide superior leverage (e.g., a rational-choice institutional account such as that provided by John Aldrich in *Why Parties?* [1995]). Nothing in *Politics on the Fringe* enables the reader to find the answers.

One may conjecture that the twenty-nine elite interviews that form the core of the original data in the book brought the author into such close proximity with his object of study that the comparative context was somewhat lost. The reference, for example, to the attraction of a Thatcherian-style authoritarian neoliberalism for at least one of those interviewed (p. 156) is highly intriguing. It definitely echoes my own experience in interviewing party leaders in the mid-1980s, when I found a pronounced admiration for the then British prime minister; the need for a more systematic contextualization and comparison of the extreme Right

across postindustrial societies is indicated by pointers such as this. Some might quibble with the assertion in *Politics on the Fringe* that the FN has never been a violent movement; although Le Pen himself (as well as other prominent party leaders) have avoided public incitations of violence and have been careful to portray the party as operating inside the confines of republican institutions, there has been an unmistakable whiff of a culture of violence that permeates the grassroots.

Ultimately, *Politics on the Fringe* has much to recommend it to those interested in a deeper understanding of the French version, perhaps the prototypical version, of the extreme Right, and particularly to those who wish to gain an insight into the leadership of this horribly compelling movement. It cannot be concluded, however, that the book adds much to a broader understanding of the extreme Right across the postindustrial world, whether in terms of development, organization, or ideology. DeClair is to be commended for his fortitude in enduring long interviews with leaders of the extreme Right in France, but one wishes that the results had yielded rather more theoretical insight into models of party development and extreme Right politics generally writ.

The Politics of Pragmatism: Women, Representation, and Constitutionalism in Canada. By Alexandra Dobrowolsky. Don Mills, Ontario: Oxford University Press, 2000. 320p. \$24.75 paper.

Avigail Eisenberg, *University of Victoria*

Can a group as diverse as women are along nearly every politically relevant dimension be mobilized or mobilize itself to alter the course of national politics? In this study, Alexandra Dobrowolsky traces how women shaped the Canadian Constitution from 1980 to 1992 and explains why they had the effect they did. On the basis of her findings, she aims to reformulate our understanding of how political mobilization and representation work.

Broadly, two approaches tend to dominate scholarly accounts about how groups affect national political agendas, and neither adequately explains the ways that women participate in and advance political change. First, interest group theory suggests that individuals mobilize on the basis of shared interests. Interest groups aggregate individual interests and have a temporary and focused purpose to advance these interests in the political—that is, state—arena by lobbying public institutions, building coalitions with other groups, and attempting to get as close as possible to those who hold power. Second, social movement theory focuses on the political change advanced by groups that are organized on the basis of loosely defined but shared ascriptive characteristics, such as gender or ethnicity. Civil society, rather than the state or public institutions, is the domain of social movements, and these movements generally aim at securing political change by “destabilizing the status quo.” Rather than lobby for the purpose of changing defined public policies, social movements have a strategic repertoire that includes everything from consciousness raising to civil disobedience (p. 5).

Dobrowolsky argues that Canadian women used a large variety of tactics to secure social movement ends. She traces the activities of Canadian women’s organizations, feminists, femocrats, and feminism as a social movement during and between three episodes of constitution building in Canada (approximately 1979–82, 1987–90, and 1990–92). She presents information from an exhaustive list of sources, including publications, pamphlets, briefs and documents generated

by more than 100 organizations, and numerous personal interviews with key players from each period. The result is one of the most thorough and interesting empirical accounts of national political activity in Canada over the last twenty years. The book brings this empirical account to bear on the question of how best to understand political mobilization in general. Unsurprisingly, neither an interest group approach nor social movement theory can account fully for the effects of the concerted efforts of women in Canadian constitutional politics.

That women had a profound influence is undeniable, according to Dobrowolsky. First, they succeeded in disrupting the conventional politics of federalism, which historically has ignored identities other than those organized on a territorial (i.e., federal-provincial) basis. They convinced Canadians that other forms of identity, including those of national minorities and those based on ethnicity and gender, need to be recognized and represented at the negotiating table. Second, directly through lobbying efforts and by disrupting conventional discourse, women succeeded in securing an expansive notion of equality that reconciles equality rights with difference and diversity. As Dobrowolsky argues, this expansive notion fundamentally changed not only the constitution but also the Canadian political and social landscape. As a result, it changed the context in which women mobilized.

Ironically, one way in which this expansive sense of equality changed politics and society may have been responsible for the demise in power of national women's organizations. Key women's groups weathered troubled times during Canada's constitutional episodes partly because, due to their own success at arguing for an expansive notion of equality, they were confronted by their own failure to respond adequately to the identity-related differences among women. In particular, women of color, Aboriginal women, and Quebec women compelled the movement to rethink the ways in which it represented women's interests. Aboriginal and Quebec women withdrew their formal participation from national organizations and advanced their distinctive interests and perspectives, using their own increasingly powerful organizations.

Dobrowolsky resists the conclusion that the women's movement in Canada operated by advancing vested interests of particular women, that is, white, wealthy professional women. Yet, her analysis shows that at one crucial juncture—during the 1980–82 constitutional negotiations—a network of such women were largely responsible for ensuring that equality rights were firmly entrenched in the Canadian Constitution. Since that time, the women's movement in Canada has displayed what might be seen as a healthy respect for the politics of inclusion and a strong resolve to ensure that differences among women, especially those based on linguistic, ethnic, and national divisions, are not neglected in favor of presenting a façade of gender-based unity. The price of inclusion, however, is a politics that does not attempt to unite women around a set of shared values and does not presume or expect solidarity among women in relation to any single political agenda.

For Dobrowolsky, the pragmatism of women's political activity in Canada is evidence of success rather than failure. She strongly resists the conclusion that identity politics is "fragmenting, divisive and therefore detrimental" (p. 197). She argues that concerns over identity politics are born out of a preoccupation with unanimity and neglect of the benefits of discursive exchange (p. 197). In this sense, Dobrowolsky urges readers to look at political mobilization using a differ-

ent set of standards, but she does not offer a more precise idea of what these standards ought to be.

Unanimity may not be a realistic ideal, but at what point can one conclude that a movement suffers from fragmentation? Discursive exchange is important to any democratic movement, but at some point should conversations move on; should decisions be made; and, as in the case examined in this book, should principles be entrenched in a constitution or be defeated? In Canada, the amendments in 1990 and 1992 were both defeated. Dobrowolsky provides convincing evidence that women as a social movement made a difference to each of these decisions, but her analysis raises the question of whether a more pluralistic movement wedded to a more discursive form of politics is not better suited to disrupting attempts to change the constitutional status quo rather than to advancing change. Dobrowolsky does not address this question, but her book provides a thoughtful analysis of the evidence and arguments one would have to consider in order to answer it.

Citizens, Experts, and the Environment: The Politics of Local Knowledge. By Frank Fischer. Durham, NC: Duke University Press, 2000. 336p. \$59.95 cloth, \$18.95 paper.

Sheldon Kamieniecki, *University of Southern California*

Advances in science and technology account for much of today's pollution, but science and technology are now being relied upon to reduce pollution, protect natural resources, and promote habitat conservation. As scientists, experts, and technical personnel become increasingly involved in these efforts, public values and preferences, especially at the local level, are being ignored. Citizen participation is declining, and professionals are playing a more influential role in environmental policymaking than before. Because a large majority of these professionals are not elected and are not directly accountable to the public, decision making is less democratic. With this as a backdrop, the major question this book attempts to address is how citizens can participate in an age dominated by complex technologies and expert decisions (p. 6).

This book is divided into four sections. Part 1 provides a broad discussion of the role of technology and expertise in today's society. The first three chapters examine citizen participation as both an ideology and an activity, criticize professional expertise, and call for alternative practices. The fourth chapter reviews the epistemological issues underlying the critique of empirical social science, natural science, and technology and offers a "postpositivist," discursive theory of knowledge. "Postpositivism," according to the author, "is grounded in the idea that reality exists but can never be understood or explained fully, given both the multiplicity of causes and effects and the problem of social meaning. . . . Critical of empiricism, 'postpositivism' emphasizes the social construction of theory and concepts, and qualitative approaches to the discovery of knowledge" (p. 282, footnote 1).

Part 2 contains three chapters and addresses the role of expertise in environmental policymaking, the political response of environmentalists to technocratic decision practices, and the resulting politics of "counterexpertise." Part 3 also contains three chapters and discusses the deliberative alternative, emphasizing the importance of local knowledge (frequently studied by anthropologists, such as Steve Lansing), citizens as experts, and community inquiry.

The two chapters in the final section attempt to show how the participatory inquiry of lay citizens and experts can be

employed to address complex issues and the ways that their collaborative assessments can be used to inform legislators. Determining environmental risk, both empirically and subjectively, is a central aim throughout the work. In conclusion, the author calls for a new understanding of the expert as “specialized citizen.”

Fischer primarily views the problems of scientific expertise and the need for increased levels of informed citizen participation within the context of political theory. “What the positivists have failed to grasp . . . is that scientific discourse is itself a highly interpretive enterprise. Given this interpretive dimension, science loses its privileged claim as superior knowledge” (p. 44). Hence, citizen participation is critical not only for the functioning of democracy and as a value in and of itself, but also because science is open to subjective interpretation, and knowledge is socially constructed (i.e., it is not objective or value free, and facts do not exist). This is precisely why the author believes scientific expertise and citizen participation should be treated equally in the policy-making process.

Average citizens, we are told, have the ability to understand the most complicated scientific and technical issues. Fischer recommends the establishment of “consensus conferences” (pp. 234–40), or about two dozen “ordinary” citizens who assess the science and technology related to a specific issue or problem. People who are chosen to participate in these conferences read and learn about the technical issues involved and, with the help of a nonexpert facilitator, develop a set of recommendations for policymakers and legislators. This approach has been employed in Denmark and elsewhere with some success.

Overall, the book is well written. The author effectively develops important issues, arguments, and ideas before staking out a position. Readers in all fields will appreciate the clarity of the discussions concerning the expanding role of science and technology in environmental policymaking and why citizen participation is vital, especially at the local level. The study successfully integrates central concepts and ideas in political theory with analyses of the most serious problems related to citizen participation in environmental policymaking.

A critical question Fischer skirts is whether more citizen participation leads to more effective environmental policies. Regardless of what mechanisms are used to involve the public, will the result be environmental policies that work? No study has shown that the level of public participation—regardless of how it is achieved—varies directly with the level of effectiveness of public policy. In fact, research shows that the relationship between these two variables is ambiguous at best. Although the author may feel that an increase in citizen participation is as important as (or even more important than) developing cost-effective, successful plans and regulatory programs, it is highly doubtful that politicians and policymakers will agree.

The failure of previous environmental policies has less to do with whether science is completely subjective or a decline in citizen participation and has more to do with poor policy design and execution. The extreme politicization of critical environmental problems (e.g., the protection of biodiversity and climate change) has resulted in no action or failed policies at the federal, state, and local level. In many if not most cases, the rise of interest group politics has prevented vital environmental legislation from being passed. Laws that have been adopted have not been strictly enforced.

The book also does not adequately address how to persuade citizens to become more actively involved in environmental policy at the local level. In general, the level and type

of public participation that Fischer desires takes a great deal of time and effort, and most people do not want to become involved to this extent. Why would they want to participate in fairly demanding consensus conferences? (Interestingly, the study does not explore how advanced computer and communications technology might be used creatively to include a larger number of citizens in environmental decision making at the local level in a meaningful way.) There must be an increase in feelings of civic duty and responsibility before such mechanisms as consensus conferences can be successful.

The major problem with the book is that it fails to offer significant new insights into the dilemma of citizen participation in modern society, particularly when highly technical issues are at stake. The same observations and possible solutions to this dilemma have been discussed by others as well as the author himself (*Technocracy and the Politics of Expertise*, 1990; “American Think Tanks,” *Governance* 4 [July 1991]: 332–53). At the same time, the vast literature on interest groups, policy stakeholders, agenda setting, and public participation in political science is ignored. Regrettably, despite the importance of the topic, the book does not break new ground or add to our knowledge of citizen participation in environmental policymaking.

The British Presidency: Tony Blair and the Politics of Public Leadership. By Michael Foley. Manchester: Manchester University Press, 2000. 374p. \$74.95.

G. W. Jones, *London School of Economics and Political Science*

In this updated edition of his book, published in 1993 as *The Rise of the British Presidency*, Michael Foley develops his argument and rounds on his critics. He remains committed to his central proposition that the study of American presidents reveals underlying political pressures that have transformed the British prime minister into the British president. He asserts (p. 331): “The comparability that has come to exist does so at a level that transcends the constitutional differences within the two systems.” He claims there has been a fundamental systemic change in British government brought about by irreversible dynamics in the British political system.

The drivers of the transformation are the media. They regard politics as a clash between leaders, who personalize their parties, programs, and governments. Both prime ministers and presidents deal directly with the people and power centers. In Britain prime ministers detach themselves from their parties, their cabinets, and Parliament in seeking direct links to the people both to attain and to keep office.

Foley makes a plausible case, writes persuasively in an elegant style, deploys apt quotations from his extensive collection of press cuttings, and shifts easily between U.S. and British experiences. Had he waited for the aftermath of the general election of 2001, he would have been able to reinforce his thesis. The party campaigns focused overwhelmingly on the party leaders and copied U.S. techniques, and the media concentrated on the prime minister and the leaders of the Conservative and Liberal Democrat parties. Tony Blair, once reelected, reorganized the core executive of the Prime Minister’s Office and the Cabinet Office, increasing and reshaping his staff and fusing the two offices under his control. To many commentators Blair copied the organization of the White House. The general election of 2001 and the subsequent reorganization of government resonate with U.S. analogies.

In this edition Foley covers the Blair government's first term and examines how Blair was governing. In the first edition he looked only at the campaign to obtain office, politics, images, and the media, without exploring the structures and processes of government, and especially the relationships between prime ministers and ministers. He still downplays the latter, asserting that underlying political pressures have transformed cabinet and ministerial government into prime ministerial government. He fails to appreciate that much of the high personal profile achieved by the prime minister is desired by his ministerial colleagues because it helps them and their party win elections and gain consent for their policies. The prime minister is only as dominant as these colleagues let him be. If he becomes a liability to winning elections and gaining consent, he will be constrained and ultimately jettisoned.

Foley also adds a section on four objections to his thesis. The first is that of deceptive appearances, which argues that a flamboyant prime minister can distort news coverage to give an impression of personal hegemony. The second is that of the flash in the pan, which argues that a prime minister may achieve governmental prominence, but it will be only temporary in a system that depends on ministers predisposed to collective working and that is produced from elections based on mandates for governments rather than for individual leaders. The third is that of the iron law of politics, which argues that a centralizing prime minister cannot be sustained against a political system rooted in conflict, division, and challenge that is expressed through Parliament and a cabinet dependent on always having parliamentary support. The fourth is that of the problem of precision, which argues that it is so hard to see through the complexity of data that it makes sense to stay with the generalization that most accords with traditional observation. The burden of proof lies with the innovator, and the presidential-government paradigm lacks a clinching body of evidence.

Foley's response is to emphasize that his analysis of spatial leadership, outsider politics, competitive populism, personal projection, media management, individuated party images, the permanent campaign, and new linkages between leaders and their public constituencies shows that new underlying political pressures have overturned the traditional institutions and processes of government.

The fifth objection to the Foley thesis, which the author fails to address, is that it lacks an historical perspective. Foley's analysis is limited to the period from Margaret Thatcher in 1979 and takes no account of how past prime ministers behaved. Disraeli and Gladstone personified their parties in the nineteenth century and were dominant in both electioneering and governing. Lloyd George exercised such personal leadership—breaking with his party, keeping aloof from Parliament, appealing directly to the people through most of the approaches noted by Foley, and increasing his staff—that he was known by the 1920s as an imperial Caesar. But the government of Bonar Law in 1922 dismantled Lloyd George's expanded prime ministerial Secretariat in the gardens of 10 Downing Street and reasserted the processes of cabinet and ministerial government. The institutions that make up the British Constitution were not transcended and transformed by Lloyd George or Margaret Thatcher. Tony Blair's dominance is contingent, not structural, the result of a unique set of circumstances that could be changed, above all by a downturn in the economy, failure to deliver on promises, and growing unpopularity.

The image of the elastic band to explain the British system is still apt. It stretches to accommodate an assertive prime minister but contracts to suit one less activist. Blair may be

the prime minister who has stretched it the most, but he has not yet gone as far as to break the elastic. He has not called his new arrangements in the core executive a prime minister's department because he knows that would snap the band by alienating his ministerial colleagues. Blair prefers a presidential system to a parliamentary system, as is shown also by his advocacy of directly elected mayors to replace collective and conciliar processes in local government, but he has not dared make the move that would indicate that prime ministerial government has replaced cabinet and ministerial government.

Factionalism in Chinese Communist Politics. By Jing Huang. New York: Cambridge University Press, 2000. 458p. \$59.95.

Cadres and Corruption: The Organizational Involution of the Chinese Communist Party. By Xiaobo Lu. Stanford, CA: Stanford University Press, 2000. 368p. \$55.00.

Bruce J. Dickson, *George Washington University*

These two books cover fundamental aspects of the Chinese political system that are widely acknowledged but not well understood. Factionalism has been endemic in the Chinese Communist Party (CCP) from its creation in 1921. Scholars and journalists often refer to factional strife within the party, but the contours and dynamics of Chinese factionalism remain obscure. Corruption, whether malfeasance or rent-seeking behavior, also has bedeviled the CCP ever since it came to power in 1949. The causes and consequences of corruption are better understood, but the CCP has been unable or unwilling to deal effectively with the problem, despite the damage to the regime's reputation and coherence. Both books attempt to shed new light on these enduring elements of Chinese politics.

Because there are few book-length studies on these issues and because these volumes promote fresh analytical perspectives, they are likely to be widely read. Both are notable for incorporating approaches that are often missing in studies of Chinese politics: for Lu, comparative analysis, and for Huang, rational choice. Yet, both are undermined by accounting for so little temporal or regional variation in their inquiries. Both imply that the causes of factionalism and corruption have remained fairly constant, despite the many political changes in China in the period covered by each book. They are strongest in their factual descriptions of factionalism and corruption, respectively, but the analytical perspectives adopted do not add a great deal to the explanations.

Huang intends to show that factionalism has been the primary independent variable in Chinese politics. The book covers the period between the late 1930s, after the CCP had arrived in Yan'an and Mao had attained supremacy within the party, until the fall of Hu Yaobang in 1987. The roots of the clash between Mao and Liu Shaoqi that was finally revealed in the Cultural Revolution are traced to the initial state-building efforts of the 1950s, which challenges the conventional wisdom and will likely engender debate. The 1987 endpoint is unexplained and omits the most important episode of factional strife in the post-Mao period, the Tiananmen demonstrations of 1989 and the emergence of Jiang Zemin as the top leader.

The emphasis of *Factionalism* is on an interpretation of elite politics during approximately 50 years. Much of this interpretation covers well-known events, and little new information is presented. Huang provides an extensive and able review of the differing approaches to the study of elite politics

in China. He favors the cultural approach championed by Lucian Pye and dismisses the structural approach developed by Andrew Nathan. As a result, his argument is China-specific and based primarily on the importance of *guanxi*.

Huang's interpretation reveals a large intellectual debt to his mentor, Roderick MacFarquhar, both in the numerous references to his work and his criticism of scholars who offer opposing perspectives, especially Frederick Teiwes. The debates in the book are largely over interpretation of events and personal motives, debates that are difficult to resolve because we lack definitive evidence. Huang challenges the reliability of other scholars' sources but is vulnerable to the same charge. Much of his analysis is based on speeches, memoirs, and biographies that often were published many years after the events, which leaves their veracity and objectivity in doubt. Nevertheless, he does a good job in showing how the behavior of Chinese leaders was constrained by insufficient information and awareness of options, and he brings to life some of the key personalities.

Huang develops a modified prisoner's dilemma game to describe specific episodes of factional strife, which is a potentially valuable contribution. He lays out the formal logic and potential payoffs of the players, but this seems to add little to the empirical analysis. For instance, he does not apply game theory to the account of the Cultural Revolution decade to highlight overlooked or underappreciated aspects. The ranked preferences of specific leaders during certain episodes are asserted but not demonstrated. Some examples of leaders who behaved "irrationally" would have been useful. Given the turmoil of Chinese politics, someone must have miscalculated at some point.

If factionalism is the independent variable, then the dependent variable seems to be the ups and downs of central leaders, especially Politburo members. There is little attention to how factions operate, whether they function as channels of communication, whether they recruit members at lower levels, split or merge, and compete for seats on the Politburo, Central Committee, or State Council. The most salient question—how factional activities affect the making of concrete policies and their implementation—is mentioned in the conceptual chapters but entirely absent from the empirical chapters.

A key insight of *Factionalism* is that "the final outcome of an elite conflict during the Mao period was often determined by which side Mao was on; during the Deng period, however, it depended on who sided with Deng" (p. 422). This captures an important dynamic of Chinese politics and an essential difference between these two leaders. Despite this acknowledgment of fundamental change, Huang argues that the nature of factionalism was a constant during the many twists and turns of the Maoist and post-Mao periods: "All the basic features of the Yan'an Round Table before the [Cultural Revolution] . . . remained unchanged" into the post-Mao period (p. 383). Huang does not incorporate postrevolutionary dynamics or intergenerational transitions. The idea that factional networks were created before 1949 and were policy blind may have been true for the Mao years (which is highly debatable), but that is clearly not the case for the post-Mao years and especially the Jiang Zemin era. This is the shortcoming of the time frame chosen: The central role of policy agendas took on greater significance with the rise of Zhao Ziyang and later Jiang Zemin, but these developments are outside the scope of the book. Much of the conceptual and descriptive argument does not ring true in later periods.

Huang concludes with a brief discussion of how factionalism changed in the 1990s and will change in the future. This is a very interesting section, but it assumes that the reader

brings foreknowledge of issues and events of these years, because no information is presented. The author claims one finding of his study is that factionalism within the party can become the basis for political pluralism, but again there is no evidence to support this provocative claim. In the end, this is a thoughtful contribution to the literature on elite politics in China, but it will not close the debate on the causes and consequences of factionalism.

Xiaobo Lu's study of cadre corruption takes as analytical inspiration Ken Jowitt's concept of the "neotraditionalism" of Leninist parties. Based on Jowitt's discussion of the organizational development of ruling communist parties and the eventual loss of their revolutionary ethos, Lu posits a model of "organizational involution," by which he means that revolutionary parties tend to adopt some rational features (in the Weberian sense) but refuse to embrace or fully adapt to the routinization characteristic of modern bureaucracies. Instead, they replicate traditional practices developed during the revolutionary years, even though these are no longer appropriate to postrevolutionary challenges. He emphasizes how the "deviant" behaviors of communist officials were the direct and logical, if unintended, consequence of official policies. The irony is that steps taken to prevent and punish corruption and other forms of deviant behavior only serve to perpetuate and deepen those behaviors. Beginning with this promising insight, he intends to show that the problem of cadre corruption in China is a generic problem of official deviance common to all communist parties after attaining power.

Lu argues that organizational involution is the most appropriate explanation for cadre corruption, even though the nature of that corruption varies across time. In the Maoist era, when markets were either very limited or entirely absent, cadre corruption was often noneconomic, even though official status was used for private purposes, such as getting children into good schools or assigned to better jobs. With the return of markets in the post-Mao years, and especially beginning in the 1990s, corruption took the form of blatant graft and rent-seeking.

Although the model of organizational involution is not elaborated enough to account for both the changing context and changing nature of corruption, it is useful in explaining why the CCP has not adequately addressed this growing problem. Organizational theory also could help explain why vested interests, dominant ideology, bureaucratic resistance, and other features of complex organizations prevented the CCP from adopting more successful strategies. Much of the empirical material concerns the 1980s and 1990s. Although cadre corruption seems to have grown more severe since then, it seems to follow the same trends described in the book and does not necessarily refute the logic of organizational involution. The chosen time frame is therefore less a problem for Lu than for Huang.

Lu admits it is hard to quantify corruption, although the temptation is irresistible. Examples are sprinkled throughout the text, drawn from local histories and other original sources. He makes good use of these new sources, but the rationale for selecting examples is not spelled out. Whether they are typical or particularly egregious is not clear. It would be useful, for example, to know whether there is regional variation in the past or present prevalence of corruption, and if so, what contextual factors explain that variation.

In addressing existing scholarship on corruption and the problem of monitoring the behavior of local officials, Lu too readily dismisses alternative viewpoints and approaches without definitively showing that his approach is superior. Paradoxically, he also mentions continuities that suggest at least

part of the explanation for cadre corruption is specific to China, not generic to communist systems. For example, he rejects the cultural determinism perspective but also shows how traditional patterns of authority survived after 1949 and how cadres “approximate pre-revolution local officials” (p. 233). This tendency to dismiss rather than build upon previous scholarship mars the otherwise thoughtful analysis.

The comparative examples in Lu’s concluding chapter help put the Chinese case into proper perspective, but they focus more on approaches than on cases. Thus, we read more about how neotraditionalism was a generic feature of communist regimes than about case material to demonstrate it. We read how patrimonialism and prebendalism are common explanations for corruption in developing countries, but only a passing reference to Nigeria is made to support the claim.

Lu ends the book with the statement that “Chinese officialdom will . . . likely remain neotraditional in the near future, nurtured by a ‘booty capitalism’—a fragmented but administratively managed market economy” (p. 257). This is an intriguing claim, but the last sentence of a book is no place to introduce such an important concept. Perhaps his next book will fill in the rest of the picture.

The Politics of Democratization in Korea: The Role of Civil Society. By Sunhyuk Kim. Pittsburgh: University of Pittsburgh Press, 2000. 196p. \$19.95 paper.

Aie-Rie Lee, Texas Tech University

This book is well written and enjoyable to read, in part because of the good use of primary sources and in part because of the deliberate and thoughtful way that Sunhyuk Kim develops the theoretical argument. Certainly, this is a welcome addition to the growing literature on democratization in general and in South Korea in particular.

One of the most compelling streams in this literature is a rediscovery of civil society. Based largely on participant observation in small voluntary groups, scholars are charting the bases for collective political action, particularly how citizens choose to identify themselves and organize for political action in response to state repression. The civil society paradigm used in this book finds that groups organized along lines that expressed new and old horizontal solidarities: grassroots religious organizations, trade unions, business associations, environmental organizations, student groups, women’s groups, neighborhood associations, indigenous groups, and so forth. The theoretical framework looks primarily to the self-organization of political space through associational life as the answer to the puzzle of building and sustaining democracy.

The purpose of Kim’s research is to explain how and why Korean democratization emerged in response to the demands and activities of civil society groups. The book focuses on the role of intermediary social and political organizations as the foundation of democracy. Employing a comparative-historical analysis of three “democratic junctures” in Korea, Kim maintains that Korean democratization has consistently been initiated and promoted by civil society groups. They have significantly precipitated—if not directly caused—authoritarian breakdowns, facilitated democratic transitions, and to a large extent determined the dynamics of politics in democratic consolidation. Crucial to understanding the origins and consequences of Korean democratization for Kim are the scope, power, and concerns of the prodemocracy coalition.

The first chapter establishes the purpose and premises of the book. In chapter 2, the author lays out the theoretical debate and argues for the applicability of the concept of civil society to Korea. This chapter also provides a list of analytical questions that define civil society in terms of actors, interactions, and issues. Chapters 3, 4, and 5 examine in depth the various civil society groups that initiated and directed democratization during each democratic juncture. Throughout, the author exhibits a thorough understanding of both the ideological differences among groups and the socioeconomic and political events that led to their creation. Chapter 6 focuses on democratic consolidation and how civil society groups have affected it. Finally, chapter 7 looks at the future of civil society and democracy in Korea.

Kim analyzes democratization in Korea as a sequence of structural opportunities and constraints. The author is certainly right in suggesting that the process is no longer considered linear or simple. For Kim, there are three stages: authoritarian breakdown, democratic transition, and democratic consolidation (p. 11).

Contrary to conventional wisdom, Kim asserts that it is misleading to argue that the elite contributed positively to democratic transition and consolidation in Korea. Instead, a series of massive, intense, and protracted prodemocracy popular movements directly caused or indirectly facilitated the change. Nevertheless, I believe the role of the elite is still an open question. Also, it is arguable that the visibility of civil society groups does not necessarily mean that they are causal.

The chapter on the role of civil society groups during the third democratic juncture, between 1984 and 1987 (chap. 5), is one of the most interesting, in part because it espouses the view that the Korean state behaved in a more “pluralistic” manner in the late 1980s than in the early 1980s. The most intriguing question is why political democracy was possible in 1987 but not in 1980, immediately after Park’s death. Kim contends that, as compared with the previous two democratic junctures, the scope and power of the prodemocracy coalition had considerably increased. The triple solidarity of student groups, labor unions, and religious organizations—and their prodemocracy coalition with the opposition party—contributed to the authoritarian breakdown between 1984 and 1987. Furthermore, the prodemocracy coalition attracted many middle-class citizens, argues Kim (p. 103).

At times Kim focuses too narrowly on evidence supporting his argument and dismisses alternative scenarios too quickly. For example, he claims that neither the precondition nor contingency paradigm explains democratic transition and consolidation in Korea. It is clear why the author challenges the precondition paradigm, in particular, but it can be argued that sustained economic growth weakened the power base of authoritarian forces because it expanded the density of civil society and, therefore, its capacity to check the monopolistic government. Although it is difficult to determine empirically the relative importance of such variables as economic development, elite strategy, international environment, and civil society, all of these seem to have contributed. The question still remains as to how and why democratization came about. Kim has definite views about why political actors chose certain courses of action, but to me his arguments are more accurately described as working assumptions rather than conclusions drawn from the Korean case.

Kim also shows how civil society became gradually marginalized following the June uprising in 1987 by delegating powers to political society. Kim speculates that if the popular movement had retained hegemony over political society a little longer, the mode and character of the Korean transition

could have been different. Certainly, this is a questionable “if.”

In sum, *The Politics of Democratization in Korea* is an accessible, authoritative, and affordable text. The author successfully tackles his ambitious project and presents a readable and manageable book for an undergraduate or graduate audience. It provides educators with one concise source for examining democratization, civil society, and the state in Korea. It is a valuable contribution to any comparative course on civil society, social movements, society-state relations, and democratization in Korea and/or the Third World.

Legislative Institutions and Ideology in Chile. By John B. Londregan. New York: Cambridge University Press, 2000. 280p. \$59.95.

Peter M. Siavelis, *Wake Forest University*

When the socialist Ricardo Lagos assumed the presidency of Chile in March 2000, it appeared that politics there had come full circle. Despite substantial ideological differences between the Socialist Party of Salvador Allende and that of Lagos, few analysts failed to remark on the irony of a socialist presidency only eleven years after the departure of a military government bent on the brutal eradication of the Left. Despite this apparent sea change, however, the legacy of Chile's military government is alive and well. Postauthoritarian presidents had relative success in the areas of economic management, civil-military relations, and social reform, but they failed to alter substantially the primary political legacy of Chile's rightist government: the 1980 constitution.

That is the point of departure for John Londregan's persuasive and expertly executed analysis of legislative institutions in Chile. Londregan seeks to answer two central questions; one is of concern to institutional analysts more generally, and the other to Chileanist scholars. First, Londregan analyzes a paradox of constitutional design in authoritarian contexts: How can an outgoing authoritarian government guarantee its preferences but at the same time provide subsequent governments the necessary flexibility to govern effectively in the face of change? This fundamental tension dictates that constitution makers strike a balance between delegation and constraint.

The second question emerges from the first. The 1980 constitution has been described variously as authoritarian, as a straight-jacket, and as institutionalizing Pinochetismo without Pinochet. It provides strong checks on civilian authorities, is quite difficult to amend, and in many ways is a recipe for gridlock. It also provides for nine “institutional” senators (the first of whom were appointed by Pinochet and his supporters). They deprived President Patricio Aylwin (1990–94) of a majority in the Senate and can act as veto players in defense of the Pinochet constitution.

Londregan asks whether democratic leaders can effect change in a political system designed so explicitly to defend the status quo. In the process, he teaches us something about constitutional design in authoritarian contexts and about policymaking in situations of divided government more generally. Londregan employs an innovative spatial model that explores the interplay between policy preferences and valence to show how democratically elected presidents can overcome the objections of veto players (in this case the Senate), even in a system in which reform seems unlikely. Drawing on Donald Stokes (“Spatial Models of Party Competition,” *American Political Science Review* 57 [1963]: 368–77), Londregan argues that all policy proposals combine

some element of positional content (a policy preference, often based on ideology) and valence content (widely agreed upon values, such as honesty, “good” education, and safety). He contends that Chile's strong presidential system and the executive's advantages in terms of access to information and control of the bureaucracy transform the president into the most important agenda setter. Legislators are forced to opt between presidential policies or the status quo. Presidents can use their advantage to propose legislation with very high valence content, raising the stakes for legislative noncooperation and swaying veto players' positional preferences closer to those of the executive.

Because data are lacking for floor votes in both legislative houses, and given the difficulty of determining policy differences between parties, Londregan relies on roll-call votes from three of Chile's most important Senate committees (Labor, Education, and Constitution). These three deal with conflicts central to the democratic transition. Senate committees require recorded votes on all amendments offered at second readings of bills, which provides a high number of concrete indicators of policy preferences. Londregan uses these data to estimate the preferred policy outcomes of the president and the most important committee members. He then analyzes to what extent members vote with one another and with their party on a series of issues central to the concerns of each committee.

The results are interesting in a number of ways. Londregan uncovers multidimensionality in political divisions based on particular issue areas, which challenges the notion that the center-leftist governing Concertación coalition and the opposition simply divide along party lines on every issue. The analysis of the Constitution Committee shows that human rights legislation is highly polarizing, and members of the governing coalition divide along the expected lines. Given the depth of division on policy and ideology, the president is unable to make proposals with high enough valence to sway opposition legislators to agree to alter the status quo. In contrast, the opposition is divided on labor issues, and Concertación senators have an ally in an Institutional Party senator, William Thayer. Thayer's position to the Left of most of the opposition gives the president an advantage in labor policy, because high valence policies can attract support from at least some of the opposition. In terms of education policy, the outcome is more complex. Londregan finds a level of division similar to that in the human rights arena, but the high valence of improving education provides the president's proposals an advantage. On social issues, such as divorce, abortion, and drug abuse, Concertación is deeply divided, and Christian Democratic senators often side with the Right.

This work has some real strengths. Londregan successfully introduces methods from the U.S. literature in a way not often seen in non-U.S. institutional work. His analyses of veto players and the interplay of policy position and valence enhance our understanding of executive-legislative relations in Latin America's presidential systems. He also provides a more widely applicable model for statistical analysis of roll-call voting in small legislatures. Sophisticated methods are combined with a very textured analysis, replete with examples and interview data to support the conclusions advanced by his models. Londregan provides a valuable window on the policymaking process in Chile, and he demonstrates the potential for constitutional change, albeit by way of a slow, messy, and piecemeal process.

Londregan could have better connected his argument to some of the literature on Chilean politics, especially that related to continuity and change in the party system. Reference to this work would have strengthened his conclusions by

providing additional supporting evidence regarding significant continuity in cleavage structures. In methodological terms, his choice of Senate committee votes makes sense, but it does not tell us enough about floor behavior in the legislative chambers as a whole, where the balance between policy and valence may be different, or at the very least much more complex. A different balance on the floor would create much tougher terrain for the executive to navigate (and with different results in terms of success).

Despite these considerations, Londregan makes a very useful contribution to our understanding of the dynamics of constitution making, the interplay of valence and policy position, and the potential for presidents to overcome the opposition of veto players in divided government. For analysts of Chilean politics, Londregan provides valuable insights about the country's legislative institutions and convincingly demonstrates the potential for reform of Chile's seemingly inflexible constitutional structure.

Oman: Political Development in a Changing World. By Carol J. Riphenburg. Westport, CT: Praeger, 1998, 248p. \$67.00.

Mary-Jane Deeb, *The Library of Congress*

This is an excellent general study of Oman in the late twentieth century. It gives a broad overview of the political and social structures and institutions of that small country, as well as a fair and balanced assessment of its progress in the last thirty years and of the problems that lie ahead. As Carol Riphenburg describes it, Oman in 1970 "had only the most rudimentary social and economic infrastructure but has since been transformed under Sultan Qaboos into a modern oil-producing state. More recently rich than other states in the area, it serves as sort of a bellwether for the region . . . as it faces the prospect of diminishing oil revenues along with the challenges of an area shaken by the turbulence of the Iran-Iraq War and the invasion of Kuwait" (p. xi).

The first chapter, "Terra Firma and People," is a very informative description of a number of elements that have made Oman the country it is today. Riphenburg argues that geography has kept it historically isolated from the rest of the Arabian peninsula, although its long coastline on the Arabian Sea made it a sea-faring nation. An important section discusses the ethnic groups that comprise this immensely diverse nation of about two million people, one-quarter of whom are expatriates. Although the majority are Muslims, the diverse sects include Sunnis, Ibadhis, Isma'is, and Shi'is. Ethnically, Arabs are in the minority. Other groups are the Baluchis, Zanzibaris, Liwatyah Indians, Hindu Banians, Iranians, and smaller communities of unknown origin, such as the Zatusis and the Shihuh.

The chapter on history provides the framework within which to understand current political and social developments. The survey starts in the eighth century, with the establishment of the Abbasid Caliphate in Baghdad, and Riphenburg notes that in the middle of that century an Omani, Abu 'Ubaydah, made the first recorded trip from the Gulf to Canton in China. She describes Oman's medieval and early modern history as marked by numerous foreign invasions by the Portuguese, Dutch, French, and British, among others. There were also domestic rivalries and wars, and a number of local dynasties ruled the country. The present dynasty of Al Bu Sa'id dates to 1749 and has had its share of problems. The current sultan took power in 1970 after overthrowing his father, a conservative and isolationist ruler, with the tacit endorsement of the British.

The rest of the book focuses on the achievements of Sultan

Qaboos ibn Sa'id (1940–), who attended public school in England and studied at Sandhurst. Upon taking power Qaboos was faced with an uprising (against his father) in the Dhofar region, his birthplace. He dealt with the crisis by issuing a general amnesty to all the enemies of his father in the region and then starting a major program of socioeconomic development and modernization of the country. He built roads, schools, and wells and spent one-quarter of the government's development funds on Dhofar alone to quell the rebellion.

The sultan spent the next few years creating a modern state within the geographical and political framework of the Arabian peninsula and the Arab world. A "benevolent autocrat," in early 1975 he introduced two major laws: the Law for the Organization of State Administration, which for the first time defined the structures and responsibilities of the government of Oman, and the Development Law, which set up mechanisms for an annual budget. In 1981 he created the State Consultative Council, an advisory body whose members he appoints but who represent the seven geographic regions of the country. A new council was created in 1991, named Majlis al-Shura, which is more representative and more powerful. It is the equivalent of a miniparliament, and representatives are now chosen by the people, through a system of election by "nominating colleges" and approval by the sultan. Women can vote and can be elected to some council seats.

Because of its religious and ethnic diversity, Oman is one of the most tolerant states in the Arabian peninsula. There are Christian churches, and Hindus (who, unlike Christians and Jews, are not considered People of the Book in Islam) have been granted religious freedom and have several temples and other religious institutions in Oman. Although respectful of Muslim tradition, the sultan has spoken against extremism and fanaticism. "The sultan's aim is to demonstrate that Islam is consistent with a modern state, interacting independently with the modern world. It represents a willingness to make adjustments to changing conditions in a pragmatic manner" (p. 92).

As for social and economic development since 1970, the author maintains that "the sultanate has moved from a poor, underdeveloped country toward a modern nation state" (p. 147). She refers to a World Bank report that considers Oman no longer among the ranks of underdeveloped nations that need its assistance. Although the commercial production of oil, starting in 1967, facilitated growth and development, the real story is the management of those resources to ensure the education of the population, the distribution of benefits to all segments, including women, and the long-term investment in such nonoil sectors as agriculture and light industry.

Riphenburg is fully cognizant of the problems that lie ahead for Oman. Diversification is limited, as is private investment in economic activities other than trade. Social services are costly and may need to be pared down when oil revenues decline. There are shortages of water, essential for agriculture and the development of a modern infrastructure, and foreign investments other than in the oil sector are also sparse. "Economic development has resulted in social transformations, engendering a sense of entitlement among the public. To fulfill its expectations, the government must ensure sustainable growth" (p. 147). Although significant steps have been made in the emancipation of women (who can vote, work, own property, and so on), many obstacles, mostly traditional, still stand in their way to equality with men.

The author concludes with a look at Oman's foreign policy. She argues that "the unchangeable effects of geography and demography—small populations, vulnerable borders in some

instances, and valuable natural resources—combine with domestic factors . . . to create a Gulf diplomatic style” (p. 184). Foreign policy is highly personal and remains the exclusive domain of a very small ruling elite. Decisions are made to ensure that dynastic interests are not undermined and that borders remain secure. Throughout the Iraq-Iran war, for instance, Oman kept diplomatic ties with both regional powers. Sultan Qaboos has been consistently pro-Western, however, and takes a conciliatory, middle-of-the-road position on regional conflicts.

This book would be very useful in a class on comparative politics as well as international relations of the Middle East or the Gulf region. It is well written but relies almost entirely on secondary sources in English. This is somewhat surprising, since the author mentions in the preface that she studied in France and in Cairo and visited Oman on a number of occasions. There is little theoretical discussion or attempt to put the Omani experiment in a framework other than that of the small states of the Gulf. The lack of a conceptual model limits to some degree the scholarly influence of an otherwise engaging and solid piece of research.

Politics, Parties, and Parliaments: Political Change in Norway. By William R. Shaffer. Columbus: Ohio State University Press, 1998. 290p. \$55.00 cloth, \$21.95 paper.

Christine Ingebritsen, *University of Washington*

William Shaffer has effectively documented the importance of the legislature as an arena for conflict resolution in an era of intense domestic political change. This study is a nice complement to two other prominent analyses published recently (Kaare Strøm and Lars Svåsand, eds., *Challenges to Political Parties: The Case of Norway*, 1997; and Donald R. Matthews and Henry Valen, *Parliamentary Representation: The Case of the Norwegian Storting*, 1999). Shaffer joins an emerging group of Norwegian and American scholars who systematically examine how political parties have shifted to accommodate changes in electoral politics and why Norwegian politics (as in other advanced industrial democracies) has become less consensual. Shaffer’s contribution is to refute the argument made by some scholars regarding the role of the legislature and its declining influence in national politics.

Shaffer develops a theory of democratic political change that explains why legislatures are institutionally well suited to cope with conflictual politics and, specifically, how this plays out in Norway. He analyzes a critical period in Norway’s political development (1985–96), when the traditional divide between socialist and nonsocialist bloc politics unraveled, but he situates his study as part of a general history of the legislature’s role in Norway’s political development.

Once a prominent institution in Norwegian political history, the parliament (Storting) lost influence to the executive and the bureaucracy during the postwar development of the welfare state. Because corporatist institutions could not manage the extent of changes in society and politics, there was a “reparliamentarization” of Norwegian politics. Here, it should be noted, Shaffer diverges from the classic analysis by Peter J. Katzenstein (*Small States in World Markets*, 1985), who maintains that the very institutions of democratic corporatism permit flexible adaptation to political (and economic) change. Whereas Katzenstein gives priority to the role of a tripartite relationship among government, business, and labor, Shaffer emphasizes the legislature as the most important arena for mediating change. As his four-part theory of democratic change (conflict management, consensus building, representation, and lawmaking) seeks to ex-

plain, the legislature is the only institution capable of managing the breakdown in consensus evident in Norwegian politics from the 1970s to the 1990s.

The strengths of the book lie in its detailed explanation of how the Norwegian electoral system functions; the specific issues that have become salient in Norwegian politics; and the different phases in the shift from a two-bloc party system to a realigned parliamentary coalition system. The coverage of how votes translate into mandates, the continuation of a national tradition (also outlined by Matthews and Valen) of weighted voting for rural areas, and changes in the percentages of votes for the major political parties are central contributions of this volume. Both domestic and international changes have led Norwegians to disagree over their relationship to the European Community, appropriate environmental policies, levels of taxation, abortion rights, and immigration policies.

Shaffer’s empirical data are novel, and the presentation is rigorous. The multidimensional diagrams of roll call votes in the Storting model how Norwegian political parties evolved from a clearly defined division between socialist and nonsocialist blocs to a minority party system that transcends the traditional division.

The focus on the legislature necessarily omits the importance of other sources of political change in Norway. Although Shaffer is not a political economist, he gives too little attention to the discovery of oil and gas on the Norwegian continental shelf and its effects on society and politics. Also, charismatic individuals (such as Carl I. Hagen, leader of the Progress Party, and Kjell Magne Bondevik, former prime minister and member of the Christian People’s Party) are not viewed as important as larger institutions in managing political change and working toward a new policy consensus. The media (as Shaffer acknowledges on page 219) has grown in importance as an independent actor, but this dimension does not figure prominently in the book. Furthermore, the effects of Europeanization and globalization seem to “stop at the border” for this domestic study of Norwegian institutional change. Nevertheless, the book exceeds expectations and is required reading for those interested in comparative democratic institutions, Scandinavian politics, and changes in electoral preferences in advanced European welfare states.

Courts and Transition in Russia: The Challenge of Judicial Reform. By Peter H. Solomon, Jr., and Todd S. Foglesong. Boulder, CO: Westview, 2000. 222p. \$25.00 paper.

Kathryn Hendley, *University of Wisconsin-Madison*

Courts have been mostly neglected in analysis by political scientists of the transition from state socialism. This book begins to fill this gap by focusing on the current state of Russian courts. The primary focus is on the courts of general jurisdiction, in which the vast majority of cases are heard, including all those in which physical persons (as opposed to legal entities) are involved. Although Russia also has a constitutional court and economic courts, these institutions are less thoroughly analyzed.

Solomon and Foglesong are among the most knowledgeable Western observers of the Russian criminal justice system, and the book benefits from their years of experience. Their discussion of the pretrial phase of Russian criminal procedure is superb. It combines a concise description of the Soviet legacy with a detailed analysis of the recent reforms. They uncover some of the reasons these reforms have not had the intended results, such as the shortage of competent lawyers, which has undermined the expanded right to counsel

for those accused, and the lack of legal training, which has thwarted the hiring of additional investigators (in an effort to curtail delays). The authors provide empirical documentation of the misery and human rights abuses associated with pretrial detention, and they argue that the solution is not to build more jails but to rethink the policy that allows the accused to be routinely detained during lengthy investigations. They acknowledge the shortcomings of the existing system but reject any wholesale abandonment of it as politically and culturally impractical. They prefer an approach that explores how to “make the existing Continental system of criminal justice in Russia as fair and workable as possible” (p. 145).

The book brings together a great deal of previously unpublished information about the operation of the courts of general jurisdiction. The system of selection and retention of judges has undergone dramatic changes since the late 1980s. Solomon and Foglesong describe the new institutions, such as the Judicial Department and the Judicial Qualification Commissions, aimed at allowing the Russian judiciary to police itself. Yet, they do not merely describe how these institutions are supposed to work; they delve into an analysis of how they actually operate. The tabular information is consistently complemented by detailed explanations of what it means in the Russian context.

Much has been made of the adoption of lifetime tenure for the Russian judiciary, but the authors clarify that because these new rules came into effect only in 1993, most judges (approximately 76% as of January 1998) are still serving out fixed terms (p. 32). Solomon and Foglesong are appropriately skeptical about the long-term effect of this reform to the judicial tenure policy, recognizing that life tenure does not automatically translate into increased autonomy. Along similar lines, their analysis of the data on delays experienced by litigants provides a useful antidote to the impression created by the popular media (both Russian and Western) of a Dickensian legal system in which cases drag on for years and in which justice is an impossible dream. The authors point out that the unreasonably short statutory deadline for handling cases makes a high level of delays in court statistics inevitable.

The use of information collected through a survey of Russian judges by the authors is somewhat less successful. Select questions (in both Russian and English) from the survey are replicated in an appendix, along with the number of respondents and the breakdown in responses (pp. 207–10). The difficulties of conducting a survey among the far-flung judiciary of Russia should not be minimized, but the distribution method—through the chairmen of the council of judges, whom the authors elsewhere report as having extraordinary influence over ordinary judges—seems fraught with problems that undermine the reliability and validity of the results, which the authors fail to acknowledge. Of the 2,000 forms sent out, 321 were returned. The authors’ claim of “confidence in the representativeness of our findings” (p. 206) is not convincing. At best, the survey results can be treated as suggestive and can serve as the basis for hypotheses to be tested more rigorously.

In parts of the book, the authors carefully limit their claims. In reporting on the attitudes of judges toward the qualifications commissions, they make it clear that their data are limited to the judges who participated in the survey (p. 36). Elsewhere the lines are more blurred. For example, when discussing the extent of financial contribution from local governments, the authors push their data too far, given that they do not have a representative regional cross-section among their respondents. “According to our survey of more

than three hundred district court judges, 58 percent of courts received some help from local governments in 1996 and 1997” (p. 39).

Solomon and Foglesong intend the book for both scholars and policymakers, although they are most interested in having an influence on the latter. Interspersed throughout are recommendations to Russian policymakers and Western donors engaged in legal reform. Their goal is to outline “what can and should be done to make courts in Russia autonomous, powerful, reliable, efficient, accessible, and fair” (p. ix). This effort to serve a dual audience of scholars and reformers yields mixed results. The recommendations are framed in terms likely to appeal to policymakers but without the theoretical resonance that would interest scholars. In some instances, they reflect a lack of awareness of the political constraints within which foreign donors operate (e.g., suggesting that foreign donors contribute equipment, when many foreign governments have a strong preference for hands-on assistance and a resistance to equipment dumps, p. 42) and an unwarranted optimism about what can be accomplished with donor assistance (e.g., intimating that a conference sponsored by the U.S.-based National Center for State Courts could teach Russian judges to become more independent of political and administrative pressure, p. 52).

The book is unquestionably an important contribution to the burgeoning literature on the role of political institutions in the transition from state socialism and deserves to be read carefully by those interested in this ongoing process in Russia and elsewhere.

Japan’s Budget Politics: Balancing Domestic and International Interests. By Takaaki Suzuki. Boulder, CO: Lynne Rienner, 2000. 284p. \$59.95.

Ronald J. Hrebendar, *University of Utah*

The key to the real nature of this book lies not in the main title but in the subtitle. Takaaki Suzuki is not simply updating John C. Campbell’s classic *Contemporary Japanese Budget Politics* (1977). Campbell analyzed the Japanese budgetary process from a variety of different perspectives, but his focus was completely on the domestic political actors. Suzuki gives relatively little attention to the domestic actors and is largely concerned with how foreign political and economic pressures affect Japanese fiscal policies and related budget policies.

Suzuki breaks the past 40 years of Japanese political history into four different patterns of budgetary behavior, each roughly associated with a decade. The 1960s were characterized by balanced and relatively small budgets (less than 20% of annual national income). In the 1970s there were greater demands for governmental spending combined with growing public and corporate resistance to increased taxes, which resulted in greater deficits. By the end of the 1980s, the government had eliminated the huge budgetary deficits. In the 1990s the pattern reversed, and there was large-scale use of deficit financing.

Suzuki attempts to explain these policy reversals over the past 40 years. He uses a two-level approach patterned after Robert Putnam to examine why Japan has experienced repeated international pressures to adopt budgetary policies to promote fiscal expansion. In addition, Suzuki wants to identify the interests and objectives of all the relevant Japanese domestic actors, and he explores the relationship between international and domestic political forces over time as they influence budgetary decision making.

In general, Suzuki assigns more weight to the domestic side of the process than to the international. He argues that the

twists and turns of Japanese budgetary policy over the past four decades were largely driven by domestic political forces, reinforced occasionally by international pressures that accelerated processes already in motion. Yet, he gives some quite plausible reasons for what he calls the international politicization of Japanese budgetary policy. It stems from the awareness that fiscal expansion can result from budgetary changes, and these can affect balance-of-payment adjustments. A major contribution made by Suzuki is to detail the circumstances in which Japan can say “no” to foreign demands.

The book is weakest in trying to explain domestic budgetary decision making. The process is never fully detailed, as the analysis is largely sited at the systemic level. In the 25 years since Campbell’s book, many changes have occurred in Japanese politics, government, economy, and society. The country is well into its second decade of economic recession and apparently has not found the secret of how to break this pattern of stagnation. The political system has been significantly redesigned, not only in terms of the party and electoral system but also in terms of the central government bureaucracy. The role of the Ministry of Finance (MOF) has changed in recent years, as politicians and other ministries have chipped away at its power. One can only speculate that all these changes have affected the budgetary process in important ways. Unfortunately, Suzuki does not fully address these.

Suzuki challenges the conventional models regarding Japanese budgetary politics. He (correctly) identifies the MOF as an independent force and not merely a ratifier in the decision-making process. The MOF, Suzuki argues, is a governmental actor that seeks to fulfill its own interests. In his larger analysis, the author rejects rational choice theory as well as neorealist and neoliberal perspectives, relying more on historical methods. He tries to identify the ruling coalition in the various eras and tries to link them to expected outcomes. This produces useful case studies of the four eras.

Japan’s Budget Politics is a significant contribution to understanding the economic relationship between Japan and the United States over the past three decades. The conclusion that Japan did not change its policies because of foreign pressures but that these largely reinforced policy patterns already in place in Japanese politics is an interesting reinterpretation of the way Japan deals with other nations. The book is also useful to students of international political economy who seek a better understanding of how domestic politics and foreign policies are intertwined. The relative lack of attention to domestic budgetary decision making is a disappointment, but although the book is not an update of Campbell’s work, it is a quite useful analysis of how Japan’s domestic budgetary politics are related to other nations’ foreign policies.

A couple of stylistic weaknesses should be mentioned. The book reads too much like a dissertation, and there is considerable repetition of the central points. These criticisms are relatively minor, however, compared to the real significance of the analysis. The author is quite persuasive regarding the flexibility of the Japanese government in adopting different fiscal policies to meet both domestic and international pressures. One of Suzuki’s major objectives is to build bridges between comparative and international politics, and he goes far in accomplishing it. *Japan’s Budgetary Politics* would be appropriate for upper division and graduate courses in both comparative politics and international political economy.

Libya since Independence: Oil and State-Building. By Dirk Vandewalle. Ithaca, NY: Cornell University Press, 1998. 226p. \$49.95 cloth, \$17.95 paper.

Ali Abdullatif Ahmida, *University of New England*

In 1951, Libya became an independent state, a federal monarchy. In 1969, a military coup overthrew the Sanusi monarchy and brought Mu’ammar al-Qadhafi to power. Since that time Libya has become invisible, the least known of the North African states in comparison to Egypt, Tunisia, Algeria, and Morocco. Continuing conflict with the United States over terrorism and the question of Palestine has made Libya inaccessible to foreign scholars. In the absence of serious scholarship, competence in Arabic, or reliable journalistic knowledge of Libyan society and politics, Western mass media and many academics have reduced Libyan politics to the persona of Qadhafi, who frequently is characterized as a mad dog who rules a rogue regime. This new book by Dirk Vandewalle is therefore an especially welcome contribution.

Vandewalle is among the few scholars—such as John Davis, François Burgat, Taoufik Monastiri, Moncef Djaziri, and Lisa Anderson—who have actually visited Libya and done research there. His earlier in-depth analysis of the Libyan experimentation in state-building is marked by openness and avoids the usual oversimplification of focusing on the Libyan leader, although it presents a critical examination of the Qadhafi government. This book is an elaborate synthesis and assessment of Vandewalle’s major arguments regarding state-building in Libya from 1951 to 1996, with special attention to the period following discovery of oil in 1959 and its marketing in 1961. Oil has become Libya’s major source of revenue.

Libya since Independence addresses three central questions. (1) What happens when rulers of a “late developer” are freed from the burden of taxation? (2) How does this seeming luxury affect the process of building the local state? (3) How do local rulers create the economic and political processes that under more conventional historical circumstances emerge from compromises? The book contains a theoretical review of the literature on state formation and the “rentier/distributive state,” an examination of Libya since independence under two regimes, and reflections on the Libyan case and beyond. Vandewalle’s eclectic approach to the rentier state, state autonomy, and late developers is derived from the literature on new political economy, institutional economics, comparative politics, and rational choice history of taxation and revenues.

Specifically, Vandewalle focuses on four boom and bust periods in the world oil market: the boom of 1963–69 during the Sanusi monarchy, the boom under the military regime between 1969 and 1973, the boom of 1973–79, and the bust of the early 1980s. He also evaluates the failed attempt at political and economic reform in 1987. The central thesis is that Libya is not an aberration but a distinct case of a rentier/distributive state. Like other rentier states, such as Saudi Arabia, Qatar, and Kuwait, Libya relies mostly on oil revenues and less on taxation of its citizenry. Hence, the state has more autonomy than in conventional cases of state formation.

Two factors make Libya a distinct rentier state. First, the government relies exclusively on oil export revenues. Second, and more important, the historical timing of rentier status occurred when state institutions and national integration were in their infancy. The author faults both the Sanusi monarchy and the revolutionary regime of Qadhafi for suspending the process of building strong economic and

political institutions fundamental for successful state formation. These institutions would have enabled the state to adjust to shrinking oil revenues and diversify the economy.

Vandewalle's analysis demystifies the Libyan case and significantly contributes to a fresh theoretical comparative analysis of the rentier state in general. It challenges both the popular view of Libya as a rogue nation and such ahistorical cultural notions as Islamic ideology and tribalism, which dominate current Middle Eastern studies. Vandewalle provides insight into the underlying processes and political economy of state-building. His belief about the central importance of strong state institutions allows him to explain why the 1987 reforms failed and why in the post-Qadhafi period Libya will face a tough future. Oil-dependent rentier states are inevitably weak because of weak institutions, argues Vandewalle, and are more vulnerable to economic crises.

The main methodological problem in this work, which offers fresh and welcome insights, is the one-sided focus on the state at the expense of society. Absent are the social forces, voices, and agencies of the Libyan people, not as clients and tribesmen but as real participants in history. State and class formation under colonialism are overlooked. Some institutions of today's Jamahiriyya "state of the masses" experiment, for example, are not just reactions to monarchy but rooted in the Tripolitanian republic and the Sanusiyya movement. The study provides no serious analysis of the opposition under the monarchy and current regime, such as student movements, the role of intellectuals, the army, underground political parties, exiled political activists, and especially today, Islamic opposition movements.

These problems aside, Vandewalle has written a major scholarly work. His research provides a major corrective critique to cultural and idiosyncratic accounts of the Libyan state. The book treats Libya not as an anomaly but as a historical case of a distributive state. This alone is an important contribution to our knowledge of Libyan politics in particular and our understanding of distributive states in general.

Parliamentary Democracy: Democratization, Destabilization, Reconsolidation, 1789–1999. By Klaus von Beyme. New York: St. Martin's, 2000. 248p. \$59.95.

Kerstin Hamann, *University of Central Florida*

Klaus von Beyme tackles an important issue, the development of parliamentary democracies in both historical and comparative perspective. He compares the stages of democratization with the separately occurring sequence of parliamentarization and shows how the introduction and consolidation of parliamentary systems is tied to the issue of regime changes more generally. He also discusses changes in the role of parliament over time. Von Beyme provides a historical analysis of parliamentary democracies rich in descriptive detail. Yet, he goes beyond a narrative account of the development of parliaments: "Classification of the causes of government termination and patterns of coalition building nevertheless need quantification in order to escape mere description" (p. 3). True to this introductory statement, the author provides throughout the book a wealth of useful and detailed tables that present comparative data on parliaments.

The book is clearly structured, beginning with an overview of different regime types and the role of parliament in these different systems. Von Beyme analyzes the position of parliaments in monarchies, presidential systems, and semipresidential systems. This conceptual clarification is followed by a

discussion of waves of parliamentarization and democratization, in which it is argued that these were distinct occurrences. The author shows, for example, how "revolutions rarely led to representative government because parliamentary government was not an idea that could mobilize the dissatisfied masses" (p. 17).

In addition, von Beyme discusses different aspects of parliamentary sovereignty, one of the prerequisites of parliamentary democracy that relates to the internal organization of parliaments. He illustrates considerable variation over time and across countries with respect to the internal workings of parliaments.

Another area of comparative inquiry relates to the functions of parliament, which, von Beyme argues, in modern democracies basically lie in the relationship between the deputies and the citizens (representation and articulation of interests), the relationship between parliament and government (controlling function), the relationship to the needs of society (legislative function), and the relationship of parliament and government (recruiting function) (p. 72). The relative importance of these functions has changed over time.

Parliaments also have a relationship with the head of state. This is particularly complex in presidential and semipresidential systems, although the head of state has a very reduced function in parliamentary systems. Von Beyme identifies the party system as the "most important variable in relations between the president and the parliamentary majority" (p. 115).

Finally, the author examines the relationship between the government and the parliamentary majority. He further provides a comparative overview of coalition formation and the role of the prime minister. Von Beyme posits that parliaments have lost much of their significance in bringing down governments. Instead, elections, cabinet collapses, or coalition collapses have become a more frequent reason for government turnover (p. 207).

Von Beyme argues that the decline of parliamentary power is a logical extension of the democratization of parliamentarism. Parliamentary power is cut by the judicialization of politics, especially when parliamentarians anticipate the intervention of constitutional courts during the legislative process (p. 212). Other factors weakening the role of parliament include the nature of foreign policy and the supremacy of the European Union for European democracies (p. 213). Processes of regionalization and simultaneous globalization further erode the autonomy of national parliaments (p. 215). Despite these developments, von Beyme contends, national parliaments are likely to retain an important regulatory function, especially with respect to the welfare state.

An outstanding feature of the book is the wealth of comparative data in tabular form in conjunction with the narrative account of the historical development of parliaments. Von Beyme highlights the variation in parliamentary systems and parliaments themselves but also points to the common features of parliamentary systems. This alone makes the work a valuable asset for comparative research. The book not only speaks to scholars interested in comparative legislatures but also ties in with the literature on regime change and political institutions. It also adds to the literatures on electoral laws, government formation, and the distinction between presidential and parliamentary systems. It is not an accident that von Beyme links these bodies of work, which often remain distinct from one another. Parliament, despite the ongoing erosion of its role, is still at the heart of the policymaking process in many democracies and, as such, ties together a large number of different political actors and institutions.

The book combines various areas of interest in comparative politics in a meaningful way. It also provides a unique overview of different variants of parliamentary systems. Von Beyme argues cogently that, despite the weakening of parliament, an immediate crisis of democracy does not loom on the horizon, and parliaments still retain important functions, although these are different—or at least have different emphases—from in the past. The major shortcoming of this otherwise outstanding book is the lack of an overarching argument or theoretical framework. The wealth of information it provides could have benefited from an analysis that is more explicitly guided by a theoretical argument.

Also, the book does an excellent job in presenting comparative data about parliaments, but the explanation of these data remains sketchy. Although this could easily be interpreted as a weakness, it is also a major strength, as it raises a host of unanswered or only partially answered questions. Anyone interested in the comparative study of parliaments will find a host of new research questions, both comparative and historical. The stimulation of new areas of inquiry may well have a significant effect on the discipline. This is a highly useful addition to the literature on parliaments and should be read by anyone interested in any aspects of comparative parliaments or comparative democracies. It is also a welcome addition to the literature on democratization.

The Mysteries of Development: Studies Using Political Elasticity Theory. By Herbert H. Werlin. Lanham, MD: University Press of America, 1998. 409p. \$68.00 cloth, \$25.00 paper.

Paul Clements, *Western Michigan University*

This book is the product of a lifetime. Inspired by Dwight Waldo and Sheldon Wolin, who were Werlin's professors at Berkeley, and informed by decades as a consultant to the World Bank, Werlin seeks to unify the literatures of public administration and comparative politics, to explain the requirements for economic growth under any kind of government, and to provide an agenda for World Bank policy in low-income countries. Although I believe he is motivated by great confidence in good will, his simplistic approach to theory would wash out the analytic power that comparative politics achieves through institutional analysis. The book at points becomes a commentary on debates within the World Bank but also reflects many of the bank's biases. The argument is constructed around a set of dualistic definitions that constitute a frame of reference in which the virtue, knowledge, and practice needed by less developed countries lie in the more developed countries. Werlin's policy agenda reflects both blindness and bias, with frightening potential consequences.

For Werlin, the key distinctions are between political hardware and software and between political elasticity and inelasticity. Hardware refers to objective rules, procedures, technologies, organizational arrangements, and methods. Software refers to the quality of relationships between leaders and followers as manifest in attitudes, teamwork, and morale (p. 8). Good political software leads to (or constitutes) political elasticity, and for Werlin this is everything—the universal criterion for good government and cause of development. Moreover, good software can be achieved through specific steps: “establishing acceptable goals, hiring qualified personnel, encouraging training, delegating responsibility, stimulating motivation and competition, paying attention to morale, expanding two-way flows of communication, promoting legitimacy, maintaining supervision, cultivating

contractors, protecting independent spheres of authority, and developing conflict-resolution procedures” (p. 64). This list is repeated several times.

Political elasticity refers to the extensive quality that can exist when power is exercised in an accommodating and consensual manner within a framework bounded by credible threats, as compared to power that is merely directive and coercive. “Under political elasticity, forms of decentralization can be expanded without reducing centralization; under political inelasticity, leaders fear decentralization, thereby weakening their implementation capacity” (p. 117).

Werlin's method provides no protection against bias. The primary form of argument is to assert a relationship and to substantiate it with a series of examples drawn largely from World Bank studies. When Werlin argues against Weber, behavioralists, and economists, he summarizes their positions in one or a few points, without engaging the fabric of their argument or showing how they arrive at conclusions different from his own. He does not explore alternative explanations, for example, from comparative politics, for the problems he addresses. His unification of public administration and comparative politics is achieved by placing software at the heart of his analysis, thereby reducing political and administrative problems to the quality of leadership (pp. 271, 293–4). The particular form of his dualism, however, has the effect of placing the source of leadership in the World Bank.

Werlin does summarize critiques of the bank's project implementation. Since his analytic approach has no place for political forces acting through the bank, however, he can take no account of the bank's systematic contributions to development failures. Also, he perhaps inadvertently reproduces the bank's arguments on development approaches, which are contested in much of the comparative politics literature.

Werlin explains political elasticity by comparing examples of well and poorly managed programs and showing that the latter lack the good political software exhibited by the former. Elasticity explains why there is better garbage collection in Japan than in Nigeria, better rural development in Korea than in Ghana. All poorly managed programs have characteristics that violate Werlin's list. His argument that politics is “more powerful than economics” (p. 16) will be familiar to economists as a discussion of public goods. Governments fail to provide law and order, basic infrastructure, and a conducive institutional environment, so there is little investment (p. 153). A politically elastic government surely would provide these goods.

Werlin's two other main dualisms are primary and secondary corruption and primary and secondary democracy. Primary corruption refers to rule violations in a polity in which rules are generally upheld, and secondary corruption is rule violations for private gain without fear or shame. Primary corruption is widespread in developed countries, but Werlin believes secondary corruption, which he sees as a “political illness” analogous to drug addiction or alcoholism, is characteristic of less developed countries (p. 199). Primary democracy, in line with political hardware, refers to elections and majority rule, whereas secondary democracy involves discussion, consensus-building, reconciliation, and leaders who reach out for popular approval (p. 9). Because primary democracy is so often not conducive to economic growth in poor countries, Werlin argues that donors “should not push them into primary democracy before they are ready for it” (p. 234).

From Werlin's seat in an elastic primary democracy, things look pretty bleak for the desperate masses. “There is so little evidence of real professionalization in [less developed countries] that advocacy of ‘deprofessionalization’ in these coun-

tries might be greeted with amusement, to say the least" (p. 162). Based on nothing but anecdotes, the author concludes that, "as a generalization, we can say that the more impoverished a country is, the more likely the press is to be suppressed, intimidated, or undermined in various ways" (p. 190). We should think of helping countries with per-capita income of about \$400 (as in India), Werlin suggests, as analogous to helping vagrants who may "suffer from various forms of mental illness or social pathology" (p. 305). In this light one can understand his outrageous argument that the World Bank should include language in project agreements that allows bank staff to require the removal of uncooperative officials (p. 330).

To see the significance of a comparative institutional approach, contrast "political elasticity" with the comparative politics standard of Peter Evans (*Embedded Autonomy*, 1995). When Werlin presents a politically elastic financial system, what he describes approximates the neoclassical ideal of a liberal financial market (pp. 26–7). In describing South Korea under Park as a model of political elasticity, however, he inconsistently acknowledges the extreme bank-based and inflation-financed interventionism of Park's government. He uses the World Bank's oxymoron for such cases of interventionism: "market sustaining incentives and disincentives" (p. 281). Evans agrees that Park provided important leadership but argues that a history of strong bureaucracy, dominant classes weakened by colonialism, and Korea's position in Cold War geopolitics were no less important for the country's rapid growth. Korea's primitive capital accumulation problem was solved by an exclusive alliance between the state and business leaders that was much more coercive, particularly in its early stages, than Werlin can acknowledge. Werlin takes no account of different structural conditions; political elasticity is the ideal for all governments at all times.

The small government, free-market regime that the World Bank enforces on less developed countries conflicts with the technologically focused investment strategies Evans recommends. He would see the bank's free-market ideology as working hand in hand with the interests in commodity imports and manufactured exports and in political influence over low-income countries of the governments that control the bank.

In the final chapter, Werlin acknowledges that the bank has been driven by its shareholder governments and its internal organizational incentives to emphasize quick project preparation and rapid disbursement (pp. 321–2). He notes that because bank staff take the initiative in project preparation there is often little local "ownership" of the projects. Under his dualisms, however, the idea cannot arise that this could undermine the corporate coherence a government needs to retain access to bank funds while dismissing much of its advice. He does not see that debt burdens following poor project implementation can serve the interests of developed countries. In this context, his recommendation that the World Bank should establish an Economic Reform Olympics, to intensify pressure and promote greater competition, should be viewed with alarm.

The Soul of Latin America. The Cultural and Political Tradition. By Howard Wiarda. New Haven, CT: Yale University Press, 2001. 417p. \$35.00.

Brian Loveman, *San Diego State University*

This book is vintage Wiarda, a synthesis of more than thirty years of research and writing on the Iberian tradition and the Catholic, absolutist, hierarchical, politicosocial system in-

stalled in Latin America by Spanish and Portuguese conquest. Stressing these continuities in Latin American societies ("the Latin American soul," p. 281), the author traces the origins and evolution of Latin American political culture from Greek and Roman times until the end of the twentieth century and compares it with the political traditions of Anglo-America.

Like much of Wiarda's work, this book is dedicated to describing the differences between the Iberian politicocultural tradition—from Roman and Moorish influences, medieval Catholicism, the *reconquista* (711 A.D.–1492), the counterreformation, authoritarianism, corporatism, and the mediated influences of positivism in Latin America in the nineteenth century—and the Anglo-American political tradition. Emblematic of this difference is the juxtaposition of political philosophers John Locke, the supposed inspiration of U.S. democratic institutions, and Francisco Suárez, a Spanish political philosopher "almost unknown in the United States" but the foremost writer among sixteenth-century Spanish neoscholastics. Chapters on liberalism, positivism, nationalism, Marxism, corporatism, the "conflict society" from the 1930s to the 1980s, and the transitions to democracy (1978–90) are framed as challenges to, and constrained by, the Ibero-Catholic tradition. In each case Wiarda considers how Latin American societies changed or, more commonly, maintained cultural continuities over some five hundred years.

Wiarda also seeks to document the consequences of the survival of the Latin America soul in the region and its implications for U.S.–Latin American relations. Evaluation of U.S. policy is usually framed by a "realist" approach: U.S. policy was, and should be, driven by U.S. interests, as defined by policymakers like Richard Nixon, Henry Kissinger, and George H.W. Bush. Whatever U.S. interests are pursued or whatever policies adopted, Wiarda emphasizes that there is no reason to expect Latin America to conform to "democratic" norms, that is, liberal pluralism, as understood in the United States. He properly warns students and policymakers that Latin America, with all its cultural variation (which sometimes gets lost in this book), must be understood on its own terms. U.S. legislatures, courts, municipal councils, the presidency, the federal system, church-state relations, and racial policies and practices are not apt benchmarks for equivalent or comparable institutions in Latin America.

Wiarda concludes the book with the following advice to U.S. policymakers, inevitably to be ignored: "The United States needs to be very careful before heavy-handedly mucking around in other people's countries. Not all democracies have to look exactly like the United States. For American policy to get too far ahead or fall too far behind the country being assisted is a formula for disaster for both parties." What this means in practice is that the United States should not "push for too much democracy before the country can handle it" (p. 358).

Wiarda's interpretation of a unique Iberian politicocultural tradition is an important foundation for students of Latin American politics. His interpretation of Latin American politics and U.S. regional policies in the post-1950s period is another matter.

Regarding Latin American politics in the 1960–80 period, Wiarda claims, for example, that the bureaucratic authoritarian regimes of the 1960s and 1970s represented an effort to restore the status quo ante (p. 305). Yet, all the military regimes and their civilian counterparts rejected the past, whether the populist Peruvian and El Salvadoran military of the 1970s or the "liberal" Chilean, Argentine, and Brazilian military. The coups and repression came in the name of

anticommunism but also of economic development and technological innovation, that is, “the future,” not the past. The post-1964 military regimes, from Brazil, Uruguay, and Chile northward to Guatemala, reified “modernization”; none proposed going back to a society dominated by large semifeudal latifundios, a society of castes and privilege based on color, ethnicity, guild-like corporations, family ties, or even legislatures controlled by landed oligarchs.

Military juntas with civilian allies sought to demobilize and repress populist and revolutionary movements in Latin America, in part as a reaction to the Cuban revolution, but never, as Wiarda suggests, to “turn the clock back” (p. 308). The military regimes were antipolitical, antiliberal, and antidemocratic, but none of them alluded to Francisco Suárez or Saint Thomas (except perhaps in the latter case, indirectly, with allusions to the notion of natural rights, “just war,” and the “right to rebellion,” that is, for military coups to overturn elected governments). In any case, Saint Thomas would hardly have applauded market determination of “just price” or “just wage,” or the privatizations and financial liberalization promoted in Chile, Mexico, and elsewhere in the 1980s.

Even more important, Wiarda underplays or ignores the crucial role of the United States in sustaining, when not promoting, the military dictatorships of the 1960s and 1970s. The civil-military regimes after 1964 relied significantly on U.S. support for counterinsurgency, civic action, and state

terrorism on an international level. Not even in the index does Wiarda refer to “Pentagon,” “counterinsurgency,” “covert operations,” or “Operation Condor.” Acknowledging what he calls the often sorry history of U.S. interventionism in the Caribbean and Central America in the early twentieth century, he comments: “Such interventions are simply what a great power, a paramount, a hegemon does to protect and advance its interests” (pp. 180–1). Maybe so, but the question Wiarda does not pose or seek to answer is to what extent U.S. meddling, coercion, exported racism, and military occupation from the 1890s until the end of the twentieth century warped, if not transformed, the Latin American soul.

Like the U.S. policymakers whom he seeks on occasion to advise, Wiarda generally overlooks the effects of pernicious and hypocritical U.S. regional security and economic policies from 1945 to the present. After accepting Wiarda’s claim that the soul of Latin America is not a natural fount of liberal pluralism, the next step is to explore the sequelae of its violation over and over again by policymakers in Washington, DC, in ways that have determined, even overdetermined, political outcomes in the region for the last century. In the end, Wiarda’s insight on the historical origins of Latin American political culture is blurred by his interpretation of post-World War II politics and the effect of U.S. foreign policy in the region. He fails to reveal to his readers the affect of the Devil’s work on Latin America’s soul.

International Relations

Legal Rules and International Society. By Anthony Clark Arend. Oxford: Oxford University Press, 1999. 208p. \$35.00 cloth, \$17.95 paper.

Brad R. Roth, *Wayne State University*

The past decade has been marked by an increase in scholarly efforts to bridge the gap between the study of international relations as undertaken by political scientists and the study of international law as undertaken by legal scholars. During the Cold War, when international norms seemed distinctly secondary to ad hoc cost-benefit calculations as influences on state behavior, political scientists largely dismissed international law as rank idealism. International cooperation has palpably broadened and deepened in the current era, and political scientists have started to take a second look. Interdisciplinary dialogue is fraught with difficulties, however, since political scientists and legal scholars approach the subject with different questions and different methodological frameworks in mind, even assigning wholly different meanings to many of the same terms.

Legal Rules and International Society should be seen as a significant contribution to the literature of both disciplines. The book is a sweeping effort both to explain the competing tendencies in each discipline to the practitioners of the other and to demonstrate how a workable interdisciplinary enterprise emerges from the interaction.

Arend argues that legal rules play a distinctive role in international relations. That role is best appreciated by adopting his own nuanced form of legal positivism as an approach to apprehending those rules (pp. 86–104) and by using constructivism as an approach to understanding their effect on the behavior of international actors (pp. 124–48). Arend takes as examples the UN Convention on the Law of the Sea (UNCLOS) and the European Union treaties

(pp. 142–7), and he contends that international legal rules can affect not only states’ treatment of nonessential matters, as rationalist institutionalists and even structural realists can concede, but also states’ interests and even their very identity. Invoking Friedrich Kratochwil’s observation that the rules of chess define the game itself (p. 130), Arend makes the case that international law can, over time, verifiably influence the ways in which state actors conceptualize their aims and the options available to them in pursuit of those aims (pp. 129–32, 137–48). (That statehood itself is a juridical construct, with international mechanisms according or withholding recognition of that status, goes some distance in substantiating this point.) Arend’s purpose is largely to invite, and to orient, empirical study of these constitutive effects.

The first half of the book examines the nature of international law or, more specifically, international legal rules; chapters explore their character, their sources, and competing methods of ascertaining their existence. Arend demonstrates some of the ways in which legal rules differ from other rules operating in the international arena, and he offers a standard examination of what jurists traditionally cite as the sources of such rules. He then presents and contrasts the law-finding methods offered by legal positivism and its leading competitor, the “New Haven” approach of Myres S. McDougal, Harold D. Lasswell, and W. Michael Reisman. Arend does an especially effective job of summarizing the complex New Haven approach, which combines an empirical inquiry into whether ordering processes manifest requisite levels of “authority” and “control” with a normative inquiry into whether those processes are adequately oriented toward the furtherance of “human dignity” (pp. 76–86). Arend dismisses this last, teleological aspect as unhelpfully subjective, observing that “there is, at present, no one conception of human dignity that the decision-making elites have affirmed” (p. 86).

Arend purports to present “a new method for determining rules of international law [that draws] on the insights of both positivism and the New Haven School while attempting to avoid the pitfalls of both” (p. 86). He extracts part of the New Haven analytic framework for his own purposes, but discards (1) its inclusion of nonstate actors as independent sources of legal authority, (2) its focus on processes rather than rules, and (3) its teleological “dignity” component. The result is essentially a positivist method (albeit differing in minor respects from the approaches of certain positivist authors) expressed in New Haven vocabulary. Thus, “authority” is reduced to the *opinio juris* of states (i.e., the subjective sense of legal obligation that state actors manifest by their acts, omissions, and statements in particular contexts), and “control” is reduced to the consistency of conforming practice (pp. 86–104). The relationship of the two is helpfully illustrated in an assessment index, although this serves more to surface than to resolve the traditional ambiguities about how much evidence of either element is requisite to finding a legal norm.

Arend next addresses the effects of international legal rules on international behavior, and he argues for the superiority of a constructivist approach over the alternatives of structural realism and rationalist institutionalism (pp. 111–48). He goes on to project a “neo-medievalist” future for the international system, with Westphalian sovereignty dissolving into a more complex scheme of overlapping authority and multiple loyalties (pp. 171–85). He concludes that the study of international legal rules establishes the existence of an “international society” that can be comprehended only through interdisciplinary cooperation, all the more so as that society undergoes change (p. 190).

In laying out the grand scheme, Arend stakes out positions on a series of controversies within both international law and international relations. Although the positions he takes are consistently plausible, his arguments frequently fall short of a convincing vindication of the advocated stance. The book seeks to cover so much ground in so little space that it cannot do justice to the full range of competing considerations that attend each issue.

For example, Arend summarily adopts the controversial position that law is essentially a set of rules rather than a process (p. 26), thereby loading the discussion in the direction of legal positivism from the start. By focusing almost exclusively on the recognition of rules, he sidesteps the difficulties in interpretation and application that tend to problematize the positivist characterization of law. That law can be understood only as a purposive process is a theme of some of the most significant works of contemporary jurisprudence (e.g., Lon L. Fuller, “Positivism and Fidelity to Law—A Reply to Professor Hart,” *Harvard Law Review* 71 [February 1958]: 630–72; Ronald A. Dworkin, *Law’s Empire*, 1986). Although Arend engages in some detail (pp. 76–86) with a methodological tendency—the New Haven School—that regards law as process, he never fully acknowledges the grounds for that view. Given the book’s ambition to do justice to the competing tendencies, this is a significant omission, even if the “law as rules” characterization is considered to be convincing on the merits.

There is inevitably much to quibble about in Arend’s summary treatment of the various methodological frameworks, but his well-organized, well-written, and concise text provides an excellent starting point for thinking through the interconnections between international law and international relations scholarship. In this regard, the book is of value not only to graduate students and upper-level undergraduates

but also to scholars of either discipline, especially those who are relatively unfamiliar with the premises of, and with recent work within, the other discipline.

Semi-Detached Idealists: The British Peace Movement and International Relations, 1854–1945. By Martin Ceadel. New York: Oxford University Press, 2001. 477p. \$85.00.

Andrew Rigby, *Coventry University*

In 1980 Martin Ceadel published his definitive study, *Pacifism in Britain, 1914–1945*. Since then he has established himself as the leading historian of British peace movements. His prime focus in a series of scholarly publications is the analysis of the different stances toward peace and war adopted by the various organizations that could be considered part of the broad British peace movement in the first half of the twentieth century. In recent years he had delved farther back in time to produce a comprehensive two-volume survey of activism against war from the French Revolution until the dawn of the nuclear age.

In the first volume, *The Origins of War Prevention: The British Peace Movement and International Relations, 1730–1854*, Ceadel traces the process whereby—under the combined influences of the Enlightenment, evangelical Christianity, and changes in the international system (especially after the defeat of Napoleon in 1815)—there developed a belief that human agency can limit the incidence of war. In the aftermath of the French wars of 1793–1815, the first peace association was established in Britain, and as domestic politics gradually liberalized, the appeal of the Society for the Promotion of Permanent and Universal Peace, normally known as the Peace Society, broadened.

In the current volume Ceadel traces developments from the Crimean War to the end of World War II. He examines the manner in which the burgeoning belief that international relations, at least among “civilized nations,” can be conducted without war received something of a setback in 1854, when Britain declared war against Russia, and when the subsequent peace was disrupted by the American Civil War of 1861–65.

The extension of the franchise to working men in 1867 enabled the peace constituency to widen its appeal, which led to formation of the Workmen’s Peace Association. The Franco-Prussian War of 1870–71 and the subsequent armed peace between the two continental European powers stimulated interest in arbitration and the codification of international law.

During the last two decades of the nineteenth century, however, the significance of the Peace Society and associated organizations weakened as Britain embarked on its imperialistic military forays into Egypt and the Sudan. In the first decade of the twentieth century there was renewed interest in the “peace question.” The First Hague Peace Conference was convened, there was opposition to the Boer Wars in South Africa, and the growing socialist movement in Britain (and elsewhere) sought to bring an end to war by means of international working class solidarity.

The failure of the established peace societies to rise to the challenge of World War I left the field open to an array of new organizations, such as the Union of Democratic Control, the No-Conscription Fellowship, the League of Nations Society, and the Women’s International League. These organizations channeled the widespread disillusionment with the outcome of the war to end all war that reached its peak of

intensity during the early 1930s, when there was widespread commitment to disarmament and the strengthening of the League of Nations.

By 1936, however, with the rearmament of Germany, the Italian invasion of Abyssinia, and the outbreak of the Spanish Civil War, the British peace movement and public opinion began to polarize. The Peace Pledge Union rapidly expanded into Britain's only mass-based pacifist movement, but others began to acknowledge the need to prepare for the impending military struggle against fascism and the "containment" of Germany.

The experience of the war and the subsequent disclosure of the horrors of the concentration camps had a powerful influence on the British peace movement, particularly its pacifist wing. Was there a nonviolent answer to totalitarianism? How significant was conscientious objection against atomic weapons? By the 1950s the movement began to reassert itself with the emergence of the Campaign for Nuclear Disarmament, which is part of the worldwide movement to avert nuclear destruction that continues to this day.

The focus throughout the book is upon peace organizations as "ideological protagonists," concerned with the moral and practical problems posed by war and committed to propounding solutions to them. Ceadel includes within the general term "peace movement" all those who not only oppose aggressive war but also are inspired by the belief that it is possible to achieve a world without war. Within this category, however, he makes the linguistically painful distinction between *pacifism* (the conviction that war is always wrong and should never be resorted to) and *pacificism* (the belief that war can be abolished, but until the necessary circumstances have been achieved it may be necessary on occasion to resort to defensive military force). Ceadel has written for many years about what is essentially the difference between the absolute pacifists and others within the peace movement, but these labels are confusing. Indeed, it is clear that Ceadel understands the difficulties by italicizing the terms throughout the text.

Another problem is that the focus is upon the different analyses of the nature of war and the international system propounded by different historical actors over a period of nine decades, but there is very little discussion on the social or political significance and scale of the different organizations and associations. Indeed, an argument can be made that to apply the label "peace movement" to the British Peace Society in the nineteenth century is something of a misnomer. Yet, Ceadel reveals the quality of his insight into the dynamics of peace movements by likening them to a pilot light when conditions are unfavorable, "keeping the flame of peace thinking alive yet requiring the fuel of combustible public sentiment in order to ignite a mass campaign" (p. 4). When conditions are favorable, the peace movement can be likened to "a glider, drawing attention to the upcurrents of peace sentiment which sustain it yet unable either to power its own take-off or indefinitely to extend its time aloft" (p. 4).

Ceadel covers all the threads of peace thinking in Britain that had some kind of organizational manifestation over a period of ninety years, and he uses primary sources throughout. This is an awesome achievement. At times the text has a density that challenges the reader, such is the thoroughness of the coverage. But for anyone who aspires to teach or research on any aspect of British peace movement history in the nineteenth and first half of the twentieth century, this book should be their first source.

Constructing Europe's Identity: The External Dimension.

Edited by Lars-Erik Cederman. London: Lynne Rienner, 2001. 271p. \$55.00.

Gerard Delanty, *University of Liverpool*

In the last few years the question of the identity of Europe has moved from internal to external issues. Until about 1997, the issue was largely discussed in terms of whether a postnational identity on the European level might be able to compete with national identities. Many positions were taken, depending on theoretical and political assumptions that relate to identity formation and the future of the nation-state. Most of these debates presupposed the more or less established geopolitical space of mainly the western European Union (EU). This volume addresses a relatively new situation, in which a different logic of identity formation is becoming apparent.

Since about 1997, when the EU opened negotiations with the former communist countries, a project of geopolitical enlargement has been initiated. This volume looks at some of the implications of the mostly eastern expansion of a polity originally designed for a much more limited membership and more restricted scale of activities. The ten chapters provide a valuable study of an underresearched topic. The book adopts a constructivist approach to identity and examines the trade-offs in the making of a new postnational European identity, which is conceived as fluid and open to new codifications. This approach, which regards boundaries as central to identity formation, places the external boundary at the center of the stage. In this it is a welcome departure from the more common internalist arguments, which see collective identity as something self-referenced.

According to the constructivist approach, EU identity is becoming more and more defined by reference to the non-European boundary, as opposed to the EU's constitutive units, the national states. By focusing on the external dimension, these essays aim to bring a new perspective to bear on the theory of European collective identity. One advantage of this approach is that it does not rule out the chances for European identity to take a cultural form, as opposed to a purely minimal political identity. The construction of a European identity is not a mere act of negative identity, as has often been argued, but a process that involves clear trade-offs. Of particular interest in this regard is the complex relationship of, on the one side, inclusion and exclusion and, on the other, thick and thin identities.

The more Europe expands eastward, the more it will dilute its identity. Yet, the more protectionist Europe remains, the more it will have to exclude. This dilemma, the editor argues in his theoretical introduction, is the case with all forms of identity: The thinner they become, the more chances there are for greater inclusion, since thick identities tend to be more exclusive. A general conclusion that might be drawn from the various essays is that Europeanization is relatively flexible, and the external dimension does not necessarily result in straightforward closure. The contributors argue, to varying degrees, that the external dimension is compatible with a thin, postnationalist collective identity based on liberal, democratic values. There are some exceptions, largely in the area of immigration, but the general argument appears to be a recognition of new logics of openness in the construction of European identity. There is a clear move beyond the usual declarations of Fortress Europe as the inevitable product of European integration. This volume suggests a more subtle reading of a project that is more open-ended than is often thought.

Craig Calhoun, in a wide-ranging discussion of identity

formation, does not see a fundamental problem with the external dimension in the making of European identity, since the essence of Europe is "inconsistency." Internally heterogeneous, Europe's strength would be to create institutional structures to encourage diversity, he argues. This is followed by a chapter on Switzerland, which presents a perplexing example of, on the one side, pluralism and, on the other, relative closure. The three authors, Pascal Sciarini, Simon Hug, and Cédric Dupont, demonstrate how exclusionary practices go together with inclusive institution building. As much an exception as an example, it shows the relative costs of a high degree of inclusion.

Philip Schlesinger, in a chapter on the cultural policies of the EU, documents the decline of anti-Americanism but notes that political communication is primarily national. This is followed by a chapter on audiovisual culture by Tobias Thelier, who shows that the EU attempt to construct a European cultural dimension in opposition to American global culture has been unsuccessful. These chapters, which deal mostly with the relation of Europe to the United States, show that the construction of a European cultural identity will at the most result in a thin identity.

When the external dimension is Russia as opposed to America, the result is likely to be a stronger affirmation of identity, as Iver Neumann argues in a chapter on eastern expansion. The chapter by Frank Schimmelfennig takes a slightly different view of eastern expansion, which he sees as less influenced by considerations of exclusion. The Union's thin postnational identity will allow it to expand to include states that share its liberal and democratic values.

Two chapters, one on immigration by Jeff Huysmans and one on asylum policies by Vera Gowland-Debbas, address issues of exclusion. It must be borne in mind, as Huysmans argues, that policies in these areas are mostly shaped by national concerns and do not reflect a European vision of a cultural threat. The final chapter, by Cederman, sums up some of the arguments around the theme of exclusion versus dilution. His question is whether this is a real or imagined trade-off.

The general conclusion is that the external factor does not result in an inherent tendency toward closure. Overall, the volume is a fairly coherent and well-sustained examination of how European integration is shaped by the external factor, which must be seen as a dimension that can be constructed in a wide variety of ways. The theoretical focus on trade-offs gives the book a certain uniqueness, although it tends to reduce the scope to a conception of identity as based on transaction costs.

Sharing Security: The Political Economy of Burdensharing.

By Malcolm Chalmers. New York: St. Martin's, 2001. 288p. \$69.95.

Danny Unger, *Northern Illinois University*

Malcolm Chalmers examines how costs are apportioned among states in pursuit of common goals. He focuses on two sets of states, the victors of World War II (France, the United Kingdom, and the United States) and civilian (defeated) powers (Germany, Italy, and Japan). As this categorization suggests, Chalmers believes that the end of the Cold War did not fundamentally revise the global order created after 1945. Domestic norms, the politics of pork that support military spending by the victor powers, and multilateral institutions all helped to freeze in place postwar arrangements. Disproportionate U.S. military spending represents not merely free riding among its allies but the fundamental postwar bargain

in which the defeated powers limited their military spending in order to reassure their allies, China and the Soviet Union, as well as their respective publics.

To explain the distribution of effort across states, Chalmers considers the relative utility of public goods theory, liberal institutionalism, realism, and strategic culture theory. He uses this analytic framework to examine a number of cases: the North Atlantic Treaty Organization (NATO), the European Union (EU), development assistance within the context of the Organization for Economic Co-operation and Development's (OECD) Development Assistance Committee, the United Nations, and the Climate Change Convention. Public goods theory helps to account for asymmetrical U.S. military spending among the NATO countries but not U.S. free riding within other regimes. Liberal institutionalism usefully directs attention to the costs of renegotiating existing formulas that determine the distribution of burden among states. Realism helps us understand many of the policies of the three victor powers. Chalmers, however, leans most heavily on strategic culture theory (the effect of history and domestic politics) to explain the policies of both groups of states.

Chalmers acknowledges the problems in assuming that defense spending by NATO allies represented public goods. He notes, for example, that the victor powers' higher levels of spending resulted in substantial part from spending for military deployments outside Europe that constituted private and, at times, competitive goods. The civilian powers eschewed military deployments beyond their borders or nuclear strategies, the two elements of military expenditures that alone account for most of the gap in levels of military spending between the victor and civilian powers.

When he turns to other issue areas, however, Chalmers is less consistently attuned to the question of whether goal divergences among the powers make problematic the notion that public goods assumptions apply. Are increases in Official Development Assistance (ODA) a public good? Certainly, officials avowed commitments to ODA increases. Other officials and many political leaders, however, especially in the United States, argued that public assistance undermined development goals. Chalmers maintains that ODA increases "should" play central roles in burden-sharing debates. Perhaps they should, but do they?

As Chalmers notes, ODA spending has been linked to a variety of private goods, evident in the Italian corruption cases of the early 1990s, as well as to rule under social democratic parties. Denmark, the Netherlands, Norway, and Sweden, for example, account for only 4% of the OECD population but well over one-fifth of its economic assistance to poorer countries. An effective political consensus in these countries probably views this aid as a global public good, it is unclear that this is the case everywhere. Similar questions arise in considering proposed cooperation in reducing greenhouse gas emissions under the Kyoto Protocol of the global climate treaty. Chalmers makes no headway in grappling with these problems, which typically plague the public goods literature.

The book focuses on the argument that the institutional form of leadership matters. Chalmers notes that U.S. officials, at best, are ambivalent about multilateralism; they hope to constrain others while maintaining their own freedom of maneuver. As a result, regimes that include the United States are apt to remain weak. In sharp contrast, German leaders have been fully committed to the European Union.

The German case is indeed remarkable and powerfully supports Chalmers's arguments. From the European Community's inception, Germany made offers that French leaders could not refuse on the Common Agriculture Policy in return

for French acquiescence to elements of supranationality in European institutions. Animated by political concerns, Germany's net contribution to the EU in the late 1990s amounted to about one-third of its defense budget. With a GDP only half that of France, Italy, and the United Kingdom combined, Germany made a net contribution three times as large. If this spending is included in calculating "aid" expenditures, Germany's total outstripped that of Japan and the United States combined. Still more remarkably, these contributions to the EU flowed through multilateral channels that diminished the political leverage afforded to German leaders by this largesse. Largely due to German leadership, Chalmers concludes, and despite the many problems facing the European Union in the process of enlargement, Europe "appears doomed to reach agreement" (p. 110).

The author's central concern is that the development of international regimes is constrained by U.S. strategic culture, and this limits their potential roles in securing global peace. Burden-sharing disputes triggered by the United States over small sums of money stymie the development of the United Nations, whereas German acceptance of dramatic spending asymmetries has made possible impressive institutional development in Europe. If realism can help us understand the policies of the victor powers, it cannot account easily for those of the civilian powers, particularly Germany. To understand German postwar policies we have to draw on liberal institutionalist and strategic culture explanations.

Globalization and the rise of the EU as a counterweight may eventually shift U.S. elite attitudes that see multilateralism as a threat. Chalmers likens these U.S. views to those evident in the United Kingdom in the 1950s. He offers some hope that the diversity of public goods provision among the OECD big six may enhance global levels and the quality of public goods provision. German support for the EU, for example, helps to secure the European peace, just as U.S. aircraft carriers contribute to stability in the Pacific. He concludes, however, that the United States often blocks efforts to address key contemporary security problems (poverty reduction in developing countries, curbs on environmental degradation.) As a result, the EU and Japan may need to assert leadership in these areas if the long peace of the second half of the twentieth century is to be sustained.

Threats and Promises: The Pursuit of International Influence. By James W. Davis, Jr. Baltimore, MD: Johns Hopkins University Press, 2000. 222p. \$32.50.

Joseph Lepgold, *Georgetown University*

How should threats, promises, and assurances be used as tools of foreign policy? In this tightly written and well-reasoned book, James Davis contends that international relations scholars, who overgeneralize from the failure of appeasement in the 1930s, focus too much on the efficacy and dilemmas of sticks and not enough on the well-placed use of carrots. His objective—an ambitious one, judging from the absence of comparable works in the literature—is an argument that integrates positive and negative sanctions into a single theory of interstate influence.

Davis argues that deterrence theory, in which aggression is seen as driven by a defender's weakness, oversimplifies actors' motives. Davis contends that when an actor's motives are opportunistic, threats of punishment for aggression make sense, but when the motivation is a sense of domestic or international weakness, the actor is likelier to fight than retreat when pushed into a corner. This reasoning reflects the

finding of prospect theorists that people are more likely to take risks to prevent losses than to make gains. By this logic, once one knows how a decision maker is framing his reference point with respect to some issue, his motives—either to forestall perceived losses or make perceived gains—can be discerned. By this reasoning, if the actor fears loss on a key issue, promises are likelier than threats to induce compliance. Moreover, according to Davis, even if one actor is trying to improve the status quo at another's expense, deterrent threats are more likely to succeed when they are accompanied by credible assurances that the punishment will be withheld if the actor behaves cooperatively.

This argument leads Davis to several hypotheses (pp. 36, 37). (1) "Threats that increase the prospective costs associated with a given course of action may be highly effective at deterring a decision maker operating in the domain of gain but *ceteris paribus* will be less effective when decision makers are in the domain of loss." (2) "The most effective promises should be those that offer the reduction or elimination of the target's losses rather than those that augment another attribute, because the corresponding increase in utility takes place on a steeper limb of the value function." (3) "To the degree to which they reduce uncertainty and increase the probability that the reference outcome will obtain, assurances can reduce fears and anxieties that often have led to assertive or aggressive foreign policies."

These hypotheses are tested across a series of cases from German diplomatic history in the nineteenth and early twentieth century: the absorption of Schleswig and Holstein; the Franco-Prussian War and the unification of Germany; German confrontations with France over colonial rights in Morocco; and the onset of World War I. These cases comprise both opportunistic and defensive motives for German behavior that challenged the status quo and the full range of methods—threats, promises, and assurances—that the other European powers used to shape German behavior. As a group, they thus exhibit variation on the independent variables (influence technique and the target's motive for expansion) and the dependent variable (the target's choice of whether to back down or use force in reaction to that influence attempt).

The cases show that a target's motives significantly affect the efficacy of threats, promises, or assurances in producing compliance with others' demands. When German leaders tried to expand because they thought others would not resist them, as in several of the Schleswig-Holstein crises, threats of Russian and French intervention caused them to back down. By contrast, when Bismarck feared in 1875 that France might recover quickly from the recent war and seek to regain its lost provinces of Alsace and Lorraine, Russian and French leaders reassured him that France would not try to revise the status quo. Once Bismarck believed that his recent territorial gains were safe, he allowed the crisis he had precipitated to dissipate. In all three cases involving Germany's colonial holdings in Africa, policymakers in Berlin saw their position deteriorating relative to the other great powers. As Davis shows, they responded in kind to tough talk and actions from Paris and London, but they behaved more cooperatively when promised compensation elsewhere in Africa and its environs.

In testing the hypotheses, Davis skillfully operationalizes the key variable in prospect theory, which states that actors evaluate prospective outcomes with respect to a "reference point," typically their conception of the legitimate status quo. Outcomes that improve on that point are considered gains; those inferior to it are seen as losses. For each case, Davis carefully examines how actors came to define the status quo.

His interpretations as to which instances of expansion were driven by perceived opportunities to improve on the existing situation and which were viewed as defensive reactions to prospective loss are usually convincing. In reconstructing these decision-making assessments, Davis mines the secondary literature and examines an impressive range of primary sources. His interpretations of the cases are at times bold—he does not hesitate to challenge the prevailing scholarly consensus about actors' motives—yet plausible, if not entirely persuasive.

Davis is willing to go out on a limb in discussing the policy implications of his argument: "If political leaders frame choices around a reference point and respond differently to prospective gains and losses vis-à-vis the reference point, then prospect theory provides answers to fundamental questions of statecraft" (p. 36). This claim is problematic. Scholars can work to reconstruct actors' decisional frames at whatever pace is deemed necessary, but policymakers in emerging crises have no such luxury. They need valid diagnoses of their counterparts' motives in the short run. Without good evidence on other actors' decision-making processes, such diagnoses are often difficult, if not impossible. As students of international relations are well aware, offensive and defensive motivations often produce similar observable behavior. Furthermore, as Davis admits, we lack a theory of how actors come to decisional frames. "Most political decision makers can imagine multiple reference points for any set of outcomes, and the proper referent is often the subject of intense political debate" (p. 41). If policymakers do not know their counterparts' reference points, they have no way to determine whether aggressive behavior reflects greed or fear. Lacking that knowledge, they have little basis for deciding whether to emphasize sticks or carrots as policy tools.

To his credit, Davis acknowledges and tries to deal with these problems. If statesmen can simultaneously have offensive and defensive goals, and if it can be difficult even with good documentary evidence to discern actors' motives, policymakers in most cases should use an array of influence techniques. Deterrence can be used to identify clear boundaries for the other's behavior; promises and assurances can be used to make the status quo more attractive and stable. When the actor is trying to uphold a status quo that is reasonably secure, or enjoys a healthy advantage in power over a challenger, she might take the risk of erring on the side of offering inducements. Conversely, if the challenger has equal power or a clear military advantage, it might make sense to risk on the side of threats, even if that choice turns out to produce an unwanted conflict spiral.

No one has a clear mirror on nature: Policymakers' assessments of the motives of others may reflect overly sticky beliefs, and the evidence itself often may be ambiguous even to an unbiased observer. For these reasons, it can be hard to know how to combine threats and promises into an effective strategy. Even so, Davis has written a perceptive and thought-provoking book. He emphasizes, against the thrust of much contemporary literature, that "even when statesmen confront an opportunist, successful deterrence requires the ability to issue both threats and assurances" (p. 154). This is surely correct: Force alone rarely suffices to stabilize the status quo over the long run. Durable deterrence requires at least some agreement on a mutually legitimate status quo. In developing an argument that drives home this point, Davis illuminates issues central to bargaining, strategic choice, and social as well as individual cognition.

Fascist and Liberal Visions of War: Fuller, Liddell Hart, Doughet, and Other Modernists. By Azar Gat. Oxford: Clarendon Press/Oxford University Press, 1998. 352p. \$85.00.

Frederic S. Pearson, *Wayne State University*

It is difficult to categorize this intriguing and illuminating volume, since it is not entirely an intellectual history of modernity and warfare, although it has elements thereof. Nor is it strictly a modern history of Western strategic thought on war, although it is so in large part. As the last in the author's trilogy of studies of strategic thought, it appears to be an amalgam of twentieth-century perspectives following from the other two volumes, aimed at an ultimate consideration of the direction of liberal versus authoritarian thought on war. The book carries on nicely from the previous work, subtly raising crucial policy questions for our time, but does not entirely cohere as a single study.

Azar Gat, of the Department of Political Science at the University of Tel Aviv, reveals important patterns and interrelations in the thought of European military, political, and journalistic figures on the Right and moderate Left, from the early to middle twentieth century. The focus wanders somewhat from initial concern with the relation of philosophies such as fascism, or protofascism, to the institution of modern war, on the one hand, to alternate conceptions of strategies and tactics for fighting, winning, or avoiding wars, on the other. There is a great deal here for students of intellectual and political thought as well as strategic thought and, especially in the later chapters, for students of international relations. The book will probably not satisfy any of these constituencies fully, however, since Gat dwells on the work of particular influential people, such as Liddell Hart. Thus, the range and depth of analysis vary.

The thread connecting fascist and liberal military thought is the notion of modernity and modernist theory. This is a fascinating concept and is already well known in the study of armament and war, as seen in the works of Keith Krause (*Arms and the State*, 1992), Russell Warren Howe (*Weapons*, 1980), Freeman Dyson (*Weapons and Hope*, 1984), and others. The modernist connection is made most tellingly in the first half of the book, which deals mainly with protofascist thought in Britain, Italy, and Germany. The individuals whose ideas are reviewed—J.F.C. Fuller, Giulio Douhet, Ernst Junger, and others—were by and large fascinated with the implications of emerging machine technology and organizational styles for society and war. The author skillfully paints a portrait of a restless *fin de siècle* generation looking for meaning and disillusioned with mass democracy, aristocratic hierarchy, and traditional folkways.

Despite variations among some strange bedfellows (including the likes of H.G. Wells and other futurist literary figures) and obvious exceptions in the *volksische* sentimentality and sometime mysticism of National Socialist and fascist ideologues, most of the individuals sampled here predicted a new form of Darwinian social order. It would be based on expert regimes and mechanized warfare, with particular excitement about tanks and aviation, and would lead to transformed and supposedly heightened civilizations. The author shows there was keen technological fascination in other societies as well, notably America and Marxist Russia, and this fascination had its effect on military theory in these states, but he maintains that the "pioneering" linkage to war fighting sprang most readily from the fascist thought.

In view of Gat's introduction, one might have expected a similar consideration of the links between the philosophical tenets of liberalism and modernist military thought in the

second half of the book. Instead, the focus shifts to the development of British strategy to contain Germany before World War II, particularly the evolution of Basil Liddell Hart's thinking from roughly 1920 onward. Disillusionment regarding total war is tested against the unmistakable need to take some effective action to thwart the advance of Germany and the fascist bloc. Liddell Hart's views are placed in the context of difficult choices faced and fumbled by the British government. The link to liberalism is not made as explicitly as in the fascist examples, and there is little discussion of basic liberal tenets regarding economic and political competition/cooperation or modernization. Yet, in the end the author shows that as total war became anathema to interwar liberal thinkers such as Liddell Hart, they reverted instead to limited war strategies ("limited liability") and containment (embargoes, sanctions, collective security). This explains much of the delayed response to Hitler by Britain and France.

The volume is at once highly insightful and uneven. One wonders, for example, why no significant sections were devoted to French fascist-militarist thought and the question of modernity. French writers and military and political leaders are mentioned *en passant*, it is mystifying that the considerable radical Right tradition (e.g., *Falangist*) and the legacy of Dreyfus are not given more systematic treatment. Did France have no techno-military fascination (unlikely, given the French aviation sector)? Is French protofascism assumed to be similar to the Italian variety? Or is the author simply more at ease dealing with the Italian, German, and British cases?

Equally baffling and revealing of a potential logical fallacy is the rather short shrift given to the U.S. case. Gat tries to contrast his continental and British findings with other places noted for technological modernism (unfortunately, not including Japan). American Taylorism and Fordism indeed were adopted as models by the protofascist modernizers. What starts out to be a comparison and contrast, however, ends up with very little contrast; there is little explanation of why "most technological modernists in the United States who took a critical stand towards capitalism . . . remained within the democratic camp" (p. 109). It may be that most protofascists were techno-modernists, but one cannot conclude, and Gat would not argue, that most techno-modernists were necessarily fascistic. Yet, Gat notes that many of them, such as Lewis Mumford and Frank Lloyd Wright, evidently because they "inclined toward communitarianism, environmentalism, and social planning," had considerable appeal among protofascists.

Instead of making the distinctions and explanations clear in counterexamples such as the American context, Gat dwells on the protofascism and anti-Semitism of Henry Ford, Thomas Edison, and Charles Lindbergh, which he traces, along with their technological tinkering, to the Midwestern parochialism from which they all sprang. Here, in contrast to his thorough treatment of European trends, the author appears to be writing as if from a distance, mixing varied movements and styles ("agrarian nostalgia, nativist Populism, Progressivism, and celebration of technology," p. 107). The reader is left with a less intimate understanding and careful discussion of the intellectual undercurrents of the time, much of which were driven as well by eastern thought, replete with its own brands of techno-capitalism and anti-Semitism.

This book and subject matter perforce require the balancing of divergent and contradictory outlooks even for the same individuals. Fascists can be at once forward and backward looking. Liberal democrats can be torn among pacifistic revulsion, pragmatic skepticism, and reluctant acceptance of or even enthusiasm for war, but wars of varying sorts and

styles. Ironically, in discussing British liberalism's interwar gun-shyness, Gat shows that Hitler's generals and others in the authoritarian camp likewise were reluctant to launch into total war (pp. 241–2). He also shows (p. 255) that liberals such as Liddell Hart were, given strategic needs, supportive of advanced weapons and new doctrines.

Despite inconsistencies in the overall thesis, and with the exception of the American context, Gat does a remarkable job of making this complex and very human side of political thought palpable. At a time when democratic peace theory is so strongly proposed and so hotly debated, his work hauntingly suggests crucial and still fundamentally unanswered questions. To what extent can liberal democracies be relied upon to undertake global or regional police or alliance functions, using what sorts of strategies in what specific contexts? What are the effects of political, industrial, and military culture, as well as scientific fashion, on evolving defense doctrines and security agreements? What do the European Union and similar integration efforts portend for regional or Atlantic defense policy? Do political philosophy, regime type, and technological orientation really predict military strategy?

Democracy Beyond the State?: The European Dilemma and the Emerging World Order. Edited by Michael Th. Greven and Louis W. Pauly. New York: Rowman & Littlefield, 2000. 191p. \$65.00 cloth, \$24.95 paper.

Mark Pollack, *University of Wisconsin-Madison*

Over the past decade, alongside the development of globalization and regional integration, politicians, activists, and scholars point to the emergence of a "democratic deficit," which purportedly results from the internationalization of domestic economies and the inadequate democratic credentials of new and existing international organizations. Much of this literature—exemplified by Fritz Scharpf's seminal *Governing in Europe: Effective and Democratic* (1999)—focuses on the experience of the European Union (EU) and examines the prospects for closing the democratic deficit through domestic and EU-level reforms.

In *Democracy Beyond the Nation-State*, the editors and contributors (Stephen Newman, Michael Zürn, Claus Offe, Edgar Grande, and Stephen Clarkson) broaden the perspective of the democratic deficit debate in both theoretical and empirical terms. By comparison with Scharpf and other key works on the EU, which draw heavily from international political economy, the essays in this volume draw more extensively on political theory, in particular the work of David Held, Michael Sandel, and Robert Dahl. In empirical terms, the four core essays (by Greven, Zürn, Offe, and Grande) take the EU as the starting point of analysis, as does Scharpf; but a penultimate chapter by Clarkson makes an explicit comparison with the NAFTA case. Ultimately, all the chapters examine two broader questions: whether globalization erodes democracy at the national level and whether democratic values can be realized beyond the nation-state. Although the authors come to no consensus position on either issue, the volume is well integrated because each author notes points of agreement and disagreement among the various essays.

Regarding the effects of globalization on national democracies, Newman sets the tone of the volume. He argues that economic globalization tends to weaken democracy, if and insofar as welfare-state and other domestic policies respond to the demands of the global marketplace rather than those of national polities and their elected representatives. At the

extreme, this process may end in a deregulatory “race to the bottom” in which democratically expressed demands for social protection are ignored in favor of policies aimed at attracting mobile capital. In Offe’s words, the so-called *acquis communautaire*—the body of legislation accumulated by the EU and devoted primarily to market liberalization—now threatens the West European *acquis nationale* of strong liberal democracy and well-developed welfare states.

Clarkson’s chapter makes a similar argument regarding NAFTA, although his survey of its effects on the United States, Canada, and Mexico reveals a more subtle picture. Canadians appear to have lost democratic control to the market and to their southern neighbor, whereas Mexican democracy arguably has been strengthened on balance by NAFTA membership, and U.S. policies remain relatively unaffected. By and large, however, the contributors take the corrosive effects of globalization on democracy and the welfare state as a given and focus on the second question: Can a race to the bottom be averted and democracy regained, beyond the state, either globally or in regional organizations such as the EU and NAFTA?

The initial response of the book to its titular question is pessimistic. By contrast with David Held, whose model of a “cosmopolitan democracy” is widely discussed by the contributors, Newman, Greven, and Offe argue that the criteria associated with representative democracy are unlikely to be replicated beyond the state, even by organizations that, like the EU, demonstrate the highest levels of institutional development. Greven and Offe provide particularly critical reviews of the EU’s democratic prospects, not because of any specific institutional failing (although they cite a number of these) but because the EU and other international organizations lack two fundamental prerequisites for even the most rudimentary form of democracy.

First, both authors point to the absence beyond the state of a *demos*, a group of people united by a sense of community or “we-feeling” that could provide the constituent basis for democracy and for the extension of the civil, political, and above all social rights to all its members. This is a common critique within the EU literature on the democratic deficit, and it applies with even greater force to the global level, the authors suggest.

Second, and related to the first, Greven argues that democracy requires a single political space, “a distinct realm of communication and meaning” within which a democratic polity debates and deliberates (p. 44). The EU has arguably attempted to establish such a political space, with extensive consultation of civil-society groups and periodic election of representatives to the European Parliament, but Greven argues persuasively that the European political sphere remains limited to a relatively few Brussels insiders, stunted in its development by the diversity of languages and political cultures throughout the EU. For both of these reasons, the EU’s democratic deficit is arguably structural rather than institutional (Grande, p. 120) and is therefore unlikely to be overcome by transferring more powers to the “so-called European parliament” (Greven, p. 50).

This book is not, however, a counsel of despair, and the later essays by Zürn and Grande, although acknowledging the absence of a fully developed European or international *demos*, argue for a broader conception of democracy that might be compatible with international organizations like the EU. Zürn gives much attention to the promise of “deliberative democracy,” which has been the subject of study on the local level in the United States but is increasingly presented as a possible strength of the EU’s technocratic, committee-driven system of governance. Such deliberation should be

encouraged, according to Zürn, although it comes at a cost insofar as most EU deliberation takes place *in camera* among unelected representatives and therefore suffers in terms of both transparency and representativeness. Grande argues explicitly for a model of “post-national democracy” that would pragmatically combine nonmajoritarian elements—including most notably consensus decision making and a system of institutional checks and balances—together with the occasional use of direct democracy in the form of Europe-wide referenda (also favored by Zürn).

Perhaps the most striking aspect of this volume is the apparently unanimous agreement among otherwise disparate authors that the EU—and by extension other, less developed international organizations—fares poorly according to traditional standards of representative democracy, and the often discussed remedy of strengthening the European Parliament offers at best limited promise. Instead, the real debate within the book concerns whether the demonstrated failings of the EU and other international organizations in terms of representative democracy can, and should, be compensated by the more extensive use of nonmajoritarian checks and balances, deliberation, consultation with civil society groups, and occasional direct democracy. None of these expedients provides a functional equivalent for representative democracy, the authors admit, and some (like technocratic deliberation) may be inherently hostile to it, yet they may be the closest approximation to democracy possible beyond the nation-state.

European Integration: Scope and Limits. By Martin Holmes. New York: Palgrave, 2001. 242p. \$69.95.

The French Road to European Monetary Union. By David J. Howarth. New York: Palgrave, 2001. 239p. \$69.95.

Carolyn Rhodes, *Utah State University*

These are two very different books, both in terms of content and purpose. Howarth offers a straightforward academic explanation of French monetary policy from 1968 to 1999, and he provides a useful, if not unfamiliar, view of French motives for monetary integration. The Holmes book is equally straightforward, but it is a polemic against European integration and British participation in the current European Union (EU) structure. Despite their dissimilarities, however, both books provide interesting insights into the politics of European integration in these two countries, whose histories, ideological orientations, and economic structures vary so widely. What first appeared to me an odd juxtaposition of books for review now seems an insightful exercise for better understanding national reactions to the development of the EU.

Howarth’s analysis of French monetary policy generally reinforces what most of us in EU studies have known for some time—France has continually pursued a monetary policy aimed at enhancing or protecting French national power, and it only accepted European monetary integration as a means of containing the influence of the Bundesbank and advancing its own monetary interests. Howarth convincingly shows that although France was also concerned with insulating the franc from dollar pressures, it was a growing concern over the relationship between the franc and the deutchmark that prompted definitive steps toward European monetary integration.

Driven by a strong currency policy, both for nationalistic and to a degree sound economic (particularly its dependence on dollar-denominated oil imports) purposes, successive French governments struggled with stagflation, high debt,

balance-of-payments problems, and reflationary demands from domestic interests. Often unable to match macroeconomic policies with a desire for a strong franc, leaders from DeGaulle to Mitterand wrestled with how to use European monetary integration to enhance their control over a currency whose value increasingly depended upon cooperation from the Bundesbank. Institutional structures at the European level became more and more attractive as it became clear to the French political elite that the French Central Bank did not have the resources on its own to manage the value of the franc.

This story, which Howarth presents systematically and cogently, reaffirms our understanding of why France, such a nationalistic country, would become a major proponent of monetary union. The realities of the global economy of the 1970s and 1980s, along with the geopolitical events in Europe of 1989–90, convinced French leadership that German economic and financial influence would continue to reduce France's national monetary power. In the tradition of the European Coal and Steel Community, and much to the dismay of a marginalized Left and the far Right, the French government decided that a monetary partnership (within EU institutions that it could influence) with its erstwhile monetary competitor would better serve its own financial and economic purposes than going it alone. Thus, deeper economic and financial integration would paradoxically offer more monetary control through supranational political structures than would an autonomous, nationalistic policy.

The Holmes book demonstrates a very different experience with European integration, born from Britain's more global economic and political history and from the perception that its distance from Europe, in mindset as much as in miles, sets its interests apart from those of the Continent. This is a collection of essays critical of the EU and of British governments whenever they ventured into deeper EU integration, and it is an unabashed representation of the intellectual and nationalistic arguments of the Conservative Party's Euroskeptics. Written from 1994 through 2000, the essays reveal this group's concerns regarding such issues as Europe's single currency, enlargement, and tax harmonization. In addition, there is a very critical assessment of John Major's ill-conceived effort to balance his policy toward EU integration in such a way as to satisfy the Euroskeptics in his own party while still venturing into deeper EU cooperation. According to Holmes, Major only played the role of Euroskeptic himself when he was attempting to retain party control in the face of Euro-criticism. When the discrepancy between the direction of his policy and his Euroskeptical pronouncements became too great, the Conservatives rejected (and rightly so, according to Holmes) his leadership. The choice of William Hague, a Euroskeptic from the start, in 1997 to lead the party was, according to the author, a positive move that makes clear the Conservative stand on European integration.

Referring again and again to the "right kind of EU," not one "run by a cling-on Commission" that defies democracy and has no accountability, Holmes argues that the EU should be reformed into a free trade association whose member states retain ultimate say on whether to participate in new EU legislation (p. 57). He is very critical of the cohesion fund (which he labels a pork-barrel boondoggle), arguing instead that true free trade would eventually address regional development challenges. Holmes accuses the EU of reneging on single-market pledges in favor of protectionist structures in case after case, and he offers a persuasive argument against the European model of custom's union and socialized economic structures. Yet, he takes this argument too far, ignoring the real free market success stories of European integration and the EU's leadership (albeit mixed) at the World Trade Organization. Like most polemics, his does not at-

tempt to present a balanced assessment; it offers one view and arranges the evidence accordingly.

The French Road to European Monetary Union is by no means earth shattering in its discoveries or approach. It is a solid book that takes the reader systematically through successive French governments and explains their remarkable consistency in monetary policy regardless of the party in control. The book is well developed and nicely written, and although the author's attempt to categorize French policies in terms of negative power, positive power, and veto seems more like an "add-on" than a fully integrated analytical approach, it is somewhat useful.

European Integration: Scope and Limits is also interesting but for different reasons. The book is mistitled. One expects a broad explanation of the integration process and some insights into why integration has limits across Europe. Instead, it is entirely a British discussion about why the United Kingdom should not become more deeply imbedded in the EU. It makes no effort to provide a balanced assessment.

The Deadly Ethnic Riot. By Donald L. Horowitz. Berkeley: University of California Press, 2001. 605p. \$35.00.

Stephen M. Saideman, *Texas Tech University*

Donald Horowitz's previous volume (*Ethnic Groups in Conflict*, 1985) was perhaps the most important book written on the subject in the 1980s; it not only covered groups around the world but also addressed a variety of topics, including the nature of ethnic identity, intergroup relations, ethnicity and the military, separatism, and political institutions. *The Deadly Ethnic Riot* considers a very specific form of ethnic conflict, but his focus should not limit its effect. It has implications beyond ethnic riots and will challenge scholars in the field to think more seriously about social psychological approaches to ethnic conflict and to think more clearly in general about ethnic strife.

Horowitz argues that the ethnic riot is distinct from other forms of ethnic conflict (feuds, lynching, violent protests, terrorism, civil war, genocide). An ethnic riot is "an intense, sudden, though not necessarily wholly unplanned, lethal attack by civilian members of one ethnic group on civilian members of another ethnic group, the victims chosen because of their group membership" (p. 1). He delineates the processes through which riots occur, moving from rumors and precipitants, to lulls, to the sudden killings and mutilations of a particular group, to the consequences of riots for further strife. The book would make a significant contribution simply as a descriptive account of ethnic riots, but Horowitz goes beyond this to examine their causes.

Horowitz argues that social psychology presents a better perspective than other approaches for understanding why ethnic groups engage in conflict in certain forms against particular targets at specific times. The key move is target selection, which he suggests is a mixture of "calculation and passion." Horowitz observes that rioters are quite selective in choosing their victims, focusing on one hated ethnic group rather than on others, and are willing to risk losing a potential target if there is some doubt about group affiliation. He considers what factors cause certain groups to be targeted rather than others. Targeting is problematic, and therefore theoretically interesting, because a group often has conflict with more than one group, so there can be variation over time and space.

Horowitz identifies several necessary conditions for ethnic riots to occur: Two groups must have a hostile relationship; one or more events and/or policies provoke a group (a precipitant); justification for killing; and a relatively low risk that engaging in violence will be costly to the attackers (p. 524). Horowitz takes hatred/antipathy seriously, considering

the various roles it plays in ethnic conflict in general and in ethnic riots more specifically, but the hostile relationship is not the same as ancient hatreds. The hostility must be current, although it may hark back to past grievances. Precipitants are very important, because not just any situation will trigger ethnic conflict. Only certain events will evoke the angry response that unleashes the killing. One path for future work is to consider further what kinds of events or policies are more or less likely to provoke violence. People also must feel justified in their actions; if not, near riots will sputter out or become something else. The last necessary condition, a perception that risks are relatively low, is quite striking. Although rioters often are seen as irrational, Horowitz maintains they attack only when they are unlikely to pay the costs of their actions. Thus, riots are more likely when the attackers greatly outnumber the targets, when the police are absent or allies, and when politicians give them cover. If any one of these conditions is absent, then a deadly ethnic riot will not occur, although other forms of violence may. Thus, the book provides some policy advice.

Horowitz makes an explicit attempt to counter recent arguments that focus on the security dilemma (Barry Posen, "The Security Dilemma and Ethnic Conflict," *Survival* 35 [1993]: 27–47) and rational choice explanations (Robert H. Bates, Rui J.P. De Figueiredo, Jr., and Barry R. Weingast, "The Politics of Interpretation," *Politics and Society* 26, 4 [1998]: 603–42, and James D. Fearon, "Commitment Problems and the Spread of Ethnic Conflict," in David Lake and Donald Rothchild, eds., *The International Spread of Ethnic Conflict*, 1998). Horowitz is largely successful in asserting that hate plays an important role in ethnic conflict because he considers how emotion and reason interact, rather than argue that the existence of antipathy means that actors are completely irrational. As a result, his book presents a challenge to recent work and forces scholars to consider more seriously the roles played by emotions, particularly hate, in ethnic strife.

Horowitz uses ethnic riots to illustrate a variety of points, and this leads to two problems for subsequent research. First, because his book is a mix of social psychological theory and inductive reasoning, it is hard to tell at times whether the theory is providing new insights into ethnic riots or whether the examination of ethnic riots is telling us new things. It is important to know what the theory does and does not tell us, so that we can extend and evaluate it. Second, because the method is a close examination of the relevant population, it is hard to determine which causal relationships matter. The book presents neither a set of case studies nor quantitative analyses. Therefore, it is difficult to establish what we really know. Horowitz considers these issues in a chapter on nonriots. He discusses how one could possibly select cases, given that it is hard to determine which nonevents one ought to study in comparison to some of the riots he analyzes.

This book makes an important contribution to our understanding of ethnic conflict. It will be a source of testable hypotheses for years to come, as Horowitz makes claims about what kinds of inter- and intragroup characteristics are likely to cause conflict (group cohesion attracts aggression) and which are not (cultural distance and economic grievances may not matter). He also provides some clues for several unanswered questions: Why do followers follow? Do leaders follow or lead? When and why do groups act as groups? One of the book's greatest potential contributions is to force scholars to think about different kinds of ethnic conflicts—how ethnic riots are similar to and different from peaceful protests, terrorism, organized rebellion, and the like. Thus far, much of the debate has focused on the presence or absence of violence, rather than the form violence may take. *The Deadly Ethnic Riot* will compel scholars

to theorize about that which is common to all ethnic conflict and that which varies, depending upon the type of conflict.

Stable Peace among Nations. Edited by Arie M. Kacowicz, Yaacov Bar-Siman-Tov, Ole Elgström, and Magnus Jerneck. Lanham, MD: Rowman & Littlefield, 2000. 352p. \$75.00 cloth, \$26.95 paper.

Stephen R. Rock, *Vassar College*

Most scholars have long recognized that international politics is not always and everywhere a Hobbesian war of all against all. Rather, certain states have formed what Karl Deutsch called security-communities. Within these communities prevail relations of "stable peace," a term coined by Kenneth Boulding to denote a peace that is not merely the absence of war, but a peace in which even the possibility of war is extremely remote. For a variety of reasons, the minimal danger of death and destruction foremost among them, stable peace is clearly preferable to a precarious peace maintained solely by deterrence or a balance of power. Nevertheless, with a few notable exceptions, little effort has been devoted to identifying the origins of stable peace or the ways in which it might be created and nurtured. *Stable Peace among Nations* seeks to remedy this situation. The editors and contributors have as their central concerns (1) to define stable peace, differentiating it from other forms of the phenomenon, (2) to explain how and why relations of stable peace develop among states, and (3) to identify the factors critical to maintaining stable peace once it emerges.

In a superb opening chapter, "Stable Peace: A Conceptual Framework," Kacowicz and Bar-Siman-Tov set the stage for the work. They define stable peace as having both a cognitive and a temporal dimension. Cognitively, it exists when war is unthinkable as a means of resolving interstate conflict. Temporally, it persists over time. The authors distinguish between the stabilization and consolidation of peace. Stabilization refers to the process by which stable peace emerges after conflict has been reduced and eventually resolved. It implies "strategic" or "complex" learning in which the parties redefine their national interests and develop a deepening commitment to one another and the new character of their relationship. Four conditions are necessary for stabilization: (1) stable political regimes, which help promote mutual trust and confidence; (2) mutual satisfaction with the terms of the peace agreement and/or the status quo; (3) predictability of behavior and problem-solving mechanisms; and (4) open channels of communication, trust, and respect between the leaders. In addition, two factors are favorable to stabilization: third-party guarantees and spillover effects and the provision of nonmilitary public goods. With respect to the consolidation of peace, two conditions are necessary: (1) a continuation of satisfaction with the status quo and (2) a common normative framework, which may be enhanced by shared characteristics, including aspects of culture and identity.

The rest of the book is divided into two parts. The first consists of a series of theoretical and conceptual chapters, most of which are rich with historical illustrations. These essays consider such subjects as the domestic, regional, and international sources of stable peace; the role of common enemies and common identity; the development of trust; the treatment of issues; the economic aspects of making stable peace; and the relationship between stable peace and democratic peace. The second part contains case studies that seek not only to explain the formation (or nonformation) of stable peace in some instances but also to assess prospects for the future. Among the cases examined are Scandinavia, the southern cone of South America, Egypt and Israel, Europe and the future of NATO, and German-Polish relations.

Stable Peace among Nations is impressive in many respects.

It is unusual to see such sophisticated conceptual and theoretical work in combination with detailed case studies. It is perhaps even rarer to find an edited volume in which the editors and contributors appear to have worked so closely together that the overarching theoretical and conceptual apparatus, on the one hand, and the individual chapters, on the other, fit almost seamlessly. The result is a study with a breadth unattainable by a single author but with a coherence and degree of integration hardly ever achieved in a collection of essays.

The book is also commendable for its paradigmatic eclecticism. It draws heavily on ideational perspectives, especially constructivism. Constructivism holds that subjective and intersubjective understandings, more than material or objective forces, shape relations among states. Because stable peace is in essence a state of mind, a cognitive phenomenon, it is a particularly appropriate subject for constructivist analysis. Many of the chapters, including those by John Owen and Magnus Ericson, suggest that a constructivist approach can be quite helpful and perhaps even crucial in explaining the emergence and perpetuation of stable peace. In their fine concluding essay, Elgström and Jerneck, who describe themselves as “moderate constructivists,” explicitly recognize the causal importance of international power structures, domestic political configurations, and other less ideational factors. Although neorealism does not figure prominently in the book—since it is skeptical that a peace of sufficient depth and duration to be described as “stable” could ever exist—certain chapters, particularly the one by Benjamin Miller, consider the international systemic origins of stable peace. Other essays examine such traditionally liberal or neoliberal variables as economic relations, international institutions, and regime type.

The study of stable peace overlaps with one of the most prominent research agendas in international relations of the past two decades: democratic peace theory. Lending support to the arguments of that theory, Kacowicz and Bar-Siman-Tov suggest that the presence of well-developed democratic regimes is sufficient, although not necessary, to maintain stable peace. But this book deemphasizes the earlier structural-constraint and normative explanations for democratic peace and focuses instead on the sense of identity shared by democratic (or liberal) states. One of the most interesting and potentially controversial claims is Owen’s contention that the formation of common identity crucial to the consolidation of stable peace requires an enemy, an “other.” The somewhat depressing but logical conclusion is that stable peace can never be universal. The best one can do is create islands of stable peace and attempt to minimize conflict among them.

If this is true, the democratized world advocated by many democratic peace theorists might not be so pacific after all. Owen’s contention is also hard to reconcile with the generally optimistic thrust of constructivism and particularly the assertion of some constructivists that we could escape the pernicious effects of the anarchic Westphalian system if only we would think differently about it. It is, however, despite its origins in an ideational perspective, consistent with the neorealist belief that peace among some states is little more than a byproduct—often a fleeting one—of competition and conflict between those states and others. The compatibility of neorealist and constructivist analysis, in this limited respect at least, is just one of the book’s many insights.

Stable Peace among Nations is an excellent work, the most theoretically developed and comprehensive study yet produced on the important but too rarely examined phenomenon of stable peace. It will doubtless remain so for some time.

A Changing United Nations: Multilateral Evolution and the Quest for Global Governance. By W. Andy Knight. New York: Palgrave, 2000. 257p. \$65.00.

J. Martin Rochester, *University of Missouri-St. Louis*

Andy Knight examines the prospects for United Nations reform in the post-Cold War era, offering both historical context as well as speculation about how conducive the contemporary international system is to building a next-generation global organization in the foreseeable future. This useful study, which seeks to blend a theoretical discussion with prescription, starts with a survey of the literature and competing perspectives on UN reform as well as a conceptual treatment of institutional change. It then traces the evolution of the UN, how it has responded to changes in its environment, and the many failed efforts at UN reform. Finally, Knight suggests a new governance model as the basis for a rejuvenated UN.

The author “tries to understand the UN’s place in the broader history of multilateral evolution, how the UN system has changed, and why” (p. 11). Although he nicely captures the “institutional foundations” in 1945 (p. 65) and summarizes the key developments since, it is hard to improve on the work of many who have traveled this path, notably Inis Claude (*Swords into Plowshares*, 4th ed., 1984, and “The Record of International Organizations in the Twentieth Century,” Tamkang Chair Lecture Series, 1986). The most original aspect of the book is the conceptual treatment of organizational change processes and how these relate to the problem of global governance, that is, the analysis of the pressures being felt by the UN, as a sovereignty-based interstate organization, to adapt to a world being buffeted by both centrifugal (subnational) and centripetal (transnational) forces that are eroding sovereignty.

There is something of a disconnect between the purported empirical thrust of the book and the normative agenda that seems to color much of the analysis. On the one hand, Knight states that “the broad purpose of this book . . . is to describe, analyze, understand, and explain the phenomenon of multilateral evolution as a concept and a practice” (pp. 10–1). On the other hand, much of the discussion is hortatory and reflects antiestablishment, antistatist biases, even though Knight accuses others (American unilateralists and other anti-UN types) of wearing “ideological blinkers” (p. 28). For example: “Future institutions of global governance must become more proactive, addressing underlying reasons for global and domestic conflicts” (p. 9). “Unless the UN can reconstitute itself so that ‘we the people’ can be brought back into the picture, there is a good chance that some of the emerging multilateral arrangements may be designed in a way that will bypass this organization” (p. 9). “With the end of the Cold War political space was created for the emergence of a human security agenda reflecting the aspirations of social forces, human groups, and individuals across the globe” (p. 169).

It is hard to dispute Knight’s statement that, in light of globalization in the economic sphere, new kinds of violence threatening in the security sphere, and numerous other shifts in the landscape of world politics, “there is a need to update the UN machinery to accommodate changes occurring in the broader international society and system” (p. 20). But even if there is a compelling logic that dictates a reformation in international governance arrangements, will, in the words of Stanley Hoffmann (*Primacy or World Order*, 1978, 193), “the need forge a way?” Knight’s answer is somewhat confusing. Although “a new consensus on the place of the UN in international politics seems to be developing” (p. 129), “the role of the UN in any new global arrangement is . . . very much unclear” (p. 150).

Studies of international organization in general and the

UN in particular always struggle to avoid the twin traps of bad realism and bad idealism, of succumbing to either excessive cynicism or excessive wishful thinking. Knight tries to get around this problem by framing his work as an exercise in critical theory. He spends considerable time discussing the agent-structure issue and how international institutions like the UN are creatures of habit and practice yet are able to break out of routines and undergo change. He acknowledges the role of power and interests—"multilateral institutions reflect the underlying configuration of power at a particular critical juncture" (p. 184)—but is strangely silent on whether the United States or any other key state actors would support the governance model he proposes as a basis for UN reform.

After a survey of various approaches to UN reform, ranging from those of "status quo advocates," "incremental process reformists," and "adaptive reformists" to more radical views ("dissolutionists" and "successionists"), Knight boils down the modes of changing international organizations to three: reform, adaptation, and learning (p. 40). He argues that neither reform (explicit, planned, formal restructuring) nor adaptation (informal, reflexive, nonpurposive organizational responses to changing circumstances) have worked to make the UN a more effective multilateral institution. He calls for learning as the preferred mode of change and offers a "subsidiarity" model as the best chance for "rethinking" how the UN should function. Similar to the model adopted by the European Union, subsidiarity amounts to "sharing the labour of governance with central, regional, and local bodies" (p. 173). The book is surprisingly sparing in its elaboration of what this would entail, and only a few pages at the end (pp. 170–8) describe the model. Indeed, at a time when Knight admits economic issues threaten to overtake traditional security issues as a paramount global concern, the only real recommendation he puts forth is in the war/peace area: a greater role for regional organizations, such as the Organization of African States and the Organization of African Unity, and, as conflict managers. Not only is this an underwhelming recommendation after almost 200 pages, but also, as the author recognizes, regional organizations have not had much success in the security field over the years.

A central twofold question of our time is: (1) How much government/governance do more than six billion human beings want in their lives, that is, what is the proper relationship between state and society, and (2) to the extent it is understood that some measure of government/governance is needed, what is the optimal level for maximizing benefits in a given issue area, that is, global, regional, national, or local? Knight's book, at times very insightful and eloquent, makes a modest contribution in helping us confront this puzzle. One only wishes he had carried the analysis farther.

The International Law Commission of the United Nations.

By Jeffrey S. Morton. Columbia: University of South Carolina Press, 2000. 225p. \$29.95.

William Felice, *Eckerd College*

Before the eyes of the world, between 600,000 and 800,000 Tutsi were murdered, many hacked to death in front of their families. Rather than confront Hutu power and challenge those responsible for this attempted genocide, the UN pulled out of Rwanda. President Clinton later apologized for the inaction and passivity of the United States. U.S. indifference toward the Rwandan holocaust endures as the Clinton administration's paramount foreign policy failure. After World War II the international community pledged that genocide would never again be tolerated, but in Rwanda it was. Can victimized peoples ever put their trust in the UN and international law? Is it possible both to establish internation-

ally binding codes of conduct that criminalize genocidal practices and to enforce these codes?

Jeffrey Morton helps answer those questions through his exploration of the work of the International Law Commission (ILC) of the UN in two specific endeavors: the establishment of a binding code of international crimes and the creation of a standing international court. His descriptive and empirical study focuses on the evolution of the term "international crimes" and the ILC debates that resulted in the *Draft Code of Crimes against the Peace and Security of Mankind* and the *Draft Statute of the International Criminal Court*. (These documents are included in the book's appendix.) Since the end of the Cold War, the ILC has devoted most of its time to these issues of international criminal law. Morton's book provides a valuable overview of the significant accomplishments of the ILC in these critical areas.

Students of international law will find the first four descriptive chapters on the history and work of the ILC particularly useful. Morton begins with a well-written summary of the history and organization of the ILC and proceeds with clarifying analysis of ILC work on international crimes, the draft code of crimes, and the international criminal court. He adeptly summarizes the breadth of international crimes, including international terrorism, drug trafficking, torture, colonial domination, genocide, apartheid, war crimes, crimes against humanity, aggression, aircraft hijacking, and piracy. His history of the struggle to codify a draft code of crimes against the peace and security of mankind dramatically illustrates the unprecedented challenge the ILC confronted. The clear summary of the legal sources used and the principles enshrined in the draft code by the ILC is of particular value to scholars of international law.

Morton's chapter on the international criminal court provides a concise review of attempts to establish the court, culminating in the Rome conference in June 1998, and a review of the arguments for and against the creation of a permanent body. Morton focuses on the role of the ILC in drafting the statute for the court. Much progress has been made by the ILC in codifying legal principles into binding rules. The end of the Cold War opened a political door for the creation of an international criminal court, and the ILC moved with dispatch to draft a workable statute.

In the final two chapters, Morton presents an empirical analysis of the ILC. He constructs a database through content analysis of the recorded debate among ILC members on the wording, substance, and scope of the respective draft articles. Commission members' positions are treated "as observations that, when coded, generate data regarding the degree of consensus within the commission on draft articles" (p. 79). Numerical values are assigned to statements, ranging from 1 (minimal consensus) to 5 (maximum consensus). From these data, Morton seeks answers two questions: "(1) Do commission members function independent of or as extensions of their home governments? and (2) What impact, if any, did the end of the cold war have on the International Law Commission" (p. 102)?

On the first question, Morton concludes, "the empirical results are clear and conclusive: the political arena in which nation-states compete for the attainment of their vital interests has penetrated the International Law Commission to such extent that commission debate is a microcosm of world politics. The existence of a cohesive East European bloc of commission members, conflict between commission members from Eastern and Western blocs, a North-South divide, and Southern cooperation in commission debate indicate in the strongest of terms that, indeed, the International Law Commission is a reflection of the political, ideological, and economic struggles of the international arena" (pp. 102–3).

In regard to the second question, the data reveal that the end of the Cold War greatly affected the ILC. "The conflic-

tual relationship of East and West ended, and Southern cooperation experienced a significant decline” (p. 105). The second set of findings thus reinforces the first, namely, “that the Law Commission reflects the international arena in which it functions” (p. 105).

Morton’s study takes a view of international law shared by classical political realists and classical Marxists: International law and international organization cannot be insulated from international politics, economics, and political power. In fact, most “idealists” and liberal internationalists would accept the impossibility of positing international law as totally independent of power politics. ILC members may strive to serve “in a personal capacity” independent of nation, but their inherent bias (due to their education and professional training) will most likely be toward their ideological worldview of their home state.

In the end, Morton’s conclusions suffer from problems often seen in behavioralist studies. Some behavioralists and numerous rational choice theorists tend to make exaggerated claims about the validity of their approach and the “science” behind their methods. Morton presents a strong defense of behavioralism as a scientific method of inquiry and argues that international law as a field has declined due to its separation from these methods (p. 75). He wants international law to adopt “systematic, statistical testing of propositions,” which would provide “a firm footing” and allow it “to enter into the mainstream” of international relations study (p. 76).

I find this argument unconvincing. I am reminded of the caution toward behavioralism expressed by Charles Lindblom in his presidential address to the American Political Science Association in 1981 “Another State of Mind: Presidential Address, 1981” (*American Political Science Review* 76 [March 1982]: 9–21). Lindblom is skeptical about political science’s pretensions to be a science. To qualify as a “science” means that our procedures would give us “discoveries,” that is, new or nontrivial insights about the political sphere. Many statistical studies end up confirming the obvious. Scholars of international law have long noted the effect of power politics and the international system on the creation and implementation of legal codes and rules between states. To these legal scholars, Morton’s statistical analysis may simply be confirming the obvious.

Traditions of War. Occupation Resistance and the Law. By Karma Nabulsi. Oxford: Oxford University Press, 2000. 293p. \$72.00.

Michael Dillon, *University of Lancaster*

This book is a work of considerable scholarship that began life as a doctoral thesis under Adam Roberts at Oxford University. More than three pages of acknowledgments bear witness to its pedigree. They also disclose an intriguing combination of English traditionalists and non-English revolutionaries who sustained the author in the transformation of a research thesis into an elegantly produced book, which is a classic in the style of the English School of international relations. Clearly, there is yet another story to be written, however, about the curious provenance of such a work. It may very well be a very much more interesting and intriguing story than the one the book pursues, although this itself is not without interest and importance.

Traditions of War pursues a number of complex and related themes and excavates many historical archives in exploration of its core problematic: the failure of the laws of war to resolve the problem of distinguishing between lawful and unlawful combatants. Traditional approaches—those of the central paradigms of international relations theory and the

key concerns of international law—ordinarily supply the analytical optics employed in these kinds of investigations. This book is refreshing because it advances three philosophical traditions of war instead: the martial, the Grotian, and the republican (indebted to Rousseau). In applying these to the rich confusion of military occupation and resistance experienced in Europe over the last 250 years, the author exposes the many historical as well as conceptual limitations and fallibilities of international realism and idealism.

War for this author is not an aberration that has to be explained, excoriated, and outlawed. Neither is war simply a legitimate instrument in the realist pursuit of stylized political ambitions. The intimate correlation between forms of war and forms of life is not a point *for* which the text argues, but one of the points *from* which it argues. More interestingly, what distinguishes this account of the three traditions of war upon which it draws is the recognition that forms of war and violence are themselves integral also to specific philosophical positions and political ideals concerned to champion varying interpretations of the good life. No idea is innocent of violence. Every idea has a violence that is essential to its realization. The legality or illegality of violence is a function of the order that different traditions commend. Thus, liberal internationalism in its high legal pomp is shocked to be reminded that liberal peace is always intimately correlated also with liberal ways of war.

Because this is a classic in the English School of international relations, the wealth of its historical scholarship casts much diversionary light on all sorts of issues and themes. Of particular interest to me is the account given of the martial character of Britain’s imperial experience. Anyone familiar with Britain’s postimperial foreign and defense policy cannot but be struck by the continuing force of that tradition at home and its deep influence throughout Britain’s geostrategic intimacy with the United States abroad. Alternatively, for those interested in the republican tradition, there is a corresponding wealth of detail on obscure European republican activists and theoreticians. The overall worth of the book nonetheless depends upon its core analytical method, the idea of a tradition, and the messages conveyed by that analytical medium. Where the English School is concerned, the medium is certainly also the message, politically as well as analytically. The advantage of tradition is that it directs attention to the complex historicity of ideas and experience, but the disadvantage is that it elevates continuity over disjunction. Moreover, as employed in this school, tradition elides a host of other relevant issues. Two seem to me to be critical.

First, we should ask about these three traditions of war. Whose traditions were they? What archives have been ignored in the constitution of this narrowly tripartite account of the traditions of war? The focus is on the laws of war and their failure satisfactorily to distinguish between lawful and unlawful violence, but there is, for example, only a very modest postcolonial sensibility at work here, and the Marxist tradition simply does not figure at all. It is as if there were no Marxist tradition, as if the Soviet Union, together with its effect for good or ill upon the trajectory of international affairs and the character of the international society upon which the English School places such store, had never existed.

Second, tradition here implies continuity and intelligibility. This book’s very account of tradition is one in which tradition lies in the seamless historical transmission and reception of texts rather than in the violent destructions and deconstructions that effect breaks, discontinuities, and dynamic incoherences in our meaning (and legal) systems. In short, the idea of tradition in the English School is a deeply conservative one that ignores much of what it claims to be able to account for. Here, in addition to the demotion of Marxist and postcolo-

nial challenges to the laws of war, the most important lacuna is the radical absence of the massive technological as well as sociological revolutions that have transformed the face of war as much as they have the very societies, locally and globally, that make war. Changes induced by the massification of war in the nineteenth and early twentieth century were matched by those induced by the nuclearization of war after 1945.

Currently, we are undergoing yet another revolution in military affairs. Driven by computer-mediated means of communication and information operating through dense systems of global and local surveillance, and premised upon the new ontology of information, this revolution presages dramatic changes in the conduct of war whose implications for the correlation of legal-illegal violence may be no less significant than those forced into international prominence by industrialization and nuclearization. The concept of a tradition of war is capable of addressing these questions but only if the concept of tradition itself is reworked in ways that do not confine it to the provenance from which this thesis took its inspiration. Ultimately, it is less the book than its pedigree whose limits are shown to be so interestingly if desperately narrow here.

Rethinking European Order: West European Responses, 1989–1997. Edited by Robin Niblett and William Wallace. New York: Palgrave, 2001. 298p. \$75.00.

Stuart Croft, *University of Birmingham*

The volume is organized around three central hypotheses. The first is that, faced with significant change in the way in which policymakers understood the world after 1989, European states—both large and small—reacted in a confused and ad hoc fashion. The leaders of major international organizations such as the European Union and NATO tend to present the last decade in terms of a coherent strategy and the evolution of policy. Niblett and Wallace, in contrast, suggest that overload and difference are more appropriate prisms.

Second, policy and academic study became separated in the early 1990s. Faced with the failure to predict the end of the Cold War, American academia focused on broad visions of the end of history, or the clash of civilizations. European (and especially British) academia concerned itself with epistemological debate. In this, “real” policy issues—institutional enlargement, relations with Russia—were sidelined. Niblett and Wallace hypothesize that academia focused on policy can make a real contribution to coherence in the development of state and intergovernmental organization strategy.

Third, the editors hypothesize that the influence of the United States, both in policy terms and in setting intellectual trends in academia, dominated the creation and development of specifically European conceptions and responses to the changing perception of the world. The subtitle of the volume is illustrative in this regard.

This book is not concerned with theoretical development or the application of theoretical insight to particular case studies. Rather, it is an attempt to understand West European policymaking in a crucial period. Two chapters establish a framework. William Wallace is concerned with outlining a notion of regional order and the effect of ideas upon such notions. He identifies a paradox, a fundamental transformation of the continent’s security order: Western Europe has continued to focus on its own parochial concerns at a time when the eastern part of the continent has undergone significant political, economic, and social change. Anthony Forster and Robin Niblett take on this theme by tracing

different conceptions of Europe as they developed through the 1990s.

Successive chapters cover national policy in Germany, France, and Britain, of course, and then Dutch, Swedish, Spanish, and Italian developments. These are collectively of a high value and establish with care the empirical detail of policy debates, initiatives, and responses in the period under review. Hartmut Mayer suggests that the German debate was shaped by the need to be intellectually “early on the beach” (a European, or certainly a British, image of German tourist behavior at European holiday resorts), an attempt to “lead without anyone realizing it” (as politician Karl Lammers put it, see pp. 80–1). Niblett’s analysis of French policy is framed by the argument that France was a beneficiary of Cold War structures. Yet, the failure of Mitterrand’s promotion of a nationalist “*autre politique*” in 1981–83 convinced policymakers of the centrality of building a European pole of power with Germany without allowing deep infringements on French sovereignty. The power of this contradictory conventional wisdom lies unchallenged among the elite, for fear of provoking a public reaction similar to that which led to the referendum rejection of the Maastricht Treaty. Forster and Wallace argue that the end of the Cold War provoked an emotional debate in the United Kingdom, which focused on the future of Germany but actually represented an articulation of concerns over the nature of British identity.

For Steven Everts, the 1990s represented a period when the Netherlands adjusted to a more continental and less Atlanticist orientation—as he puts it, “adjusting to Germany.” Lisbeth Aggestam plots the reconstruction of Swedish neutrality. Niblett explains the consistency in Spanish European policy since the return of democracy, and Christopher Hill and Filippo Andreatta explain the challenges both to Italy’s confidence as a foreign policy actor and reliance on an intellectual construction of European integration as the core of its external identity. This picture of incremental adjustment, perhaps of incoherent response, is mapped further in Wallace’s concluding chapter.

The volume provides a wealth of empirical detail about the nature of European policy during the early and middle 1990s. It does not simply focus on the major powers in the European context but gives a more rounded view of the nature of the West European discourse at this crucial historical juncture. Three sets of questions emerge from the analysis.

First, what is to be said about the relative influence of different states in the process of rethinking European order? We take it as a core assumption that states—or discourses within states—are key ordering priorities for the authors. But what is the relative difference between France (or French discourse) and, say, Sweden (or Swedish discourse)? What are the mechanisms of influence, and how does one assess which is more active in a particular policy area? There is a sense from the structure of the volume that each is equally important; indeed, each national debate has very specific characteristics that make it different from others. All of that may be persuasive, but it does not help the analyst understand how West European responses, writ large, are arrived at. Following from this, there are silences that are not explained. Why a Dutch chapter, not a Portuguese? Why Sweden and not Austria? And so on.

Second, what is “Western Europe” in this case? It is often a label that means members of the European Union, particularly as many of the country analyses argue persuasively that foreign policy is constructed through conceptions of European integration. But what, for the analysis of this volume, is the effect of the EU institutions? The European Commission, for example, is only mentioned in passing, but it has key

responsibilities in defining European order, not least in the process of enlargement.

Third, we do not learn too much on the “why” question. Why did Western Europe respond in the ways that are so carefully outlined? Why were there no European blueprints? On this question, why did Western Europe not interact in different ways with the United States (and, for that matter, with Russia)? This is a strong volume that describes what was going on, but within certain very specific parameters.

The Asian Financial Crisis and the Architecture of Global Finance. Edited by Gregory Noble and John Ravenhill. New York: Cambridge University Press, 2000. 310p. \$64.95 cloth, \$22.95 paper.

Ethan B. Kapstein, *Insead Business School*

The problem of how social organizations achieve stability has occupied political scientists since the very beginning of the discipline. For this reason, much literature is devoted to analyzing consequential episodes of instability—wars, revolutions, and political and economic crises. In the subfield of international relations, the search for stability brings with it particular theoretical and empirical challenges, given the absence of centralized, authoritative centers for conflict management.

This edited volume is a noteworthy addition to the study of instability in the global financial system and the domestic and international efforts by governments to shore up that system. Written on the heels of the Asian financial crisis, the text has an immediacy that is refreshing, but sacrifices little in terms of analytical rigor. The editors and contributors are to be commended for their ability to look at a recent crisis with some sense of detachment and produce a work of real value to scholars of international political economy.

During the Asian financial crisis of 1997–98, the International Monetary Fund (IMF) and other international organizations generated a wave of studies on the weaknesses in regional financial markets and why these led to a flight to safety by creditors. In the aftermath, U.S. Treasury Secretary Robert Rubin and other public officials proclaimed the necessity to reform and strengthen the global financial architecture, in particular the need for greater transparency by debtor countries, stronger national regulatory regimes, and closer harmonization of those regimes at the international level. Scholars and pundits joined in these debates about causes, consequences, and reforms, but the emphasis was overwhelmingly on economic and technocratic issues associated with the governance of financial markets.

Missing from all this discourse was much in the way of political analysis, and it is this gap that the Noble-Ravenhill volume ably fills. The editors and authors examine the political institutions that govern financial flows at the national and international levels as well as how these institutions channel interest group preferences and constrain the political choices available to leaders. The underlying argument is that “politics matter” because decisions about such policies as a nation’s relative openness to international capital flows, patterns of credit allocation, and banking regulations are made in a heavily political context.

As an example, consider the countries among those most affected by the crisis, Korea and Thailand. Stephan Haggard and Andrew MacIntyre argue (p. 57) that “institutional arrangements in the two countries rendered them vulnerable to problems of policy incoherence.” Thailand’s flawed parliamentary system and Korea’s weak presidential system made it difficult to respond decisively when the crisis hit. In

the discussion of Taiwan, in contrast, Noble and Ravenhill ascribe that country’s ability to weather the storm with minimal damage to a political history in which a dominant party created strong economic institutions and policies, which enabled the country to sacrifice “growth to stability” (p. 102).

At the international level, as Miles Kahler illustrates in his chapter, proposals for a “new global financial architecture” were also made within the context of governance structures that could not be easily changed; any reforms would only be made on the margins, at best. Although there were some prominent calls for radical reform of institutions, notably for dismantling the IMF by the troika of George Schultz, Walter Wriston, and William Simon, these ultimately carried little weight with public officials around the world. Instead, most of the proposed changes to the IMF were relatively minor in import, and some of them, such as the renewed focus on crisis prevention, may only lift that organization’s importance and profile in global financial affairs over time. Indeed, to the extent that there has been serious discussion of financial reform by public officials and international bureaucrats, it has focused on domestic rather than multilateral regulatory institutions.

The Asian financial crisis forced officials and scholars to ask once again why it is so difficult to design a stable global financial system. It seems that the world has experienced an endless series of financial crises since the collapse of Bretton Woods in 1971, each time creating global recession and in particular countries a prolonged period of negative growth, high unemployment, and rising poverty. This book emphasizes the domestic political conditions that make financial instability in certain countries more or less likely as well as the constraints on the international system that make it incapable of doing much more than the current set of institutions and agreements already offer. Overall, it appears that we are tinkering on the margins, and it is reasonable to assume that more financial crises lie ahead. This book will provide useful guidance for assessments of the next crisis, and in the meantime it will prove valuable to every student of the global economy.

Honor, Symbols, and War. By Barry O’Neill. Ann Arbor: University of Michigan Press, 1999. 344p. \$49.50 cloth, \$19.95 paper.

Scott Gates, *Michigan State University and PRIO*

Symbolism is evident in the handshake between the leaders of two countries, the carefully chosen words of an official apology (or nonapology), the yellow ribbons of the Gulf War, and the rise and fall of the Berlin Wall. Indeed, international relations are rife with symbolism. Yet, despite their ubiquity, symbols have largely been dismissed as peripheral aspects of real international politics, not worthy of serious theory building. O’Neill demonstrates that they not only deserve a central role in theories of international relations but also are worthy of rigorous analysis.

This delightful book weaves together hundreds of historical examples from international relations with such ancient poems and tales as *Sir Gawain and the Green Knight* and *The Feast of Briciu*, as well as game theoretic analyses of symbols, honor, and social face. Rather than focus on the power politics of national interests, military capabilities, and alliances, or on different political and economic systems as explanations for the causes of war, O’Neill concentrates on the role of symbols. The central argument is that states still fight over issues of honor. Status regulators, which include honor, social face, prestige, and moral authority, rely on

symbolism and, in turn, play a profound role in defining international relations. Game theoretic analysis is used to demonstrate in a rigorous manner how symbols and status regulators affect the choices made by foreign policy decision makers. O'Neill provides an analytically precise and theoretically rich examination of symbolism and its roles in international relations.

Three categories of symbols are identified: message, focal, and value symbols. In May 1994, Nelson Mandela invited his white jailer to his inauguration as a gesture of "hope and reconciliation" (p. 6). This is an example of a message symbol. Focal symbols stem from events that induce observers to expect a certain outcome in the game being played. The symbolism of an arms agreement, for example, may be more important than its actual content. A flag is a value symbol; it elicits strong attitudes but also exhibits a multiplicity of meanings. Symbols, in turn, play a role in determining the nature of challenges to honor, commitments based on honor, insults as assaults on face, commitments based on face, apologies, prestige, norms, and moral authority. All of these play a role in international relations.

Are wars caused by insults and challenges to honor? O'Neill answers affirmatively and cites a number of examples to support his claim. He is careful in his distinction between challenges to honor and insults as assaults on face. A challenge to honor originates from a party of equal rank, and codes of honor dictate the nature of action taken in response. Face centers on hierarchy. Insults attack the recipient's position in a society and require common beliefs as to what has transpired. O'Neill develops a convincing argument as to how the symbolism that affects honor and face can escalate hostilities that can lead to war. I do not believe other explanations for the cause of war should be disregarded, but it is clear that symbols, honor, and social face play a part.

From a game-theoretic perspective, war—like any other outcome—results from actors' choices, information, and payoffs. Most applications of game theory to the study of war and conflict take a realist perspective, which features national interests and material resources. By focusing on the interaction of beliefs and choices regarding subjective assessments of probability and beliefs about other players' beliefs, especially as they relate to symbols, honor, social face, and challenges, O'Neill incorporates elements of the constructivist perspective into his analysis. Discourse and perception become the featured aspects of international interactions. In this way, this book integrates aspects of realist and constructivist thought through the analytical tool of game theory.

Despite the many strengths of this excellent book, there are a few shortcomings. The theory would have been strengthened by greater focus on the game theory employed for analysis. Glimpses of profound insight lie buried in footnotes or in stray passages but never receive the full attention they deserve. Indeed, the discussion of game theory and intangibles is relegated to an appendix. This discussion, especially with an elaboration of the role of common knowledge in communicative acts and the interpretation of symbols, should have appeared early in the main text to demonstrate the utility of the game-theoretic approach in this context.

Another shortcoming is the lack of focus on rhetoric. O'Neill's analysis of symbolic communications between nations is limited to direct interactions. Honor and social face are viewed as direct causes of war, but the use of symbolism and rhetoric to persuade a domestic audience and justify bellicose actions is not examined. Indeed, O'Neill dismisses that perspective and is intent on demonstrating that honor plays a direct role in causing war. Public references to

"honor," "national will," "resolve," "credibility," "reputation," and "image of weakness" are not intended for foreign consumption alone. In 1990–91, when President Bush insulted Saddam Hussain before the Gulf War, his intended audience was not only the Iraqi but also the American public. Rhetoric, and specifically the symbolism that reflects honor and social face, plays an important role in rallying the general public in support of the actions taken by political leaders.

This is an important and provocative book. Combining a rich set of real-world examples with rigorous game-theoretic analysis, O'Neill develops a convincing argument for taking symbolism into account when studying international relations. His work justly deserves the 54th Annual Woodrow Wilson Foundation Book Award for the best book published on government, politics, or international relations.

Marketing the American Creed Abroad: Diasporas in the U.S. and Their Homelands. By Yossi Shain. New York: Cambridge University Press, 1999. 294p. \$54.95 cloth, 17.95 paper.

Rodolfo O. de la Garza, *Columbia University*

What is the national interest, and who defines it? These questions have become inextricably linked to the debate over how the dramatic increase in Asian, African, and Latin American immigration have affected national identity. These demographic and political changes, combined with globalization, transnationalism, and the increased demand by the leadership of the new ethnics to participate in foreign policymaking, have prompted scholars such as Samuel Huntington and Arthur Schlesinger, Jr., to warn against ethnic leaders who pursue narrowly defined country-of-origin interests that undermine the national interest.

Yossi Shain's objective goes beyond challenging such claims to arguing that the increased involvement of ethnic leaders in foreign policy actually enhances the national interest. They not only support American core values, such as democracy and individual freedoms, but also strongly advocate that country-of-origin governments adopt core American values as the price of improving relations with the United States and advancing economically. Because of their connections to the home country, Shain argues, ethnic leaders are ideally situated to market the American creed.

This work is of broad scope. To support his claim that ethnics abandon their home-country allegiances when these conflict with "American" interests, Shain reviews the historical examples of the Germans, Irish, Japanese, and others. To address the current debate regarding the effect of immigrants, he focuses on Arabs and Mexicans but also addresses the experiences of many other groups, such as Czechs, Slovaks, Koreans, and Cubans. His analysis emphasizes their concern to be recognized as legitimate participants in American society. Absent such status they will experience social, political, and economic discrimination. Moreover, they will be incapable of influencing U.S. policy toward their country of origin, and they will suffer if relations between the United States and their homeland deteriorate or become openly hostile, as U.S.-Iran and U.S.-Iraq relations illustrate.

The volume's principal value is that it adds substantial new material to the well-known arguments developed by Louis Gerson, Alexander DeConde, and, most significantly, Lawrence Fuchs. The latter's influence is especially visible in Shain's argument regarding the efforts by ethnics to establish their American credentials so that, if they engage in foreign policy, they do so as patriots rather than as citizens with divided loyalties. Shain brings that argument up to date by

locating it within the context of a new international environment. Specifically, he considers how states such as Mexico, which were historically antagonistic toward emigrants, now seek closer ties with emigrants in the hope that they will in turn help influence U.S. policies that are in line with home country interests.

The study would have been greatly enhanced if the theoretical foundations had been made explicit and the argument had been developed more systematically. Each of Shain's cases uses some combination of variables—such as diaspora demographics, immigration history, home country perceptions of the diaspora, home country regime type and international status, and characteristics of home country relations with the United States—to explain how each example relates to Shain's core proposition. Yet, the author does not systematically weigh the relevance of each of these in his numerous case studies, and he does not rank order them in terms of their overall relevance to the general issue of diasporic foreign policy involvement. The result is a volume rich in detail but short on theory, which leaves unclear the significance of the case studies.

The lack of theory makes it impossible to evaluate claims that all diasporas support core American values and promote them in their dealing with homeland governments. Such assertions implicitly challenge the utility of other literatures on diasporic foreign policy involvement, such as those on transnationalism, domestic sources of foreign policy, and socioeconomic assimilation. Shain seems to be calling for a new approach, but he never explicitly delineates it.

Because the case studies identify multiple factors that affect a diaspora's foreign policy involvement, Shain's assertion that all groups ultimately promote American democratic values in the home country is both empirically questionable and theoretically puzzling. Empirically, his own examples counter his claim. For example, Mexican American (as contrasted with Mexican immigrant) leaders have never publicly promoted democratic reform in Mexico. Similarly, his description of the attitudes of mainstream Arab Americans as compared to rightist and leftist Muslims toward the United States and its Middle East policy indicates that many leaders in this diaspora neither embrace core American values nor promote them in the homeland. Shain also does not document that promoting democracy and related values is vital to the Chinese diaspora.

Theoretically, the argument suggests that none of the variables considered in the case studies have any ultimate effect on diasporic foreign policy activities, which is a dubious assertion. This may be most obvious with regard to the effects of time. How long must immigrants be settled before they act as Shain describes? Is it years or generations? Do the effects of time vary as a function of the characteristics of the home country regime? Of the social capital with which immigrants arrive? With how they are received? The three groups cited above differ in terms of history, demographics, ideology, and home country regime type, and they all act counter to Shain's assertion regarding the promotion of democratic practices in the homeland. What explains this? In addition, Shain argues that all groups must claim they are acting in the national interest when they engage foreign policy. Does this suggest that often such claims are made only for strategic reasons? These are some of the issues that the study fails to address because of its atheoretical approach.

With the possible exception of the Muslim case, Shain's examples (predictably) rebut the charges that the involvement of new immigrants in foreign policy undermines the national interest. He should have noted, however, that diasporas have a right to participate in public debates on the

national interest, and, depending on a variety of factors, may support significant changes in American foreign policy. In other words, Shain should have acknowledged that some of these new actors may offer a different vision of the national interest, but he should have explicitly challenged the argument that such competing perspectives undermine the national interest by noting that all citizens have the right to challenge extant definitions of the national interest, either through public debates or congressional lobbying. Those who would deny that right to diasporas in effect seem to be calling for a return to the undemocratic decision-making processes of the past that are increasingly unacceptable to the American public.

In conclusion, this study offers a detailed description of diasporic involvement in foreign policy. It would have been more valuable if it also had been theoretically grounded.

Theory of the Global State: Globality as an Unfinished Revolution. By Martin Shaw. Cambridge: Cambridge University Press. 2000. 295p. \$59.95 cloth, \$21.95 paper.

William I. Robinson, *University of California, Santa Barbara*

At a time when globalization has thrown traditional social science paradigms into a quandary, one would expect international relations (IR) scholars to question the continued centrality of state-based explanations to worldwide social phenomena. Martin Shaw is acutely aware of the limitations of mainstream social science in the face of emergent globality. Curiously, however, he presents a study that attacks the problem of the limits to state centrism with a thesis that retains as the state as the center of analysis. Globalization has made clear the untenability of subsiding an array of social, economic, cultural, political, and ideological processes and forces under the statist conception of social life central to traditional IR approaches. Yet, this is exactly what Shaw does in his attempt to develop a theory of the global state.

To be sure, this is an important and innovative study. Shaw argues that we are undergoing a period of historic transformation such that the "global" needs a "core meaning" of its own (p. 5). The global should be understood through three major concepts: globality, global revolution, and the global state. With this triad Shaw wants to move beyond typical globalization theories that stress transformed conceptions of time and space in social relations. He finds the notion that globalization as process lacking and prefers to identify the phenomenon as a state or condition, hence globality. He concedes that transformation of time and space has recast social relations, but for him new social structures do not constitute the essence of globality; rather, it is the new social meaning these transformations involve, in particular "the development of a *common consciousness of human society on a world scale*" (pp. 11–2, emphasis in original). This constitutes the idealist core of his theory of the global state, in which social relations "become global when they are significantly and systematically informed by an awareness of the common framework of worldwide human society" (p. 12).

How should we conceive of the current period of worldwide transformation? Postmodern approaches see cultural change as causal, says Shaw, whereas post–Cold War approaches emphasize political and military transition, and theories of globalization underscore technical and economic change. These dimensions, he claims, are subordinate to the central driving dynamic of our time, the emergence of a global state. The global revolution is alternatively conceived by Shaw as the rise of this global state ("so that *the political structure of social relations on a world scale has fundamentally*

changed," p. 17, emphasis in original) and as the spread of democracy in global society (the "global democratic revolution"). Indeed, he suggests that the "democratic revolution" centered in the former Soviet bloc paved the way for the rise of a global state. "In contrast to those accounts of globality which plot the extending worldwide web of economic and cultural linkages, I focus on the traumatic political upheavals (the global democratic revolution)" at the center of globality as the causal factor in the transition to a "global state-system" (p. 145).

There is a circularity here that indicates the underlying problem of the lack of an identifiable historical determination in the globalist outcome. Economic causality is dismissed as "economism," and the causality of social forces is likewise dismissed as "sociologism." Instead, globality should be grasped, as primarily statist (p. 81), and explanations of the current epoch should focus on "the state core of contemporary globality" (p. 90). But how does Shaw reconcile the antinomy of his idealist definition of globality as consciousness with his material emphasis on the rise of a global state? What ultimately drives global state formation is the incomplete process whereby "global consciousness" demands "global state relations" (pp. 20–1). This becomes a circular argument, since in Shaw's formulation global state relations are said to have shaped global consciousness.

Shaw then proceeds to adopt Michael Mann's neo-Weberian definition of the state as a territorially demarcated, differentiated set of institutions and personnel with a center that exercises authoritative rulemaking backed up by force. In order to arrive at his conception of what exactly comprises the global state, Shaw adds the qualification that a particular power center must be "inclusive and constitutive of other forms of layers of state power" (p. 190). With this qualification, he can conceive of a global state as a higher layer of state power constituted by, but superseding, national states, such that the latter become components of a larger entity. This superior layer of state power is a "global Western state," territorially bound and comprised of the North Atlantic bloc plus Japan, Australia, and New Zealand. This configuration of core capitalist states constitutes a global state, he suggests, because it is organized into a military alliance and because its rulemaking backed up by force extends to the entire international system. Despite Shaw's formal rejection of realism, we seem to have here a neorealist theory of hegemonic stability account of the capitalist core as a new hegemon within an otherwise anarchic system of states and state blocs.

Indeed, Shaw identifies three types of states in the current global order. The global Western state is "an integrated authoritative organization of violence which includes a large number of both juridically defined states and international interstate organizations" and "functions as a single center of military state power in relation to other centers" (pp. 199–200). The "quasi-imperial nation-states" are exemplified by "numerous major non-Western states, across Asia, Latin America, Africa and the Middle East." The "new proto- and quasi-states" include autonomous provinces and districts in what was formerly the Soviet bloc or the contested and tenuous states of a number of African nations.

"The power relations between states of these three types are crucial to understanding world order" (p. 213). Through transformed relations within and among these different state blocs, "external relations between states are transformed into the internal relations of new state power . . . complex global concentrations of state power [. . .] have actually come into existence" in a form other than an "uncomplicated center of state power, a singular sovereign" (p. 92). This is a crucial insight into our understanding of transnational state forma-

tion, and it constitutes a major contribution of the study. But why is Shaw so concerned with placing the state at the center of globality? A global society, economy, and culture can be said to exist "to the extent that global patterns of relations have become inclusive and constitutive of these (social, economic, cultural) relations in general" (pp. 177–8). In turn, in Shaw's account, these social relations are seen as subsumed by (or at best, organized by and around) state structures. The state drives and organizes these relations, which for Shaw are derived from state forms.

I do not want to negate the importance of this work, especially since it explores uncharted terrain, but nevertheless it remains inconclusive, contradictory, and deeply troubling on several counts. First, Shaw eschews political economy as an instrument of analysis and is explicit in his hostility to the "theory of capitalism" (p. 42). Hence, entirely absent is any discussion of economic globalization, of capitalism as a global system, or of class groups, elites, and other social forces in the current period of transformation. World system theory is dismissed out of hand, and there is no engagement with the burgeoning literature on international political economy (IPE). This rejection of political economy is symptomatic of an underlying problem: There is no consistent theoretical approach or any systematic application of analytical precepts. Shaw's account of the global state is constructed through an eclectic admixture of neo-Weberian, realist, liberal, postmodern, and institutional analysis. His core concepts are never fully unpacked or operationalized and hence remain undertheorized.

Second, Shaw thoroughly reifies the state as a macrosocial agent *sui generis*. Despite his insistence that the current epoch cannot be conceived in terms of the old nation-state categories of social science, his study is ultimately—and thoroughly—a state-centered and realist account. Shaw describes the global as a "global *interstate* world" (p. 16, emphasis added), in which globality is reduced to a reorganization of the historical interstate system of discrete nation-states. The transition from the "bloc-state system" of the Cold War into an "incipient global state-system" is the key transformation of our epoch. Shaw develops the insightful notion that globality involves moving beyond the "ordering principle" of nationality-internationality as "a historically specific structure of state-society relationship" (p. 28), but it is not clear how his own account does in fact move us beyond this principle.

Third, the structure of Shaw's thesis is normative and ideological. The global Western state is equated with a "progressive" and benign version of Kautsky's "ultra imperialism," a heroic bloc championing democracy and civility worldwide (p. 161), in contrast to "major non-Western states," which are prone to intra- and interstate warfare, authoritarianism, lawlessness, and "ruthless discrimination against peripheral, especially indigenous, peoples" (p. 209). The Third World is largely ignored, aside from sweeping generalizations about Africa, Asia, and Latin America, where social change in the epoch of globality is reduced to a struggle between forces of "democratic change and violent reaction" (p. 168). The narrative is laced with strong doses of crude anticommunism. Adjectives such as "brutal," "genocidal," "illegitimate," "totalitarian," "stagnant," "bureaucratic," and "repressive"—to mention but a few—abound in Shaw's discussion of the former Soviet Union and detract from a more objective narrative.

Conspicuously absent is the historic role of Western colonialism and imperialism in contributing to current global structures. Since theories of global capitalism, the world-system, or IPE are dismissed, linkages between the non-

Western world and a larger global system dominated by a Western capitalist core play no causal role or are not accorded any determination or explanatory value in the condition of both the Western and the non-Western worlds. There is little recognition by Shaw of the deep and rising racial, gender, and class inequalities; escalating polarization between the global poor and the global rich; unprecedented levels of deprivation, marginality, injustice, and militarized borders; and the concentration of political power in emergent transnational elites that so perforate the brave new world of global capitalism. Shaw need not agree with this more critical view of emergent global society, but his one-sided enthusiasm is not convincing. In this regard, it is difficult to avoid concluding that this is a story of the West and the rest; a renovated colonial tale of Western superiority and the “peoples without history” that provides a comforting theoretical rationale for the emergent global capitalist order.

Limited Adversaries: Post-Cold War Sino-American Mutual Images. By Jianwei Wang. Oxford and New York: Oxford University Press, 2000. 305p. \$35.00.

John W. Garver, *Georgia Institute of Technology*

This interesting and solid study of Chinese and American mutual elite perceptions is based on 268 interviews conducted during 1990–92. Interviewees fell into three categories: academics largely involved in study of the other country, diplomats, and business people involved in commercial activities with the other country. The views of these elites were solicited via a standardized set of 66 questions probing the views of the other country’s people, society, culture, and international behavior. Respondents were also asked to describe how their views had changed since about 1970, and especially how they had been affected by the Beijing massacre of June 1989. Wang concludes that the effect of June 1989 was far less than is often asserted, based on the relatively low change in perceptions reported by his respondents. Wang is well aware of the methodological problems with such an approach—the tendency of people to conclude post hoc that “I saw it coming”—but he believes his findings are valid. He also acknowledges that elite perceptions, formed on the basis of greater information and personal interaction, may well differ from mass perceptions.

Wang finds that elite images on each side are not simple or unidimensional but fairly complex. Elites hold positive mirror images of the “people” of the other. The Americans tend to see Chinese as open, having a sense of humor, resourceful, and intelligent. The Chinese tend to see Americans as open, direct, honest, enterprising, and pragmatic. Both sides see the other as considerably more emotionally and intellectually accessible than the people of other nations—Russians, Japanese, or Europeans. Yet, there are negative views. The Americans simultaneously see Chinese as poor and politically impotent, and the Chinese see Americans as shallow, dissolute, and arrogant. Wang suggests that the perceived similarities between Chinese and Americans may make stable Sino-American cooperation difficult by covering up deep, underlying differences in actual national character. Initial establishment of rapport is easy because of perceived similarities, but disillusionment may set in once underlying differences surface.

Wang finds asymmetrical perceptions regarding “society” and “culture.” Chinese elites tend to view the U.S. system of government positively, stressing its political stability, efficiency, ability to incorporate a range of diverse views, and effective central-local relations. American elites, interviewed

not long after the events of June 1989, viewed the Chinese government with disgust. Chinese elites were repelled by intractable American social problems and seeming governmental indifference to social injustice, but they judged positively the American work ethic, sense of personal responsibility, and spirit of entrepreneurship. American elites expressed admiration for Chinese culture but made clear that this referred to China’s traditional culture, not to the culture of contemporary, communist-ruled, post-1949 China.

Regarding international behavior, Wang again finds mirror images: Elites in both countries tend to see the other as a normal power pursuing national interests. There was unanimity among Chinese elites regarding the preponderant nature of U.S. power, but different normative valuations were associated with that power. Some Chinese see the United States as inspired by hegemonic motives and attempting to establish a U.S.-dominated unipolar world order. Others evince sympathy and understanding for U.S. global leadership. Effective resolution of global problems requires a leader, according to this Chinese view, and only the United States has adequate “comprehensive power” to play a leadership role. Moreover, the substantive content of U.S. leadership generally accords with China’s own interests. Some Chinese even went so far as to suggest that the United States is a relatively benign great power, comparable to China during its periods of premodern greatness in East Asia.

There is a similar bifurcation of perceptions on the U.S. side. There is basic unanimity that China does not constitute a direct military threat to the United States. Beyond that, perceptions split. Some of the Americans view China as a nonthreatening power that is gradually joining the international mainstream. Others see it as a deviant from universal norms of behavior, both domestic and international. As such, China is often disruptive and a source of international instability.

On the basis of the considerable area of shared benign and positive images, Wang concludes that the two countries are not “enemies,” only “limited adversaries.” His presentation is cast in terms of the deterioration in relations that occurred during the 1990s, when strong voices in both countries proclaimed the other as an enemy. Such a black-and-white view does not accord with the complex, sophisticated elite perceptions uncovered by the interviews, Wang maintains. Most elites in both countries do not regard the other as threatening or as an outlaw country, and they are very much aware of the interdependence of interest and consequent need for cooperation. Thus, the two countries are “cooperating adversaries,” neither enemies nor allies, but normal powers with a range of convergent as well as conflicting interests.

Wang has some very interesting things to say about how various well-intentioned U.S. actions in the 1990s, intended to demonstrate U.S. sympathy with the Chinese people, were viewed in China as pressure, arrogance, and intimidation. He asserts that these cognitive processes have not transformed the psychology of the Sino-American relationship into a perception of “enemies.” One is still left with the fact that many negative events occurred after Wang’s interviews: the imbroglio over the 2000 Olympics, the linking and delinking of most-favored-nation status, Lee Teng-hui’s visit to the United States and the Straits confrontation, the bombing of the Chinese embassy in Belgrade and the besieging of the U.S. one, the spy plane incident, and so on. Assertions of faith, authority, and hope aside, we simply cannot know how much these events have altered the relatively positive elite perceptions Wang found. His study provides a solid benchmark for measuring subsequent change.

Constructing National Interests: The United States and the Cuban Missile Crisis. By Jutta Weldes. Minneapolis: University of Minnesota Press, 1999. 316p. \$47.95 cloth, \$18.95 paper.

Aaron Belkin, *University of California, Santa Barbara*

The Cuban missile crisis may be one of the most studied (overstudied?) cases in the literature on international relations. Theorists of almost every stripe have turned to it for evidence, and a brief search through the University of California's on-line library catalog indicates 83 books and 103 journal articles on the case. Despite the abundance of scholarship, however, Jutta Weldes has written a fresh and compelling book, one that no scholar of foreign policy crises should skip.

Weldes begins with an interesting question (pp. 2–3): “How do we get from the Soviet missile deployment in Cuba to the Cuban missile crisis?” After the Kennedy administration discovered Nikita Khrushchev's attempts to sneak medium-range ballistic missiles into Cuba, the American interest in getting rid of them was self-evident, and the White House never considered inaction to be a viable response. Although theorists have spent millions of foundation dollars investigating Soviet motives, no one has asked why the administration believed it could not ignore the situation. The answer is nonobvious and essential, Weldes suggests, for understanding the origins of the crisis.

Why did American national interest require removal of the Soviet missiles from Cuba? Weldes argues that the unthinkability of ignoring the deployment resulted from “the constant, numbing repetition of the same stock phrases and descriptions,” repetitions that “contributed to the reception of these representations as common sense” (pp. 226–7). These stock phrases (e.g., “the Soviet Union is bent on world domination”) were the linguistic effects of the “security imaginary,” a set of ideas about entities that populated the international system as well as relations among them. The postwar American security imaginary depicted the United States as the defender of freedom, while appeasement, Soviet duplicity, the creeping subversion of international communism, and the Cuban revolution were figured as threats. Security imaginaries clarify who we are and who our enemies are and how and why they threaten us.

Two processes—articulation and interpellation—explain how the national interest is stitched together out of the security imaginary. Articulation refers to the process by which “different terms and ideas come to ‘summon’ one another” (p. 98). For example, “references to Castro and his revolutionary associates were persistently articulated to the adjective ‘bearded.’” Invoking Castro's beard implied that he was “irresponsible, uncivilized, and a danger to the United States” (p. 98). Weldes argues that the national interest draws on numerous articulations (“communist,” “aggression,” and so on) and that, through repetition, these pairings can come to appear natural and part of common sense.

Interpellation refers to processes by which “identities are created and concrete individuals are interpellated by, or ‘hailed’ into those subject positions” (p. 103). “Out of an abstraction designating a territory, a population, and a set of governing principles and apparatuses is created an anthropomorphization, the fiction of an apparently acting subject with motives and interests” (p. 104). Weldes shows that postwar American identity came to incorporate several overlapping ideals, including leadership, defense of freedom, and

the maintenance of credibility. The construction of U.S. subjectivity helps explain why President Kennedy could not imagine ignoring the missiles, as failure to remove them would have undermined the core of American identity.

This study may call into question some of the value of applying scientific methods to the analysis of international crises. Certainly, the Cuban missile crisis can be conceptualized as an instance of a broader pattern of interactive events whose predictors and internal logic might be discovered through scientifically disciplined inquiry. It seems unreasonable to suggest, to take one of many possible examples, that there is nothing at stake in efforts to determine whether the missile crisis is best viewed as a prisoner's dilemma or a failed bluff (Glenn H. Snyder and Paul Diesing, *Conflict among Nations*, 1977, p. 114). But the reader comes away from Weldes's analysis suspecting that an appreciation of idiosyncrasy probably is essential for understanding what matters most about the story.

Of course, Weldes does not try to answer every important question about the origins of the Cuban missile crisis. She acknowledges thoughtfully that her emphasis on socially constructed aspects of international relations is not intended to imply that politics are arbitrary, and she says that power relations constrained the range of possible outcomes (p. 102). Perhaps in future work she and other scholars will explore interactions among reality constraints that are grounded in power and socially constructed national interests.

The findings of this study gesture ironically at a complicated set of relationships between scholarship and decision making. Motivated by the best of intentions, scholars who study international crises have spilled more ink formulating recommendations for policymakers than perhaps any other subset of the discipline. For better or worse, few if any of these recommendations appear to have been taken seriously in Washington (Richard K. Betts, “Should Strategic Studies Survive?” *World Politics* 50 [October 1997]: 7–34). Yet, Weldes suggests that scholarship did have an important although possibly unintended influence on policy during the Cuban missile crisis. “By authoritatively defining the real, [international relations theory] removed from critical analysis and political debate what was in fact a set of socially constructed representations” and “led to the reception . . . of one particular U.S. national interest (that of securing the removal of Soviet missiles from Cuba) as common sense” (p. 241). Weldes is not the first to notice this phenomenon, but her expertise on the origins of the national interest lends weight to the argument.

Another ironic implication of the study is that American decision makers may have had very little freedom of choice. Weldes and other critical theorists, such as Alexander Wendt, argue convincingly that individuals shape as well as respond to structure. If the missiles' meaning was socially constructed, perhaps Kennedy and his advisors could have interpreted the situation differently. Weldes does such a convincing job of linking the Cold War vilification of Soviet communism to the administration's belief that it could not ignore the missiles, however, that one wonders whether the White House could have imagined the weapons as anything but a metaphor for the East-West rivalry. Perhaps the president and his advisors could have chosen to understand the deployment in a different way, but they would have been swimming against a very strong discursive current that was only partially of their own making. What a terrifying thought to consider.