## POLITICAL THEORY

**Uncivil Society: The Perils of Pluralism and the Making of Modern Liberalism.** By Richard Boyd. Lanham, MA: Rowman and Littlefield, 2004. 351p. $75.00 cloth, $25.00 paper.

— Louis Hunt, *Michigan State University*

In his book, Richard Boyd offers a thoughtful reappraisal of the relation between the idea of civil society and the tradition of liberal political thought. He questions the view held by many contemporary political theorists that the development of civil society, understood as the realm of what Tocqueville called "voluntary associations" mediating between the individual and the state, is a necessary condition for the maintenance of liberal democracy. Against this position, Boyd argues that an earlier tradition of liberal thought was justifiably suspicious of the potential for subpolitical social groups—especially, but not limited to, intolerant religious sects—to undermine rather than support liberal institutions. In a series of well-argued chapters, he traces the trajectory of the connected ideas of civil society and social pluralism from initial suspicion to perhaps uncritical acceptance.

Boyd begins with a reconsideration of Hobbes's well-known view that even subordination to an absolute state is better than the threat of civil conflict and violent oppression inherent in subpolitical associations. The most original aspect of Boyd's analysis of Hobbes is his contention that Hobbes's emphasis on the solitary, rational individual—the very feature of his thought that makes him anathema to defenders of civil society—can best be understood as an attempt to counter the irrational psychology that animates religious sects and political factions insofar as they are bound together by often fanatically held common convictions and beliefs: "Intended to rescue solitary and rational individuals from the irrational passions that inflame groups, Hobbes's individualism merits attention as an alternative to the pluralism he rejects" (p. 68).

In his chapters on Locke and the Scottish Enlightenment, Boyd demonstrates the persistence of this suspicion of sectarian intolerance, even as these thinkers rejected Hobbes's authoritarianism for an incipient conception of civil society as distinct from the institutions of the state. Of particular interest is Boyd's analysis of the debate within the Scottish Enlightenment about what makes some social groups "civil" and others "uncivil." This is an issue of particular concern today given the confused and contradictory responses of contemporary liberal regimes to the demand for toleration made by illiberal political associations and religious sects in their midst. To take a recent example, is the French law forbidding Muslim girls from wearing head scarves to school a violation of the rights of a religious minority? Or is it a legitimate response by a liberal secular state to the dangerous zealotry of an intolerant religious sect?

As the author shows in his careful analysis of the views of Hume and Smith, there is no easy answer to such questions. Hume shared Hobbes's distrust of sectarian religious "enthusiasm" as a source of political dissension, but like Madison in *Federalist* 10, he did not think it possible to repress factions directly without also extinguishing civil liberty. Hume and Smith suggested various indirect solutions to the problem of diminishing the influence of intolerant factions within society. Boyd notes that in addition to their defense of the ameliorating effect of commerce on religious sentiments, both Hume and Smith supported the role of an established church as a check on the fanaticism of the dissenting sects. Indifferent or hypocritical adherence to an established religion was less dangerous to civil liberty, in their view, than the sincere beliefs of religious dissenters.

In the latter half of his book, Boyd explores the reversal in attitude toward civil society that emerged at the end of the eighteenth and the beginning of the nineteenth century. It is here that he locates the origins of our contemporary valorization of civil society. The most important figure in his account of this transformation is Edmund Burke. The crucial backdrop to Burke's defense of the "little platoons" of civil society was the centralizing tendency of the French revolutionary state and its hostility to religion and landed property, the twin pillars, in Burke's view, of the English constitutional order. Anticipating Tocqueville, Burke regarded the dispersal of power at the level of society as a necessary bulwark against centralized despotism. Speaking of both men, Boyd writes that "beginning with the French Revolution, they witnessed an age of centralized political power, the abdication of political responsibility by an aristocratic class and the concomitant democratic revolution. Perhaps for the first time in the history of political thought, the vices of individualism seemed to outweigh the perils of pluralism" (p. 170).

There can be no doubt that the positive evaluation of civil society in contemporary political discourse derives much of its power from the contrast with the totalitarian regimes of the twentieth century, which both inherited and radicalized the ambition of the French Revolution to destroy any intermediary institutions between the individual and the state. For the dissidents in formerly communist Eastern Europe, who were probably most responsible for the contemporary revival of interest in civil society, the term *civil society* stood for the intermediary institutions that were lacking in the repressive regimes under which they lived. In his discussion of the political theories of Michael Oakeshott and F.A. Hayek, Boyd shows how these thinkers, in response to the experience of totalitarianism, attempted to develop models of civil association that could dispense as much as possible with any substantive role for the state.

With the collapse of communism and the rise of political Islam and other fundamentalist social movements,



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however, the earlier liberal concern with the danger of illiberal sectarianism is arguably of greater salience today. Boyd's historical reconstruction of this position is illuminating and provides a good starting point for thinking through these questions as they arise in contemporary social and political life. It is not a criticism of his work to say that he provides no clear formula for discriminating between civil and uncivil associations and supplies no simple answer to the even more difficult question of how to check the influence of uncivil associations without undermining the institutions of liberal democracy. His is a work of intellectual history, not public policy. But, as Boyd reminds us in the conclusion of *Uncivil Society,* "nothing less than the fate of liberty" (p. 320) depends on our finding workable answers to these questions.

**The Communitarian Constitution.** By Beau Breslin. Baltimore: Johns Hopkins University Press, 2004. 288p. $48.00.

— Daniel Cullen, *Rhodes College*

Beau Breslin desires to make a breakthrough in the "communitarian-liberal debate" by moving it to the plane of constitutional discourse (pp. 18–19, 109). He describes his study as a work of constitutional theory, but of a generic sort; his constitutionalist standard owes more to John Rawls than to James Madison. "Constitutionalism" refers varyingly to an architectonic vision of political perfection (or "modeling") and to the limits on political power. The communitarian constitution is caught in a dilemma that arises from these conflicting imperatives.

Breslin recapitulates (fairly, but perhaps at unnecessary length) the familiar critique of liberal theory and practice: Liberal theory rests on an implausibly atomistic understanding of selfhood, which propagates an unreasonable understanding of the requirements of individual freedom; liberal politics systematically discounts or undermines the social foundation on which genuine freedom depends by an overzealous concern for the protection of individual rights. He is aware that the term "communitarianism" is contested and unstable, but he argues that the multifaceted critique of liberalism by Charles Taylor, Alasdair MacIntyre, Michael Sandel, and Michael Walzer, augmented by the "movement" led by Amitai Etzioni, amounts to a coherent vision of an alternative polity. Indeed, Breslin insists that it is time for the communitarians to take responsibility for the comprehensive framework implicit in their myriad criticisms of liberal politics, and he constructs on their behalf a communitarian "city in speech," as it were (pp. 13–14, 24, 107). Holding communitarians accountable for their emphasis on the "constitutive sources" of the self, he outlines *the* communitarian constitution, imagining the actual policies that communitarian theory entails (p. 101). His ultimate purpose is to demonstrate that the communitarian polity is essentially incompatible with constitutional principles: No communitarian polity can abide the "external" limitations on popular will that are the hallmark of constitutionalism because communitarianism insists that society be regulated exclusively by "internal" principles, that is, the "shared understandings" or the substantive account of the good life that makes a people what they are (pp. 136–41). "Communal values will always and inevitably clash with constitutional provisions" (p. 143). Like the Platonic city in speech, the communitarian constitution is therefore an impossibility, or a paradox.

The plan of the book is to provide what communitarian thinkers have so far failed to deliver: a positive and systematic account of their alternative to liberalism, and then to subject that framework to the scrutiny of constitutionalism. Breslin suggests that communitarians have failed to appreciate the significance of their critique, which is radical, even revolutionary (pp. 13, 108). He sees the contemporary argument as a struggle over fundamental principles, reminiscent of the debate over ratification of the Constitution. At the same time, he acknowledges that communitarians themselves assure us that "their vision of a political society is little different from the one we currently inhabit" (p. 103). *The Communitarian Constitution* stands or falls, then, on whether Breslin has understood the implications of communitarian theory better than communitarian theorists themselves. One reason to doubt the author's construction is that several times, the book both maximizes and minimizes the differences between communitarians and liberals, and the reader is left wondering how wide the divide really is (p. 217).

The search for antecedents of the contemporary dispute results in some bold claims. Lincoln's suspension of habeas corpus is portrayed as a paradigm case of the antagonism between communitarianism and constitutionalism. Lincoln is a proto-communitarian in his willingness to set aside the constitution to effect "the will of the community" (p. xi). Breslin holds that even in the circumstance of civil war, nothing can prevail over the provision of the constitutional text (p. 25). But one wonders whether this extraordinary example can bear the weight the author attaches to it. Breslin's view almost suggests that the Constitution might indeed be a suicide pact. (He is also critical of the Patriot Act and links it to the same alleged communitarian syndrome [pp. 149 n.75, 215–16].) That the Lincoln who recommended that reverence for the laws and the Constitution become our "political religion" represents the antithesis of constitutionalism is an odd result. One might say that Breslin makes the mistake of forgetting that for Americans, the Constitution is not fundamental law but only the most fundamental law we have. Constitutional government means nothing if it does not place limits on power, and through the American Constitution, the people limit or control themselves by constitutionalizing their sovereignty, by agreeing to limit the expression of that sovereignty to constitutionally authorized means; but *the* principle of constitutionalism for Lincoln remained the natural rights and equality of



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human beings, the premise from which "the sovereignty of the people" is derived. Lincoln's controversial decision (in emergency conditions) arguably reflected the right relationship of ends and means, even if his vision of a constitutional government "of, by and for the people" runs afoul of Breslin's strict separation of internal principles and external limits.

The same issue of ends and means could be pressed against Breslin's understanding of the Anti-Federalists, who are also depicted as proto-communitarian in their concern to preserve civic virtue and local identity (pp. 5–11). One wishes that the author had confronted some of the objections to construing the Anti-Federalists as "classical republicans" who promoted a genuine alternative to modern natural rights philosophy, for much hinges on his claim that the Federalist-Anti-Federalist struggle foreshadowed the communitarian–liberal debate. If, as Hebert Storing and Thomas Pangle have argued, both sides of the ratification debate accepted social contract theory and its natural rights foundation, Breslin's claim collapses, and one must wonder whether the gulf between communitarians and liberals is also as great as he suggests.

Although he is aware of the danger of "building a communitarian straw man" (pp. 79, 107–8), Breslin fails to avoid it. It is hard to imagine any communitarian thinkers who would ratify the constitution he has framed for them. Whereas communitarians complain of *excessive* individualism, emphasize *restoring a balance* between rights and responsibilities, and even warn of the dangers of promoting community *for its own sake,* Breslin saddles them with a vision of "communal supremacy" that is nearly totalitarian in its emphasis on promoting social cohesion: "All obstacles, therefore, that may potentially hinder the realization of that primary mission [determination of the common good] must be eliminated" (p. 135; cf. pp. 80, 86, 88, 148–49). Citizens of the communitarian polity "have no recourse from the apparently unfettered grip of the community" (p. 25). This reconstruction of the communitarian position is reminiscent of Patrick Devlin's loose generalizations about moral change and the threat of social "disintegration." Breslin suggests that the communitarian polity is held hostage by its existing value commitments and can brook no restraints on communal will (pp. 142–43).

Community politics must therefore be essentially preservative and defensive and not real politics at all; but again, which communitarian theorist would endorse such a view? Ironically, in his very efforts to be fair and to acknowledge the diversity of communitarian thought, Breslin finds himself forced to a level of abstraction at which suitably generic versions of communitarian principles become available; but for that very reason, important qualifications or nuances are abandoned. Virtually every communitarian thinker is sensitive to the difference between attempting to create community where it does not or cannot exist and nurturing it where it does; and every communitarian thinker expresses a concern for individual freedom and therewith restraints on the popular will. Despite his comprehensive and sympathetic understanding of communitarian theory (from which the reader will profit), Breslin has substantially altered its claims in the process of refining them. Communitarians respect individual rights and fear majority oppression but question whether that respect must pit freedom and authority against one another in a zero sum game. It is not at all clear that they need a constitution of their own; and surely it is a telling point that they do not seek one.

**Challenging Diversity: Rethinking Equality and the Value of Difference.** By Davina Cooper. Cambridge: Cambridge University Press, 2004. 246p. $75.00 cloth, $29.99 paper.

— Sanford Lakoff, *University of California, San Diego*

In the *Encyclopedia* (1751–65), the Chevalier de Jaucourt observed that while "natural equality" is the moral foundation of liberty, "absolute equality" in society is a "chimera" of fanatic minds. In the social state, he explained, distinctions and subordinations are necessary, including those attached to "differences of condition." Later, as the old order yielded to democracy, Tocqueville discerned an inexorable tendency toward the leveling of those very differences. Would it end, he wondered, in a new Caesaristic despotism in which an atomized populace, deprived of the protection of intermediate powers, is enslaved by an all-powerful ruler? In modern societies considered free, the march toward equality of conditions has stopped well short of this dead end. But how does the modern conception of equality square with ways of living that challenge prevailing social norms? That is the subject of this inquiry, which, somewhat ironically, aims to restate from a leftist perspective the case put by the Chevalier de Jaucourt for accepting some such differences.

Davina Cooper describes her perspective as "progressive," "radical democratic," and "left," distinguishing it from the views of liberals like Ronald Dworkin and "liberal multiculturalists" like Will Kymlicka. In other words, she starts with sympathy for efforts to diminish remaining social inequalities, especially those involving race, gender, sexual orientation, age, and disability—social relations "which have joined, and in some cases superseded, the left's preoccupation with class" (p. 3). But her main concern here is to investigate the ways in which diversity, or respect for different ways of living, requires something other than sameness of treatment or recognition of group rights. She sees "diversity politics" as an effort to reverse "the monolithic tendencies" found not only in liberalism but even in Marxism and feminism (p. 40), and she supports "the struggle to protect and enhance counternormative ways of being and living" (p. 9) because they express individual freedom and because cultural diversity is to be valued in its own right.



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Although Cooper's effort to address this issue is highly theoretical, it refers to field research she has conducted in Britain and that has otherwise been reported. Among the examples of nonconformity cited are those of Orthodox Jews, homosexuals, the transgendered, women peace campers, a progressive school, and "LET" ("local exchange trading systems"). She also mentions claims for the rights of smokers, the homeless, head-scarf wearers, hunters, loiterers, and those accused of being nuisances, and, in passing, gated communities, sex workers, truants, and protest groups. Her stated intention is to use such examples as "a prism through which to explore the space of diversity politics" (p. 35).

Cooper does not so much set out to answer the question of what diversity requires as to explore its implications and ambiguities. The trouble is that in every particular case, the complexities defy formulaic solution and reveal "discrepant interpretations" of freedom (p. 26). A case in point is the desire of Orthodox Jews to string a thin, high wire around a neighborhood or city so as to create an "eruv"—a symbolic space allowing them to carry on activities beyond the home on the Sabbath. To allow it would show respect for privacy and group differentiation; to deny it could be seen as expressing intolerance and reinforcing historical Christian dominance over Judaism. But would it cause harm by attracting so many believers as to make other residents uncomfortable living in a religious enclave to which they do not belong? Would nonbelievers and secular people see it as an aggressive form of territorial self-expression aimed at them? Might it reduce property values? Would it revive memories of concentration camps among Jewish immigrants and attract anti-Semitic vandalism? Such difficulties lead Cooper to wonder how benefits and harm should be understood. After examining the issues, she concludes that "diversity politics" provides "a set of premises and perspectives" favorable to the erection of an eruv (p. 35).

To this reader, it remains unclear why Cooper comes to this conclusion and why, more broadly, she supposes that her perspective offers better guidance than the liberal viewpoint that individuals should be allowed to pursue, independently and in association, whatever ways of life they choose for themselves, so long as these do not interfere with the liberty of others. For her, everything depends on whether the way of life enhances values she considers progressive or at least not threatening to social equality. Presumably, the demands for acceptance by communities committed to "practices of domination" (p. 193) would be disallowed. But where should the line be drawn and by whom? Should gated communities be forbidden because they discriminate against the poor? Should churches be banned if they bar women from the priesthood and are governed undemocratically? Must public schools in France allow Muslim girls to wear head scarves and refuse to take part in athletics—or do these practices reinforce the subordination of women? Nothing in her exposition provides a clear answer.

This substantive failing is only exacerbated by the style of exposition, which may be congenial to poststructuralists, radical feminists, and deconstructionists but will often strike others as opaque and convoluted. The substitution of "discourse" for theory, and the use of "privilege" and "disadvantage" as verbs, are now fairly common if lamentable practices. So, to a lesser extent, is the use of topographical terminology, such as "space," "pathways," "terrain," and "mapping," but here these terms are used so freely that they sometimes lose specificity. At one place, the reader is informed that "a valorised and protected discursive space has been crafted around the experience of minority status" (p. 6); at another, that while "national spaces" appear more culturally diverse and heterogeneous with respect to gender, "these trends do not negate the countervailing drive for spaces to become more ordered, efficient and mono-functional" (p. 97). If that is not confusing enough, what is to be made of such expressions (and this is only a small sample) as "overestimating or decontextualizing the interpretive power of the utteree" (p. 34), "a co-constitutive dyad" (p. 99), or "the dilemmatic environment" (p. 197)? He may have been wrong, but the Chevalier de Jaucourt was a lot clearer.

**Justice and Democracy: Essays for Brian Barry.**
Edited by Keith Dowding, Robert E. Goodin, and Carole Pateman. New York: Cambridge University Press, 2004. 242p. $70.00 cloth, $25.99 paper.

— Peter Stone, *Stanford University*

Getting academics to focus on a single topic can be a bit like herding cats. Present them with a theoretical problem and they tend to write about whatever they want, making only the merest nod in the direction of the problem specified. The result is that many edited volumes ostensibly devoted to a common theme wind up containing myriad papers that do little to speak to either the common theme or one another.

*Justice and Democracy* does remarkably well at avoiding this common ailment of edited volumes. The theme is quite clear: How do we make sense out of the ideals of justice and democracy such that both are normatively compelling and compatible with each other? This question is highly significant, and its answer is far from obvious. Democracy, after all, holds that a society's people should (more or less) get what they want. Justice holds that a society should (more or less) distribute goods (and bads) in accordance with certain principles. But what happens when people want to distribute goods in a manner contrary to those principles? How does one assign the proper place to each of these values? In their introductory essay (Chapter 1), the editors present this problem for its



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contributors to address, and those contributors generally do so in a manner that is constructive and engaging.

All of the contributions to this book deal with democracy and justice in some way. For some, the focus is a bit lopsided. For example, Julian le Grand's analysis of voluntary social exclusion (Chapter 10) and Russell Hardin's critique of contemporary nationalism as a divisive, rather than a unifying, force (Chapter 11) both focus on questions of justice to a degree that mostly leaves democracy out of the picture. The essays by Norman Schofield (Chapter 4), Albert Weale (Chapter 5), and Jon Elster (Chapter 7) offer arguments that also focus on one of the concepts, but in a manner that has clear implications for understanding the other. Schofield offers a model of political behavior in which citizens can act upon both interests and judgments. He also finds precedents for this model in the writings of Condorcet and Madison. While he says little directly about justice, his model should be of interest to anyone trying to puzzle out how democratic citizens might consistently pursue both private ends and public principles. Elster examines the mechanisms by which people might be moved to act impartially (i.e., on principles of justice) for self-interested reasons. The relevance to the design of democratic institutions should be obvious. Finally, Weale forcefully argues that contractarian arguments cannot work without begging important questions about what constitutes rational and/or reasonable action. He therefore concludes that political theory is not reducible to ethics but must find its own foundations. Contractarians concerned with democracy and/or justice should not neglect Weale's argument.

The remaining five chapters unambiguously explore the relationship between democracy and justice. David Miller (Chapter 8) approaches the question from its flank, by asking what justice demands with regard to the provision of public goods, and what restrictions this may impose on that which democracies may legitimately do. Philip Pettit (Chapter 9) asks what the common good is and how we might identify it. To the extent that the common good is identical with what justice demands—an idea that is arguable but, I believe, defensible—Pettit's argument converts democracy into a means for advancing justice. This is the position defended forthrightly by Richard Arneson (Chapter 3), who denies that democracy has any value except instrumentally, as a way to achieve other values (such as justice). Robert Goodin (Chapter 6) takes a similar position. He wishes to make democracies just by inducing people to make democratic decisions only on the basis of "justice-relevant reasons."

Only Keith Dowding (Chapter 2) attempts a resolution that does not appear to reduce democracy to a means of realizing justice. While he denies that such a reduction is possible, he does assert that "the fundamental justifications for having democratic procedures lie essentially in the same realm as arguments for social justice" (p. 25). Does this leave democracy independent of justice, as Dowding believes? I am not sure. Dowding's conclusion is that "arguments for changing the institutions we have for promoting both democratic outcomes and just ones also proceed from the same basis, that is, the unjust consequences which follow from the institutions we have" (p. 39). But that sounds a lot to me like saying that democratic institutions are only valuable insofar as they promote justice. Dowding's chapter is entitled "Are Democratic and Just Institutions the Same?" Judging from his conclusion, the correct answer might well be "yes."

I have suggested that Pettit, Arneson, Goodin, and Dowding all reach a similar conclusion regarding justice and democracy. Each of them reaches this conclusion, however, by a very different route. In a review of this length, I cannot detail the differences among these arguments. I can only emphasize that anyone interested in the question of whether democracy is of purely instrumental value will benefit from reading all four chapters.

"Brian Barry," the editors of this volume write, "almost uniquely has figured centrally in debates on both democratic theory and social justice" (p. 4). It is to Barry that this volume has been dedicated. And the tribute is a fitting one. The only honor greater than having your colleagues dedicate a volume to you is having them stay on topic while writing for that volume.

**Feminist Interpretations of Niccolo Machiavelli.**
Edited by Maria J. Falco. University Park: The Pennsylvania State University Press, 2004. 422p. $95.00 cloth, $39.50 paper.

— Anthony Parel, *University of Calgary*

This volume of 14 essays makes a valuable contribution to three related academic fields: history of feminist thought, history of political thought in North America, and the general history of political thought. In her Introduction, editor Maria Falco has done an excellent job of bringing her readers up to date on the evolving state of the feminist critiques of Machiavelli.

Machiavelli had raised (and continues to raise) a number of serious issues: the role of *virtu* or human agency in political "founding" and political "innovation" (the problem of the new prince); in the maintenance of liberty through laws, education, and the satisfactions of the humors; in the necessity of expansion through empire; in the primacy of the ethic of the ends over that of the means; and in the rejection of any reference to the transcendental destiny of humans. The case for human agency was made against the background of an outdated Ptolemaic cosmology, according to which the natural motions of the planets were thought to exert not only physical but also moral influence on individuals, states, religions, and history. In this universe, regulated by uniform laws, unforeseen chance events also occurred. This latter phenomenon, treated in philosophy as "chance" or *tyche,* was treated by Machiavelli as if it were



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produced by the personified figure of *fortuna,* considered alternatively as a woman and a goddess (*dea*). The exercise of human agency in such a cosmos appeared to him to involve a struggle with naturalistic contingency.

How the treatment of these grave issues was affected by Machiavelli's obvious gender bias is the large question that this volume addresses. Did the bias affect the substance of this thought or only its mode of articulation?

Feminist opinion is divided—as these essays amply testify. Its focus is mostly on the woman metaphor of *The Prince* 25, on the image of the citizen-soldier, on the evaluation of historical figures such as Queen Giovanna of Naples and Caterina Sforza of Forli, on fictional characters such as Lucrezia and Sofronia, and on the portrait drawn of fortuna as a dea, as in *Discourses* II.1 and Machiavelli's minor poems.

The divisions within the feminist school follow roughly the following lines. Jean Bethke Elshtain started the discussion in 1981 when she spoke of the war of the sexes. In 1984, Hannah Pitkin carried it further. Arlene Saxonhouse, demurring, took a different line, arguing that there was plenty of gender bending in Machiavelli and that, given certain cultural conditions, women could turn out to be as good citizens as men. Machiavelli himself was uncertain about what role to assign to women in politics. Wendy Brown spoke hopefully of a postmasculine future for politics. Catherine Zuckert painted a different picture of his women characters than did Melissa Matthes. On the question of citizen-soldier, R. Claire Snyder argued that *educazione* was likely to be more relevant than gender, a point that is compatible with the position taken by Cary Nederman and Martin Morris. The latter see his theory of humors as permitting a gender-free way of settling internal political conflicts.

Vesna Marcina has made one of the creative suggestions found in this volume, namely, the need to reread Machiavelli in the light of the intellectual context of his time. Jane Jaquette has followed this up with yet another creative suggestion, namely, the need to rethink Machiavelli. We see in this volume, then, a welcome threefold progression—from complaints to the need to reread, to rethink, and to reinterpret our author—and in that order. And everyone, not just the feminists, should be grateful for this. And Andrea Nicki's explanation of Lucrezia's satisfaction with adultery (p. 377) fully justifies the need for the new approach. Whereas Machiavelli had justified it only in terms of the discredited ethic of astral determinism, Nicki justifies it in terms of Lucrezia's alleged "new moral insight."

Nowhere is the need for the new approach more urgent than in interpreting Machiavelli on fortuna. Rereading him in context would reveal that he had based his case on a soon-to-be discarded Ptolemaic cosmology. The rereading therefore should start with works on fortuna produced in his own time. The authors would include Pico della Mirandola, Savonarola, Pontano, Nifo, Bellanti, Ficino, and Pomponazzi, among others. In the 1490s, Florence was so inundated with works on the old cosmology that Savonarola counted 12 authors by name, including Ptolemy, "the prince of these astrologers" (*il principe di questi astrologi*). Pico's *Disputationes adversus astrologiam divinatricem, libri xii,* appeared in 1496, Savonarola's *Contra astrologiam divinatricem* appeared in 1497, and Machiavelli was appointed Secretary in 1498. Jacob Burckhardt said of Pico's work that it made "an epoch in the subject," and Eugenio Garin compared it to *Novum Organum* and *Discours de la Method.*

Allied to Ptolemaic cosmology was astrological medicine—both fields being endorsed in the Preface to the *Discourses,* the only portion of the work that has survived in Machiavelli's autograph. Galen, Galenic writings, and Albumasar are also indispensable background sources for Machiavelli studies.

When all this is taken into account, what emerges is that Machiavelli's subscription to a premodern cosmology and medicine disqualifies him from being considered the founder of modern political philosophy—a title, to his great credit, he had never claimed. One cannot be modern and at the same time explain fortuna the way he did. A negative proof of this is that no modern political philosopher has ever paid any serious attention to fortuna. The new cosmology made that unthinkable.

The new possibilities that *Feminist Interpretations of Niccolo Machiavelli* has opened up are challenging and rewarding. Young scholars who have the mental flexibility and the time to reread, to rethink, and to reinterpret him should take full advantage of them.

**Talking Democracy: Historical Perspectives on Rhetoric and Democracy.** Edited by Benedetto Fontana, Cary J. Nederman, and Gary Remer. University Park: The Pennsylvania State University Press, 2004. 337p. $55.00.

— Devin Stauffer, *The University of Texas at Austin*

This edited volume of eleven chapters draws from sources as diverse as Thucydides, Plato, Cicero, St. Augustine, John of Salisbury, Christine de Pizan, and John Stuart Mill. On the one hand, the volume is intended as a selective but wide-ranging study of the history of the theory and practice of rhetoric. As the editors Benedetto Fontana, Cary J. Nederman, and Gary Remer stress in the Introduction, the essays are "committed to a historical analysis of their subject matter" (p. 20). But the volume is much more than an exercise in historical analysis or an intellectual history of theoretical reflections on rhetoric. The essays are drawn together into a genuine whole by an argument that runs throughout the book.

The argument of *Talking Democracy* is first presented in the extensive Introduction, in which the editors prepare the way by discussing the rise of deliberative democracy



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within democratic theory. They are in some sympathy with the aims of deliberative democrats, such as Joshua Cohen, Seyla Benhabib, Amy Gutmann, and Dennis Thompson, who, like their civic republican counterparts, are seeking "to recover the original and more profound sense of political activity, as something that transcends the immediately private and economic" (p. 5). Yet while the editors are impressed by the originating impulse behind the movement toward a more deliberative model of democracy, they present their volume as a critique of deliberative democracy. Their quarrel with the deliberative democrats is in part a historical one. Contrary to the deliberative democrats' dismissal of classical and medieval forms of political activity as irrelevant to contemporary notions of public deliberation—a dismissal that has its roots in Jürgen Habermas's argument about the transformation of the public sphere in the modern era—the editors and other contributors want to rehabilitate premodern models by arguing that important and vital forms of deliberation can be discovered in at least some premodern polities. The richness and relevance of premodern theory and practice has been obscured, they argue, by the deliberative democrats' overly restrictive notions of what should count as legitimate public speech. This historical line of argument, however, is meant not merely to correct a deficiency in the historical analysis of the deliberative democrats but also to open up a broader line of criticism. The deeper argument of the book is that a reconsideration of the history of the theory and practice of rhetoric as a vital form of public speech can lead to an understanding of democratic activity that is more complex, more vibrant, and more realistic than the "desiccated, abstract model of democracy" produced by the idealistic and overly rationalistic demands of the deliberative democrats (p. 18).

The critique of deliberative democracy running throughout the book has a political dimension and an epistemological dimension. As a challenge to the vision of politics presented by deliberative democracy, the contributors raise the question: To what extent can power, interest, and irrational passions really be overcome by open discourse? And on the epistemological level, they raise the question: Is it realistic to expect, or even to hope, that the conclusions reached by public deliberation could ever be unblemished products of fully transparent and rational processes? In posing these questions, the contributors are careful to avoid the stance of premodern and postmodern cynics who would argue that politics will always reduce to power and the techniques used to disguise it—to mere force and fraud. Instead, they want to find a middle path that is appreciative of the messy business that democratic deliberation really is, even or precisely when it is conducted through public speech. They argue that democratic deliberation, as it actually operates and especially when it involves rhetoric, is deeply flawed but not without its own dignity. If such deliberation does not arrive at perfect clarity because it must reach decisions based on sketchy information provided by biased sources, that is both its vice and its virtue, since it calls on citizens to exercise a kind of judgment that resembles the sifting activity of a historian like Thucydides, as Arlene Saxonhouse argues in her chapter, "Democratic Deliberation and the Historian's Trade." And if it must be acknowledged that public speech, which is rhetoric more often than "pure speech," appeals not just to reason but to a whole range of complex human emotions and passions, that means that to understand democratic politics, we need a rich moral psychology, such as the one Russell Bentley finds in Aristotle's *Rhetoric* in his chapter, "Rhetorical Democracy."

In addition to presenting a realistic and multifaceted portrait of democratic deliberation, the book also recovers and explores the troubling moral questions surrounding the use of rhetoric. Precisely because premodern authors took rhetoric so seriously, they had to confront the charges that can be leveled against rhetoric. The two charges that most concern the contributors to *Talking Democracy* are the charge of elitism and the charge of manipulative deception. In short, does the art of rhetoric give the speaker an illegitimate source of power that violates the democratic principle of equality? And does not rhetoric often violate the moral principles of openness and honesty? These vexing questions run throughout many of the chapters, especially the one on Cicero by Remer and the fascinating chapter on John of Salisbury and Christine de Pizan by Nederman and Tsae Lan Lee Dow. Broadly speaking, on these moral issues, the contributors are again seeking to find a middle path—in this case, a path between an unrealistic and naive insistence on perfect equality and transparency, and an unprincipled slide into a Machiavellian acceptance of all forms of deception and manipulation.

This is an excellent volume, not only because the essays recover forgotten sources of profound reflection on rhetoric and the true nature of democratic deliberation, but also because the contributors display a remarkable flexibility in bringing historical perspectives to bear on contemporary issues. Perhaps the greatest virtue of this volume is the service it does in bridging the divide within political theory between the study of the history of political thought and contemporary attempts to construct new theoretical models. The contributors have not merely argued that even premodern sources can shed light on questions central to political theory today—they have demonstrated it.

**Identity in Democracy.** By Amy Gutmann. Princeton: Princeton University Press, 2003. 264p. $35.00 cloth, $ 17.95 paper.

— Sonia Kruks, *Oberlin College*

What ought to be the role of identity groups in the politics of a just democratic society? In her book, Amy Gutmann sets out to explore this question. She does not



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offer a definitive answer to it; however, she does offer evaluative criteria by which we may decide, on a case-by-case basis, whether or not the political interventions of organized identity groups are beneficial to a democratic society such as she presumes the United States to be. When, Gutmann asks (p. 5), may the politics of organized identity groups contribute to a society structured on what she identifies as the three basic principles of democratic justice: civic equality (equal standing as a citizen), liberty, and opportunity? Conversely, when are they inimical to such principles?

Gutmann's goal, wisely, is not to enter the fray of the often-heated, all-or-nothing, for-or-against debates that have surrounded identity politics. Instead, she seeks to clarify what constitutes identity group politics in contradistinction to interest group politics; to differentiate between differently constituted kinds of political identity groups (the four core chapters address, in order, [1] cultural identity groups, [2] voluntary identity groups [3] ascriptive identity groups, and [4] the question of whether religious identity is a special case of identity group politics); and to discriminate among their kinds of political interventions so as to be able to evaluate "the good, the bad, and the ugly of identity politics." Methodologically, the book proceeds to make such discriminations through a nuanced evaluation of numerous specific examples and concrete cases. These enable her to explore issues as diverse as those arising from practices of clitoridectomy among immigrant groups in the United States (Chapter 1), the exclusion of homosexuals from the Boy Scouts (Chapter 2), the self-interested use of ascriptive identity for personal material gain (Chapter 3), or balancing religious conscience with the demands of nondiscrimination (Chapter 4), among many others.

One important question that Gutmann addresses is how to distinguish identity groups from more traditional interest groups. The paradigmatic distinction she offers is that interest groups are instrumental alliances of self-interested individual agents and are formed only for the purpose of attaining a particular end that their members all want, whereas identity groups are bound up "with a sense of who people are" (p. 15). Identity groups thus have important affective qualities, and are often about a shared sense of belonging—that is, "identification" with others. This is especially so for identity groups based on involuntary ascription, for example, groups organized to resist racist or gender oppression. However, as she notes, in actuality, the distinction between interest and identity groups is often blurry. For identity groups frequently struggle for their members' interests, while members of interest groups sometimes develop identifications with one another over time. This being so, it might well be that many of the questions the author raises about identity group politics also apply to more conventional interest group politics.

Gutmann is highly critical of instances in which the rights of an identity group are asserted at the expense of the rights of any of its individual members for, she claims, group identity can never be fully constitutive of individual personhood. Thus, principles of democratic justice are violated in cases that subordinate an individual's rights to the claims of his or her group. But questions about the relationship between group identification and individual selfhood remain disappointingly underexplicated in the book. Gutmann's claim that social identity is not ever wholly constitutive of the self veers strongly toward a conception of the human self as a rational, autonomous individual, one that is a bearer of interests and a maker of free choices existentially prior to its identities. Such a conception of the self has been profoundly put into question not only by thinkers in the communitarian tradition who have emphasized the strong social constitution of selves, and by poststructuralist thinkers who emphasize the discursive construction of selves, but also by feminist and critical race theorists who have argued that identities such as gender and/or race are so deep that they are ineradicably constitutive of selfhood. An engagement with other perspectives such as these would have clarified Gutmann's own conception of selfhood and further strengthened the book.

In spite of her concerns that identity groups may sometimes be coercive of their members, or that they may be too particularistic in their agendas, Gutmann is also appreciative of the rich goods that identity group politics may bring. For there are positive values to "identification" that are sui generis and that are not reducible to either rational self interest or to principled moral obligation. Beyond matters of political efficacy, participation in organized identity groups may give people a much-needed sense of belonging and so contribute to their personal well-being. In addition, participation may also enlarge our sensibilities about the injustices suffered by others beyond our own identity groups. Thus, Gutmann argues, impartiality and a strict sense of moral obligation are not necessarily the most effective paths toward a more just society. For when we identify with others, we may fulfill ourselves in acting on their behalf, instead of sacrificing ourselves to do so: People "can live more fulfilling lives by contributing to justice-friendly associations of their choice [such as the Red Cross, or Oxfam] out of identification rather than obligation" (p.148). By pointing us toward the importance of the domains of affectivity and emotion as also integral aspects of a just democratic society, she provocatively complicates those conventional liberal arguments that ground accounts of democratic justice above all in principles. Both here, and elsewhere, *Identity in Democracy* provides a rich and nuanced resource for thinking creatively about the place of identity groups in contemporary political life, and Gutmann opens up new and important avenues for further inquiry.



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**Private Selves, Public Identities: Reconsidering Identity Politics.** By Susan J. Hekman. University Park: The Pennsylvania State University Press, 2004. 159p. $35.00 cloth, $24.00 paper.

**The Politics of Identity: Liberal Political Theory and the Dilemmas of Difference.** By Michael Kenny. Cambridge: Polity Press, 2004. 212p. $59.95 cloth, $27.95 paper.

— Beth Kiyoko Jamieson, *Princeton University*

The debate about multiculturalism takes many forms, and can focus on cultural domination, legal recognition of difference, and particular policy construction. But in these two volumes it pertains to liberal political theory and identity politics. Both are concerned with the challenges evident in the inability of some conceptions of liberalism to respond to the claims of the political relevance of categories of identity. Both are well grounded in current literature exploring the limitations and possibilities of liberalism. Both focus more on the political theory than on the political reality. But in the end, they champion different sides—Susan J. Hekman reconceptualizes and rehabilitates identity politics, and Michael Kenny reframes and reinvigorates liberalism. Although their conclusions are not incompatible, they are aimed at different audiences. Were the authors sharing a stage, they might largely talk past each other.

Hekman's *Private Selves, Public Identities* is a valuable and insightful book, which offers a fresh and well-argued defense of the importance of identity politics. Her analysis is strongly situated in feminist theory. Her understanding of the pitfalls of postmodern subjectivity (i.e., "woman" is a fiction), as well as liberalism's claim to neutrality (i.e., "man" equals human), informs her claim that the first point of error is our understanding of identity. Hekman uses object relations theory to challenge both Judith Butler's (*Gender Trouble,* 1990) assertion that (gender) identity is simply performative, and therefore identities might be made and remade in perpetuity, as well as the polar notion that identity is essential, unchanging. She argues that all individuals have a core self, formed in early childhood in relation to others, particularly in the complex relationships of parenting. That self endures, although it always reflects the context of being "constituted by a complex array of forces that are both public and personal" (p. 6). In this way, her conception of identity is both strong and contingent—providing a more realistically contestable character for deployment.

Further, Hekman's focus on identity illuminates the particularity embedded in liberal theory. Although some defenders of liberalism have worried that identity politics has no place in public life, Hekman explodes the notion that identity has ever been excluded from the polity. Citing Carole Pateman's (1988) book *The Sexual Contract,* she notes that the liberal citizen has always had a specific identity—white, male, owner of property—and that the threat to liberalism comes from the refusal to recognize that he is both universal and masculine, both neutral and positive. That is, liberalism has been hostile to only some sorts of identity, and Hekman uses the insights of feminist theory to help explain how identity can be both public and private, both fixed and shifting, both important and distracting. She advocates a shift in practice from the politics of "I am" to the politics of "I want," recognizing that identities shift saliency with policy goals and partnership needs. Ultimately, she recognizes that identity politics is the usual way of doing business. But she urges us to recognize that marginalizing those "others" who claim nonhegemonic identities is no victory for liberalism, but antiliberal politics operating under a different name. Liberalism and identity politics can coexist for Hekman, because they share the same principles of "equality and justice for all as the cornerstone of political order" (p. 147). Identity politics does not kill liberalism but necessitates its transformation: "Differences between identities become the starting point of politics rather than that which must be eliminated" (p. 141).

Hekman's claims are well argued and persuasively presented in this well-written volume, which would be useful for advanced undergraduates, as well as for scholars of political theory less versed in feminism and feminist scholars new to liberal political thought. Particularly notable is Hekman's focus on the applications of her new conception of identity politics. Her concern is not for abstraction but for ideas about politics that can transform inequality, expand the pool of participants, and best effectuate justice for the marginalized.

Kenny also assesses the threat identity politics poses to liberalism, but his focus is on the integrity of liberal political theory. He describes a Manichean struggle between enlightenment universalism and multicultural particularism. In his description, the conflict takes on an all-or-nothing character, in which the assertion of identity claims threatens the liberal polity. Such identity claims are (erroneously, he says) too easily characterized as irrational claims to knowledge and political authority that would disrupt the public interest of the polity. Kenny presents a scrupulously detailed analysis of the many ways liberal thinkers have parried the advances of identity politics, including assertions that identity threatens democratic participation, saps citizenship, and inflames the benign ties of association. In response, he suggests that liberal values motivate the best alliances of identity and interest, and give rise to a more inclusive and democratic politics. The danger lies not in identity claims, which he shows have persisted through liberal thought, but in mistaken polarized notions of liberalism and the politics of identity.

At the same time, Kenny demonstrates that liberal theory can tolerate many more assertions of identity than have been claimed, so long as identity claims themselves are reconsidered. For example, he makes a distinction between ascriptive identity and voluntary identity, and concludes



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that the threat to liberalism comes from dominance, not difference. Throughout his argument, he focuses on the middle ground, on the points of agreement. He finds promise in an expanded notion of civil society—which would reflect the many associations of identity in which citizens might engage—by explicitly considering the political dimensions of social movements.

Kenny argues that exemplars of the politics of difference (he names William Connolly and Iris Young) fail to take seriously the implications of their particularist politics and neglect to see the elasticity of liberal thought. At the same time, those who demonstrate the politics of recognition (Charles Taylor, Nancy Fraser, James Tully) can provide a way to incorporate specific recognition of individuals and groups into the liberal tradition. The author applauds "the increased willingness of political theorists to consider culture and identity in relation to the self [which] reflects a growing understanding of the importance of psychological and interpersonal harms that can arise from the systemic marginalization and disrespecting of social collectivities in modern contexts" (pp. 172–73). He asks liberal thinkers to reconsider the utility and coherence of identity politics, and asserts that the tent of liberal theory has room for the particular claims of modern multicultural citizens.

*The Politics of Identity* is dense and best suited for advanced graduate students and those well versed in the liberal tradition. In contrast to Hekman, Kenny uses few examples from contemporary Anglophone politics. His arguments would have benefited from the judicious use of contemporary political and policy debates.

Both Hekman and Kenny find ways to reconcile identity claims and liberal virtues. Although their approaches differ, both recognize the importance of public recognition of assertions of identity and the fidelity to liberalism broadly understood. Identity matters because democratic participants say it matters. Equal moral status does not demand uniformity of identity. The politics of identity are not going to disappear quietly, and Hekman and Kenny offer two ways to manage their challenges.

**The Liberty of Strangers: Making the American Nation.** By Desmond King. New York: Oxford University Press, 2004. 229p. $29.95.

— John Francis Burke, *University of St. Thomas–Houston*

A half century ago, Louis Hartz articulated how extensively U.S. political culture was steeped in Lockian individualism, a contention subsequently echoed, in a more critical vein, by the seminal studies of Robert Bellah and Robert Putnam. However, in terms of nation building, Desmond King challenges the conventional wisdom that discrimination against individuals due to some group identity has been overcome so as to realize a nation that first and foremost celebrates "a formal equality of individual rights" (pp. 6–7). Instead, he contends that "group-based distinctions" (p. 7) have always characterized U.S. nationalism and will continue to do so despite political and theoretical rhetoric to the contrary.

Three key points comprise King's argument. First, the conventional "one-people" nationalism hides the fact that ethnic, national, and racial identities—some chosen and some imposed by the state—have persistently led to group divisions in the United States. Second, these long-standing divisions, which are continually renewed, thwart the complete realization of liberal individualism as the nation's identity. Finally, the ensuing tension between "group-based demands for democratic inclusion" (p. 168) and one-people nationalism has engendered a very strong state that has promulgated public policies, replete with group categories, designed to remedy past discrimination.

In the first half of his text, King reviews public policies and cultural practices since the U.S. Civil War that illustrate how inscribed group divisions have been in the pursuit of national identity: the "civilizing" of Native Americans through so-called Christian schools, the segregation of African Americans both through public policy and Garveyite separatism, the "civilizing" of Filipinos in the Spanish-American War, the restrictions placed on Chinese immigrants through labor laws and Japanese immigrants through World War II internment camps, and even delineations between Northern as opposed to Southern and Eastern European groups. King provides an eye-opening account of how in the late nineteenth and early twentieth century, a hierarchical categorization of races was part and parcel of the Progressive Movement.

In the second half of the text, King argues that as the United States expands its foreign policy presence in the twentieth century, the contradiction between its purported opposition to imperialism and colonialism and its domestic treatment of marginalized minority groups becomes quite glaring. Woodrow Wilson's "global liberalism" (p. 73), U.S. democratization of Japan post–World War II, and Eleanor Roosevelt's championing of the United Nations Declaration of Human Rights are each in tension with the reality of domestic group divisions. The subsequent recourse to remedies, such as affirmative action and revisions of immigration law, he adds, further underscores how intrinsic group categories are to the national psyche. Finally, he accents how domestic cultural groups increasingly lobby Washington on foreign policy issues connected to their native lands. Nevertheless, the myth of individualism, he reiterates, prevents realization of "a genuinely inclusive nationalism appreciative of the country's community of groups" (p. 171).

The strength of King's presentation is his countless examples of group categories in post–Civil War history, and therefore it is a valuable resource for historians and political scientists teaching courses on ethnic, racial, and multicultural relations. However, the author's theoretical



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contention that the individualist rendering of U.S. nationhood obfuscates how crucial group divisions have been to the construction of national identity is less persuasive, not because the argument lacks merit but because he does not fully substantiate his argument.

First, it is not clear from King's argument how much these group identities are the construction of public policymakers and how much they are the construction of the groups themselves. For instance, he contends: "American Indians paradoxically had to establish their distinct group identity as a basis for inclusion within the boundaries of the American nation" (p. 135). This connection between identity construction and terms of inclusion needs further clarification. In this instance, the link would be challenged by Native American separatists.

Second, King's claim that "the existence of group divisions is far more entrenched" (p. 163) in the United States than elsewhere is contrary to recent scholarship illustrating how intensely group rights are being debated both in Europe and in Canada. He counters he is articulating a "post-multiculturalism" (p. 168), but when one considers how cultural categories are explicitly incorporated into decision-making networks and public policies, for instance in Belgian, Dutch, and German cities (e.g., see Patrick Ireland, *Becoming Europe: Immigration, Integration, and the Welfare State,* 2004), one has to be a bit skeptical of King's claim, or again further clarification is needed.

In truth, such substantive shortcomings are actually stylistic: Too many of King's valuable ideas remain embedded in his reference notes. For instance, given his focus on group categories, how does his argument compare to Will Kymlicka's important works on multiculturalism that also seek to move beyond an abstract individualism? Although Kymlicka's work never appears in the narrative, King acknowledges it in the endnotes. Similarly, the work of Samuel Huntington, among others, suffers the same fate. Political theorists will find this to be exacting: Almost like a website, King's argument leads to crucial links that readers have to follow on their own to develop the argument. His narrative does not do justice to his extensive research and his provocative ideas.

Beyond this stylistic drawback, the author never engages the growing literature on the fluid intersection of cultures—hybridity, cultural hyphenation, syncretization, and transculturation. He does not sufficiently explore how in the United States, cultural groups are intersecting and transforming not just one another, but also the national identity, without necessarily culminating in assimilation. As much as he makes a valuable contribution by juxtaposing his narrative regarding group categories to the conventional one of individualism, his analysis needs to move beyond the individual-group dichotomy to engage the "border crossings" that are increasingly characterizing personal, cultural, and national identities.

In sum, King's scholarship challenges the hegemony of individualism in the construction of U.S. national identity. In turn, he now needs to subject his vivid articulation of group divisions to the same scrutiny from the stance of the lateral intersection of identities. In so doing, he would genuinely further the vision of "integration of groups" (p. 3) communicated by Lyndon Johnson in his famous 1965 Howard University address with which King begins *The Liberty of Strangers*.

**The People Themselves: Popular Constitutionalism and Judicial Review.** By Larry D. Kramer. New York: Oxford University Press, 2004. 363p. $29.95.

— George Thomas, *Williams College*

The provocative premise of this book is that "the people themselves," in James Madison's phrase, are the enforcers of constitutional boundaries and limits. In Larry Kramer's able hands, popular sovereignty is not an abstraction, where the people quickly fade from view and the Constitution they ratified is given life by courts and lawyers. On the contrary, Kramer seeks to document how the people themselves are not just active participants in the constitutional enterprise but the fundamental bearers of constitutionalism.

Kramer is at his best in the masterful opening chapters that illuminate the spirit of "popular constitutionalism." He deserves great praise for his detailed historical research, which recaptures the flavor of early constitutionalism and its deep connection with an active and spirited American people. He also deserves great praise for untangling the different conceptions of "constitution" floating around and rendering that understanding easily accessible to a modern audience. While "modern discourse has so thoroughly conflated the meaning of 'constitution' with 'law' and 'law' with 'courts' that we simply presume that the Supreme Court is the authoritative enforcer of the Constitution," it was not always so (p. 24). Thus, Kramer argues, the fundamental misconception of the modern insistence upon judicial supremacy "is the assumption not just that *someone* must have final authority to resolve routine constitutional conflicts, but that this someone must be a *governmental agency*" (p. 107).

Beginning with the notion of the "customary constitution," Kramer illustrates how fundamental law is best understood as "political-legal," distinguishing it from both ordinary law and ordinary politics, but binding upon the legislature nevertheless. Even prior to the notion of a written constitution, the language of "constitutional" and "unconstitutional" was invoked as a means of "maintaining liberty against arbitrary power" (p. 28). And demanding adherence to the "customary constitution" was something that came from the community itself by way of voting, petitioning, pamphleteering, jury nullification, and even "mobbing" (which has since been given a bad name). A great virtue of the author's argument is that it makes



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constitutional sense of so much activity within the age of revolutionary politics—like the Boston Tea Party.

Kramer then turns his attention to the development of judicial review within the confines of written constitutionalism. Even here, he contends, the notion of fundamental law enforced by "the people themselves" applies. The practice of judicial review must be understood within the contours of popular constitutionalism. Happily, this does not rehash the tired debates about the "problem" of judicial review and democracy that have so preoccupied constitutional theory, as if judicial review were the whole of American constitutionalism. Indeed, Kramer demonstrates how judicial review emerged as one aspect of enforcing fundamental law long before Chief Justice John Marshall's famous *Marbury* opinion. But most importantly, contrary to the modern understanding, judicial review was only one way of maintaining fundamental law, not *the* way.

This gives new resonance to Marshall's insistence that when the Court refused to abide by an unconstitutional law, it was simply adhering to the will of the people as expressed in fundamental law. Against the modern reading of *Marbury,* which equates judicial review with judicial supremacy, Kramer persuasively argues that it is best understood as a claim for the Court as a coequal interpreter of the Constitution. Nowhere does Marshall insist in *Marbury* that the other branches of government are bound by the Court's interpretation of the Constitution once it has spoken. On the contrary, Marshall noted, "courts, *as well as other departments,* are bound by that instrument."

Yes, political scientists have taken up many of these arguments (some of whom Kramer acknowledges in the text and footnotes). And one could certainly quibble with some of his readings of the Federalists and the Republicans. Still, in tracing out the changing context of constitutional law and its connection with party politics over the first half of the nineteenth century, Kramer deepens our understanding of constitutionalism in this era. Connecting popular constitutionalism with Andrew Jackson and Martin Van Buren against the Whig defense of judicial supremacy is one of the most interesting aspects of this work, as this era is often glossed over. By taking the story further into the nineteenth century than past scholars have done, he also illuminates the conceptual struggle to reduce fundamental law (as embraced by our written Constitution) to a more ordinary form of law.

However, while Kramer is right to reject judicial supremacy, I wonder if he does not overplay "popular constitutionalism" to the point of draining the written Constitution of any fixed meaning. He persuasively highlights Madison's departmentalist understanding and connects it, at root, to the people. But he seems to neglect the fact that Madison's departmentalism was a way to best *preserve* constitutional meaning. At times, it seems that Kramer thinks the Constitution could be *whatever* the people make it. I think this belies Madison's argument. If the people are, practically speaking, the final arbiters of constitutional meaning, there is an immediate distance between the people and their representatives by way of constitutional forms. And these forms, including the judiciary, are structured so that they might, when necessary, act against the "popular current." (This view might also go some way toward reconciling the tension Kramer sees between the "young" and "old" Madison on the scope of judicial power.) This does not mar the author's analysis of constitutional politics in the late eighteenth and early nineteenth centuries, but it does make his discussion of popular constitutionalism in the twentieth century wholly unpersuasive.

Here, Kramer attempts to play the progressive critique of constitutionalism and the New Deal's casting off of many constitutional limits as acts of popular constitutionalism. All too predictably, he offers up the "New Deal settlement" as the proper understanding of our constitutionalism: "While making their presence felt on questions of individual right, these Courts carefully respected the space carved out for popular constitutionalism at the time of the New Deal and left questions respecting the scope of national powers to the political process" (p. 220). Recent scholarship, for example, Ken Kersch's *Constructing Civil Liberties* (2004), makes this a very difficult sell. But even more importantly, Kramer ducks the hard questions of why the judicial protection of (some) rights is consistent with popular constitutionalism while judicial limitations on national power are not. Nor does he explain why Ronald Reagan's rejection of the New Deal settlement is not the most recent manifestation of popular constitutionalism. These latter chapters read far more like typical law school advocacy, falling short of the book's promise and making one wish that Kramer had stopped in the mid–nineteenth century. There, *The People Themselves* is a provocative and original analysis of American constitutionalism that will command a wide audience.

**Leo Strauss and the Politics of American Empire.**
By Anne Norton. New Haven: Yale University Press, 2004. 256p. $25.00 cloth, $16.00 paper.

— Barry Cooper, *The University of Calgary*

Anne Norton's book combines analysis of texts, which can be checked, with oral history or gossip, which cannot. Norton also shares her opinions and judgments about U.S. foreign policy. The mixed nature of the book makes for a difficult academic review.

The first paragraph establishes the author's credentials as a student of a student of Leo Strauss, a "political philosopher" (p. 5). She also knows "many Straussians and some of the students of Strauss" (p. ix). The distinction between Strauss and his pupils, and Straussians, is important; Norton repeats it several times. Straussians are *epigones,* which she takes to be "followers and toadies" associated



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with disagreeable forms of life—flies, fleas, and lice (p. 24). Even though ideological labels such as liberal and conservative (according to Strauss) conflict with the aspirations of political philosophy, Norton makes a plausible case that Strauss might be called a "conservative" in a vague Burkean sort of way. The neoconservative Straussians, in contrast, rejoice in being revolutionaries, big-government people without manners or courtesy. Animated by fear, they are captivated by the "allure of empire" (pp. 161–80).

In light of these remarks, I must note that I am not now, nor have I ever been, a Straussian or a pupil of Strauss or of his pupils. I confess to knowing several Straussians named in this book and count a few friends among their number.

Norton wrote the book "because I have debts to pay and ghosts to lay, and because I was made, somewhat against my will, the carrier of an oral history" (p. ix). She does not say to whom the debts were owed, though she does discuss the debts owed by Socrates and by Strauss (pp. 218–19). She does not describe the ghost nor indicate how she will lay it to rest. She indicates that she valued the "stories" she was told and the pleasure she took in being favored by her teachers to listen to them. Indeed, much of the book consists in fascinating tales. Other reviewers (Clifford Orwin, for example, in *The Claremont Review of Books* 5 [no. 4, Spring 2005]: 14–16) have disputed their truth, but that is another story.

Norton concludes with a discussion of Strauss and Abu Al-Nasr Al-Farabi and Baghdad, where Farabi taught. Because of the large U.S. garrison in Iraq and "because the Straussians are prominent among those who govern," this coincidence has great significance, which makes her recollection of her academic formation, including the stories, "no longer part of a curious personal history but elements of a common legacy." Finally, her remembrance of things past brought to her mind "the shapes of two futures" (p. xi). Thus, her intention is to combine: 1) a personal memoir, chiefly of graduate school, 2) an analysis of the later careers of some of her former classmates and teachers, and 3) the fate of the nation.

The argument Norton makes is that Strauss the political philosopher was betrayed by the Straussians who then turned America into a "moral battleground" (p. 2). The Straussians employ the language used by Strauss, but in fact misuse it because they have forgotten (or perhaps never knew) the Socratic questions at the heart of political philosophy. In place of the "pure and whole questioning" of Strauss, they have found "safer if more suspect certainties" (p. 76).

Some of the book is intellectual history. She discusses, for example, the personal and philosophical relationship between Strauss and Hannah Arendt, and the complex connection they both had to Martin Heidegger and Carl Schmitt, and especially the latter's "concept of the political." As many other studies have argued, their relations with Heidegger were personally and philosophically intense. With respect to a similar intensity regarding politics, the case is less easily made. According to Norton, Schmitt "shaped the term," namely "the political," for each of them and for their students (pp. 39–41). Unfortunately, there are no citations given to the appropriate texts and none of Arendt's students is named. In any case, the term was used by Hegel and is present in the distinction between *ta politika* (polis-affairs) and *to politikon* (the political) in Herodotus (VI, 57), Plato (*Laws*, 757c-e) and Aristotle (*Politics,* 1253b19).

Norton has critical remarks to make about Leon Kass (on biotechnology and bioethics), about Carnes Lord (concerning strategy), and about Tom Pangle (regarding his hiding his real meaning in plain sight). She also uses intense language to describe "Straussian truth-squads" and "intellectual brownshirts" (p. 45) roaming the halls of the University of Chicago intimidating cowardly professors. Whoever these unnamed bullies were, it sounds serious. It turns out, however, that they were only "mocking behavioralists" and rejecting "the claims of science to ethical neutrality" (p. 46).

The author's sharpest criticism is directed at Allan Bloom, who "taught both the most powerful and the most vociferously ideological of the Straussians," and at his famous 1987 book, *The Closing of the American Mind* (pp. 58 ff). Again, some remarks are intellectually engaging—her criticism of Bloom's interpretation of Flaubert's *Madame Bovary,* for example (pp. 65–66). Her attribution of motives to Bloom for writing *Closing,* chiefly *ressentiment,* are less defensible. She says nothing about his translations of Plato and Rousseau or of his time in Toronto.

When Norton turns her attention to the conduct of U.S. foreign policy, which she understands as analogous to the Sicilian Expedition described by Thucydides, her confident judgments are both dated and intrinsically questionable. She tries, for instance, to turn the Straussian neoconservatives into a mirror image of the jihadist terrorists by drawing on alleged similarities between Strauss's reservations about liberal democracy and the opinions of Sayyid Qtub (pp. 109–13). She thinks that the capacity of Americans to endure violence "has gone below zero" (p. 157). The fighting in Afghanistan and Iraq indicates otherwise.

*Leo Strauss and the Politics of American Empire* is not a book without humor, however. Norton's description of the Last Man—or rather, of the Last Kids and the Last Dog—is quite funny (p. 124). Her view of Churchill restores balance to the uncritical way that many Americans view him. On the other hand, her discussion of the debate between Strauss and Alexandre Kojève (pp. 147–48), which seems to imply agreement regarding the Universal and Homogeneous State, is simply wrong. In short, like most books, this one is uneven. Unlike most uneven books, however, its exasperating features are unredeemed by significant insights.



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**Just Marriage.** By Mary Lyndon Shanley, with Commentators. New York: Oxford University Press, 2004. 116p. $45.00 cloth, $12.95 paper.

— Linda Gordon, *New York University*

Should marriage as we know it be abolished? This is one proposed solution to the injustices of marriage today: unjust to those excluded from access to it, to those who might like it but cannot find a partner, and to those who choose other forms of relationships or even consider marriage oppressive. Or does marriage remain an institution so bound up with social needs, such as child raising and care for dependents, that the state cannot abdicate regulatory authority over it? Mary Lyndon Shanley argues the latter position.

The title is a pun. Shanley is concerned with making marriage just, but she is also responding to some who think that marriage is just one relationship among numerous possible relationships—none of which should be the concern of the state.

*Just Marriage* is one of a series of *very* small books, the New Democracy Forum, published by Oxford University Press and the *Boston Review.* Each consists of one essay with numerous *very* short responses. Obviously, short is the point here, to produce and distribute a quick read on issues of importance and contemporary controversy, such as standards in public education, tolerance in Islam, or multiculturalism.

In this volume, political scientist Shanley offers a thoughtful essay arguing "the public importance of private unions," then listens to 13 commentators, and finally concludes, remarkably, by altering her position somewhat. Thus, the book is closer to a real conversation than many other anthologies that make such a claim. It may be too short, however. (Shanley's essay is approximately 6,500 words, less than half the length of a typical article in a humanities journal, and the responses are often only three pages long.) None of these essays can flesh out an argument; the conversation is barely begun, and so it may leave the reader—as it did me—feeling like there was not enough for dinner. But I came to think that the extreme brevity might be useful if it generates further discussion. It is not a bad thing to go away hungry for more.

Shanley rehearses sharply and concisely the view of marriage that came to the United States from British (Anglican) tradition: as a covenant and a hierarchical relation in which the couple became one flesh with the father/husband as the head—and, of course, the political representative of the whole family. She explains with the clarity of a fine teacher that marriage was a public institution; despite whatever individual inclinations or agreements might arise, the marriage had to follow terms set by the state. Spouses could not, therefore, divorce because the relationship no longer pleased them, but only if one of them committed an act against the law of marriage. Divorce in those days typically meant lying and even getting friends to lie on one's behalf. And in a consequence that my students usually find loony, if both spouses wanted the divorce, that preference would ipso facto prevent the divorce.

Several generations of feminist activism have shorn marriage of many characteristics that were once part of its definition. You can get a divorce without lying; married women have rights; courts try, however clumsily, to defend children's interests; and, in some instances, nonmarital partners can get some of the protections marriage provides. But as so often happens in campaigns for social justice, the gains only make the continuing injustices stand out.

One theoretical remedy would disestablish marriage, making of it no more than a private contract. Shanley argues that this would be a mistake: "Marriage partners are not only autonomous decision makers; they are . . . social beings who will inevitably experience need, change, and dependency" (p. 15). The public thus has an interest in promoting family relationships. Most provocatively, Shanley argues that marriage creates something more than two individuals in partnership. Long-term commitments shape the self and promise mutual responsibility in all sorts of unforeseen circumstances: The commitment is "unpredictable and open ended, and the obligations . . . cannot be" anticipated (p. 28).

This claim is fascinating but, as commentator Nancy Cott points out, Shanley has not spelled out exactly what the public gains from marriage. I would ask, is marriage mainly a system of protecting the polity from higher welfare costs? Exactly how is the individual affected by marriage, and does Shanley's case rest on the claim that those in long-term relationships are somehow better people? Dubious. But surely we cannot discard the feminist and communitarian insight that contract theory assumes an autonomous individual and fails to honor the social nature of human beings, as commentator David B. Cruz points out. Marriage, I would add, could never be a relationship free of social, cultural, and economic norms and inequalities. But is this not true of all contracts? Thus, in trying to conceptualize just marriage, we must face up to the critique of liberalism altogether. As commentator William N. Eskridge, Jr., points out, "the liberal conception of self . . . is not . . . a persuasive basis for thinking about human happiness and well-being" (p. 58).

Of course, marriage creates something more than placing its members in a legal agreement, but so do all relationships—friendship, teacher/student, boss/employee, team, performance group, co-workers, and so on. And we could hardly abolish all individual agreements on the grounds that there are no individuals; this would be postmodern thinking stretched to absurdity.

Shanley read the criticisms of her view and responded. When she first wrote, her goal was to "reclaim the discussion of civil marriage from the grip of the far right and affirm the value of public support for personal commitment" (p. 111). Her concluding view is that the United



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States should do away with the civil category of marriage and let "marriage" mean unions sanctified by religious ceremonies. The law should recognize all kinds of civil unions. But, as she would agree, many questions remain. Should polygamous relations be recognized as civil unions? Mother–child families? Long-term roommates? Communes? And should any partner to a relationship be able to leave at will without any continuing responsibilities, economic or caretaking?

This little book also offers an object lesson in how historical developments refuse to let such discussions remain theoretical. Shanley was not originally focusing on gay marriage, but now the gay marriage skirmishes make marriage reform talk take on urgency. Some are excluded from its benefits here and now, and they are understandably eager to fix this wrong. If it is to be fixed, chances are that any reform will happen piecemeal, inconsistently, and without consensus. The future of marriage will rest more on the balance of political activism than on intellectual debate. Meanwhile, this small book offers to activists and nonactivists a fine introduction, and provocation, to serious debate.

**The Nature of Political Theory.** By Andrew Vincent. Oxford: Oxford University Press, 2004. 354p. $65.00.

— Leslie Paul Thiele, *University of Florida*

Survey texts exercise considerable violence upon the thinkers and works they assess. Andrew Vincent is no pacifist. Yet his book succeeds admirably in gaining breadth without unduly sacrificing depth and subtlety. Vincent's encyclopedic knowledge (with more than 500 titles in the bibliography) is coupled with a sharp critical eye and refreshingly blunt appraisals. The result is a thoughtful and wide-ranging interpretation of twentieth-century Western political thought.

Political theory grounded upon a (retrospectively imposed) canon of thinkers and texts did not gain disciplinary status until the mid–twentieth century, with its first independent journals appearing in the 1970s. The institutionalization of theory quickly produced a coterie of professionals speaking to one another in specialized languages. What perpetuates the discipline these days, Vincent suggests, are not political concerns that demand urgent resolution, but "immensely powerful institutional, career, and professional interests" that bear the "dubious patina of political engagement" (p. 27). His deprecation of the discipline's self-enclosure parallels John Gunnell's worries about the alienation of political theory from politics (e.g., *The Descent of Political Theory,* 1993; *The Orders of Discourse,* 1998). Ironically and, perhaps, consistently, Vincent himself never links developments in the discipline to changes in the world outside the ivory tower, with the exception of a brief reflection on the "background reasons" for the rise of interest in pluralism. In this respect, *The Nature of Political Theory* merely gestures at the worldly reflections that it chastises theorists for neglecting.

Vincent focuses on political theory's preoccupation with "foundations," the (quasi) metaphysical realities that are meant to supply objective standards for moral and political judgment. By and large, twentieth-century theorists have rejected thick, comprehensive, universal foundations, opting instead for thinner, weaker means of support. While Vincent ignores earlier efforts to grapple with this issue, such as Don Herzog's *Without Foundations* (1985) or Stephen K. White's *Sustaining Affirmation* (2000), he does provide a stimulating assessment of the field's foundational angst.

Vincent adeptly summarizes the discipline's efforts to secure minimalist foundations. He examines, for example, the historical foundations sought by Leo Strauss, Quentin Skinner, and J. G. A. Pocock; the empirical or analytical foundations sought by David Easton, other behavioralists, and rational choice theorists; the neo-Kantian foundations sought by John Rawls and Brian Barry; the neo-Aristotelian, conventional, narrative, or experiential foundations sought by Michael Oakeshott, Alasdair MacIntyre, Michael Walzer, and Charles Taylor; and the linguistic (non)foundations sought by neo-Wittgensteinian, Nietzschean, or Heideggerian thinkers, such as Peter Winch, Richard Rorty, Jean-François Lyotard, Jacques Derrida, Michel Foucault, and William Connolly.

Vincent concludes his survey with a plea for theorists to reexamine the benefits of minimalist dialogic foundations, as exemplified in the work of Hans-Georg Gadamer. While sympathetic to Jürgen Habermas's discourse ethics, Vincent believes that Gadamerian hermeneutics offers the best rebuttal to the postmodernists' outright rejection of foundations.

Gadamer's "hermeneutic circle" exposes the means by which our fore-conceptions interact with our reflective deliberations to allow informed and self-conscious understanding. We cannot and should not hope to escape the particularity of our being, as we are largely constituted by the prejudices that ground us historically and socially, including the "prejudice *for* reason" that animates so much contemporary political theory (p. 300). Fore-conceptions allow access to the world and facilitate the process of dialogic interaction. Opposing reflective reason to prejudice bestows the former with a "false power." Rather than pursue a Habermasian quest for the universal pragmatics of reasoned speech, Vincent suggests, political theorists would do better by engaging in hermeneutic efforts to bring their fore-conceptions to the foreground.

To experience something hermeneutically is to have prejudices revealed and challenged. This is an interminable process. Yet Vincent argues that it would be wrong to think of hermeneutics as relativist or postmodern. To be



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sure, there are for Gadamer no ethical or political absolutes. There is no Archimedian point from which one might gain access to pristine truths, uncontaminated by linguistically mediated understanding. Still, hermeneuticists seek a fusion of horizons, a commensurability of concerns if not a consensus in resolutions. As such, they cultivate an ethic of psychological and moral growth.

At the end of the day, psychological and moral maturity amounts to a form of Socratic wisdom—a knowing that one knows nothing. Just as Socrates sought to test and reconfirm his wisdom by venturing into the agora each day, so hermeneutic engagement with texts and people affords endless opportunities to foreground one's own (and others') prejudices. While firm foundations can never be secured, the human desire to seek them is understood to be unquenchable. The wise person (and good theorist), while tempering expectations, never abandons the search for truth.

The psychological and moral maturity that develops from hermeneutic exercise is identified by Vincent as practical wisdom. Practical wisdom is not "rational certainty, fixity of purpose, and decisive proof" (p. 322), but rather the skillful navigation of an ambiguous, contingent, and dynamic world. Here, he might well have explored why Gadamer, having attended Heidegger's lectures on Aristotle, identified practical wisdom as a hermeneutic virtue.

Pitting Gadamer against Habermas, Vincent argues that hermeneutics militates against the "hubris of universalism" (p. 323). But Vincent does not address how and why practical wisdom, while inherently embedded in language, is not reducible to it. There are tacit and affective dimensions to practical wisdom (recognized by Aristotle as habit and desire) that have extralinguistic qualities and foundations. Vincent, like most political theorists, is enamored by cognition. Thus, he ignores extralinguistic qualities and foundations. Engaging in dialogue with those who shed light on these extralinguistic dimensions of practical judgment would go some ways to helping political theory escape its self-enclosure.

**Politics and Passion: Toward a More Egalitarian Liberalism.** By Michael Walzer. New Haven: Yale University Press, 2004. 208p. $25.00.

— Stanley C. Brubaker, *Colgate University*

With characteristic elegance, clarity, and nuance, Michael Walzer here refines and extends the thesis he first ventured 15 years ago that the fragmentary effects of liberalism require periodic communitarian correction. Nearly all of the chapters are revised from previous articles or book chapters composed in the years intervening, and so their relation is more thematic than structural. But their collective force is considerable.

Against the contract tradition of autonomous individuals freely choosing their associations, Walzer develops the extent to which our identity is constituted, inevitably and often beneficently, by associations that are entirely, primarily, or initially involuntary—gender, family, race, ethnicity, religion, and culture. Against interventionist liberals, such as Stephen Macedo, who seek to instill liberal values pervasively in civil society, Walzer defends toleration and even cautious subsidization of illiberal "totalizing communities," such as fundamentalist and ultraorthodox religious groups or indigenous ethnic groups; only when their practices "radically curtail" the life chances of their members or "limit the responsibilities of citizenship" should the state intervene (pp. 85–86). Against theorists of "deliberative democracy," such as Amy Gutmann and Dennis Thompson, Walzer expands our appreciation of the range of actions that legitimately shape public policy (and in which actual deliberation forms only an incidental role); extends reciprocity not only to individuals who are "rational in exactly the same way we are" but also to "members of groups with beliefs and interests that mean as much to them as our beliefs and interests mean to us" (p. 104); and embraces passion (especially for recognition in the politics of identity) as not only a powerful but also a legitimate aspect of political participation.

This communitarian corrective was his first motive, but in writing *Politics and Passion,* Walzer reports, he became "increasingly engrossed" with the implications of these correctives for the "struggle against inequality" (p. xi) and the recognition that not all "communities" are egalitarian. Thus, he considers the involuntary associations not only in the way they shape our understandings of the good life and the blessings of liberty, but also in how their internal structures and their power alignments with each other implicate equality. He balances his tolerance of totalizing communities with a concern that their members become capable citizens in the larger body politic. In making room for the passionate indignation of the pariah groups—their desire "not to be dominated"—he recognizes that he also risks engaging the "desire to dominate" (p. 122). Communitarian and multicultural sympathies, he recognizes, do not always mesh easily with social democracy. In the one chapter freshly composed for this book, "The Collectivism of the Powerless," Walzer provides his most sustained effort to synthesize these sometimes competing commitments. In a society with differential power among its ethnic or cultural groups, such as our own, the only effective way to achieve equality is through group empowerment of the weak or stigmatized—possible, he argues, only with assistance from the state—enabling them to provide their members with life-cycle services (child care, education, health, retirement) and rite-of-passage celebrations. Thus, we should aspire to a political system with power radically dispersed not only through separation of powers and federalism, but also through a multitude of ethnic and other groupings providing life cycle services—what he calls, "meat and potatoes multiculturalism"



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(p. 38)—with the ideal result that individual citizens will hold "identical 1/n shares" of political power. This, he argues, is "a picture to be admired and a plan that we should try to realize" (pp. 42–43).

Walzer writes with such warm sympathies to his various interlocutors—multiculturalists, liberals, social democrats—that it is sometimes difficult to determine just where he stands. Except that he faces Left. To the Right, he turns his back. This is unfortunate on several grounds. It is unfortunate for political practice because he fails to explore the areas where there may be common ground. Why not the "faith-based initiative" to assist in the provision of life-cycle services? Why not school vouchers to empower groups that wish to pursue a distinctive curriculum? Why not a "thousand points of light" to spur local initiatives? It is unfortunate empirically, for many of his claims are mere assertions. Is it really true that disadvantaged racial and ethnic groups can only progress through state-sponsored, race- and ethnic-conscious programs? The vast work of Thomas Sowell receives no notice. Should we really regard the Wagner Act and the closed shop (repeatedly invoked, pp. 78, 97, 134, 157, 168 n. 9) as the model for state-assisted group empowerment? The decline of the unionized North, the rise of the nonunionized South and Southwest, the relative impact on wages of unions versus growing productivity spurred by competitive enterprises, the impact of unions on unemployment and outsourcing of jobs—none of these concerns is explored. And it is unfortunate for political philosophy, as he never makes the comparative case for his vision of what equality requires. Why should we prefer a polity where each has "1/n" of power to one in which all of us would be better off, but some more so than others? Why should the Rousseauian vision of each citizen with "1/n" of political power be our dream rather than our nightmare? Rousseau after all argues that a polity approximating this ideal would have to be molded by a singular Lawgiver, fostered with rigorous attention to education and mores by censors, and preserved through sworn belief to a civil religion. Identical equal shares of power among the citizens can only be maintained with powerful central authority, vigilantly redistributing power whenever one citizen gets more than his or her allotted share. Why not equality of natural rights, the equality of each of us, as Lincoln said, to eat the bread that his hand earns? Why not equality of opportunity, which allows excellence to flourish without regard to race, religion, or ethnicity?

All of this neglect is unfortunate, but not surprising, for in the diverse and multicultural universe of higher education, conservatives are probably the least empowered.

## AMERICAN POLITICS

**Governing NOW: Grassroots Activism in the National Organization for Women.** By Maryann Barakso. Ithaca, NY: Cornell University Press, 2004. 192p. $49.95 cloth, $18.95 paper.

— Maureen Hogan Casamayou, *George Mason University*

This is a case study of the National Organization for Women that explains how NOW's governance structure shaped the organization's strategy from 1966 to 2003. "Strategy" is defined as NOW's "goals and tactics it uses to pursue them" (p. 2). The author contends that current conceptual approaches to explaining organizational behavior, such as the availability of resources, or political opportunities, or "collective belief systems," can fall short in their predictive powers. There are times, for example, when organizations appear to be irrational and self-destructive as they ignore "obvious political opportunities" or invest "resources inefficiently" (p. 6). Analyzing organizational behavior from the perspective of its "governance structure" or "political system," however, "resolves many of these apparent inconsistencies" (p. 6). Governance structure is "simply a political system described by a group's guiding principles (its goals and values) on the one hand and its formal decision-making process on the other" (p. 1).

According to Maryann Barakso, this approach, too long neglected by scholars, explains NOW's long-term deference to its grassroots base, its commitment to a feminist left-of-center multiissue agenda and tactics (irrespective of scarce resources or receptivity of political climate). During its early history, the organization recruited from the consciousness-raising groups of the women's liberation movement. These "true believers" of feminist principles believed in grassroots empowerment and upheld an "anti-elitist ethic" (p. 28). Most important, they succeeded in bringing about a governance structure at NOW that was not only "highly representative and participatory" but also dedicated to the organization's "guiding principles (goals and values)." These goals and values are embedded in the following five statements or principles: "1) to remain in the vanguard of the women's movement; 2) to be an activist rather than an educational group; 3) to maintain political independence from governmental and political institutions; 4) to focus on a diverse set of issues and tactics; and 5) to mobilize activists at the grass roots" (p. 2).

As guardians of these guiding principles, the rank and file manipulate the highly democratic processes of the organization's decision-making structure. This explains the seemingly irrational decision by the national leadership to push for not one but several causes (the equal rights amendment, lesbian and abortion rights) during NOW's start-up



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years (between 1966 and 1971) when resources were scarce and the organization was relatively obscure.

Of course, this is not without the tensions associated with keeping the leadership in line, or at the very least, "sensitive" to the concerns of the rank and file. When NOW's president, Eleanor Smeal, made ratification of the ERA top priority and declared a "State of Emergency," for example, many members were uneasy with the creation of a political action committee for ousting recalcitrant state legislators in unratified states in the 1974 elections (p. 69). Smacking too much of establishment politics, such tactics, some feared, could undermine the guiding principles of the organization in terms of NOW's commitment to grassroots activism, to being the vanguard of feminism, and to being an outsider to establishment politics. Accordingly, the national organization went to great pains to reassure the members that this was purely a temporary device and would be dissolved after the ERA campaign was concluded. The members were also informed that the governance structure of such a committee would mirror that of NOW (p. 64).

In terms of the book's organization, the author devotes two chapters on the formation of this governance structure, which produced a left-of-center bias in both issue choices and use of political tactics. The four subsequent chapters cover the history of NOW and the impact of its governance structure on the leaderships' agenda and policy positions. Each chapter covers one of four major periods, namely, 1966–71, 1972–78, 1978–82, and 1983–2003.

One important strength of this work (that builds on other studies) is the manner in which the author shows how representation and participation in the organization's democratic processes by the rank and file strengthens our own democracy through their newly acquired civic skills and education. These are transferable to other settings (in other organizations or public forums) involving deliberation and decision making, yet the author did not explore the ironies of this. Acknowledging the constraints of a case study, one may speculate, for example, that using these skills in public forums may actually weaken the ideological resolve of the doctrinaire rank and file as they gain entry (through their skills) into the pragmatic and compromise-oriented milieu of the political establishment.

Another important finding of this case study is the paradox of having an interest group organization that is internally very democratic but externally politically weak. Really, it is the left-of-center ideology that constrains the leadership through the governance structure. This retains the core loyalists but does not build up the membership and subsequent resources.

Last but not least, defining certain key concepts would have greatly sharpened the clarity of her arguments and anchored the work. For example, the power distribution of the NOW organization is not clearly enunciated and categorized. Is it a confederative design, or what? Moreover, in the introduction, the term *governance* is described but not defined. The same can be said for *political system.* Equally important, the author does not make it clear at the outset whether NOW is categorized as a social movement organization (SMO) or an interest group. On page 51, it would appear that she is saying that by 1972, NOW had evolved from an SMO to an interest group, but she does this without making any attempt at drawing a distinction. Finally, the five critical guiding principles (goals and values) of the organization are not defined and elaborated upon. This would have been very helpful to the reader since they drive the four chapters on NOW's strategic history.

**The Illusion of Public Opinion: Fact and Artifact in American Public Opinion Polls.** By George F. Bishop. Lanham, MD: Rowman & Littlefield Publishers, 2004. 223p. $80.00 cloth, $27.95 paper.

— Adam J. Berinsky, *Massachusetts Institute of Technology*

With another campaign season behind us, it is not hard to make the case that opinion polls pervade politics. Even casual observers of politics are inundated with polling results from media and academic organizations. But just because surveys are ubiquitous does not mean they are useful. In his book, George Bishop makes the provocative argument that just about every bit of evidence that academics, journalists, and policymakers draw from opinion polls is erroneous. This argument will be controversial—indeed this book is clearly intended to provoke controversy—but the force of evidence that Bishop presents demands to be taken seriously by even the staunchest defenders of opinion polls.

Social scientists have long known that the particular wording and order of questions influence the responses individuals give to survey questions. Bishop's review of this evidence is thorough, but he has bigger fish to fry. He argues that the current practices of academic and political pollsters essentially force individuals to "make up" answers to survey questions on the basis of vague understandings of pertinent political controversies. Low levels of political knowledge and ambiguous questions combine to produce survey marginals that reflect illusionary measures of the public will. As a result, Bishop argues, political polls—at least in their current form—are not useful tools for understanding how the public feels about specific political issues.

The author draws upon a large body of data to make his case, ranging from measuring trust in government to approval for stem cell research. This evidence is largely persuasive. Consider, for instance, public opinion concerning the teaching of evolution. A number of recent polls have found that Americans favor by a large margin the teaching of creationism along with evolution in public



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schools. However, the majority of Americans are completely unfamiliar with the meaning of the term "creationism." What, then, should we make of the survey findings? Bishop presents question-wording experiments that show that respondents are guided by their general feelings toward religion in public school when they answer the survey question. Thus, he argues, the widely cited polls on creationism do not measure specific attitudes toward the instruction of evolution. Rather, they capture the general desire of the mass public to find a middle way on the question of the separation of church and state.

Given the findings Bishop presents, it would be easy to make the leap that opinion polls are incapable of transmitting meaningful information from the mass public to political elites and should be rejected out of hand. But he argues that much of the problem with polls arises not from the survey method but from the manner in which they are currently conducted by media and academic pollsters. He therefore devotes the final chapter of his book to describing a number of measures that could be taken to improve the information gleaned through polls, including probing the meaning of responses to specific questions, attempting to gauge the strength with which citizens hold particular opinions, and measuring the levels of policy-specific knowledge that undergird the answers people give to pollsters.

While there is much to admire in *The Illusion of Public Opinion,* there are some problems as well. The book makes an important argument, but Bishop sometimes takes his argument too far. At times, the later chapters border on polemic. For instance, Bishop makes a critical point about exerting caution when interpreting opinion polls on specific issues. These polls, as he rightfully points out, may in fact measure general sentiment on political issues. But scholars and practitioners should not lose sight of the fact that this general sentiment matters in politics. For instance, take the creationism example. The author makes much of the fact that citizens do not have a firm grasp of the creationism controversy (and do not even know what "creationism" means). But this lack of knowledge does not render information gleaned from polls meaningless. The polls on the teaching of creationism tell us that citizens seek a middle ground on the place of religion in public schools. This result is important, both for students of mass behavior and for politicians who make education policy. The bottom line is that while individual polls may be problematic, a consistent message across different polls—here, the desire to find a place for religion in public schools—can be an important and powerful signal for policymakers.

Another difficulty with the book is that Bishop's discussion sometimes divorces polls from their political implications. He catalogs a number of cases where the specific meanings of particular questions have changed over time, including some of the most venerable survey questions, such as presidential approval, trust in government, and ideological attachments. For instance, he notes that the correlation between party identification and liberal/conservative identification has increased notably since the early 1970s; Democrats are more likely to say they are "liberals" and Republicans are more likely to identify as "conservative." Bishop argues that this increased correlation is not evidence of increased ideological sophistication on the part of the mass public. Rather, he makes the case that the tightened association between these two concepts is an artifact, the result of "a simple increase in the sheer frequency with which the terms "Democrat" and "liberal" and "Republican" and "conservative" have been associated with each other in the mass media" (p. 124). I would argue instead that the increased association of ideology and partisanship represents a meaningful change in how ordinary citizens approach political controversies in this country. Recent work by Nolan McCarty, Howard Rosenthal, and Keith Poole demonstrates that political elites have become more polarized since the 1970s; Democrats and Republicans in Congress have moved toward the ideological extremes. Partisanship and ideology are linked in public discourse because the political environment in America has changed. Thus, what Bishop labels an artifact seems instead to be the public consequence of a change in the political system, and one with serious implications for the practice of politics in this country.

Finally, Bishop's discussion of interpersonal comparability of survey responses illustrates both the strengths and weaknesses of this book. If the same question means different things to different people, it is difficult to make sense of aggregate poll results. Two citizens might give the same survey response but mean different things in doing so. This is an important point, to be sure. But like other arguments in this book, Bishop sometimes takes this point to its extreme. For instance, he demonstrates that measures of trust in government are determined in large part by individuals' assessment of the president. Those who approve of the president are more trusting than those who disapprove of the incumbent. Thus, "trust in government" means different things to supporters and opponents of the administration. The evidence is clear on this point. However, an examination of the data over the last 30 years reveals that the gap in trust between supporters and opponents of the president is fairly steady—on the order of 20 or so percentage points. Among the two groups, the familiar over-time trends—the drop in trust in the early 1970s, the resurgence under Reagan—are evident. Thus, even though mean rates of support for supporters and opponents of the president might be different, the over-time changes in levels of trust are similar. Politicians and political scientists can learn a great deal about political behavior from studying these trends.

These criticisms aside, *The Illusion of Public Opinion* is an important book that should be read by students of



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mass political behavior as well as practitioners. Bishop has thrown down the gauntlet, providing a broad and provocative review of the current state of opinion polling. Even those who reject his conclusions must engage his argument.

**Common Waters, Diverging Streams: Linking Institutions and Water Management in Arizona, California, and Colorado.** By William Blomquist, Edella Schlager, and Tanya Heikkila. Washington, D.C.: Resources for the Future, 2004. 210p. $70.00 cloth, $30.95 paper.

— Jeffrey S. Ashley, *Eastern Illinois University*

Water supply and allocation is a problem that has plagued the West for most of its history. Water marketing, a greater emphasis on conservation, and surface water augmentation projects are all methods that have been used to address the problem. Another approach that has been around for quite some time but has been pushed to the forefront in recent years is the idea of conjunctive use management. In its broadest sense, conjunctive use management is simply linking surface and groundwater together and managing them as a common water supply pool. Rather than having one set of groundwater users and one set of surface water users, all are bound together by the need for more water. By doing so, excess surface water can be stored beneath the ground during wet times, and these same users can draw on the underground supply during times of drought. Similarly, the storage will increase the amount of water available underground so that overdraft conditions are diminished. In theory, this means that there will be an overall gain in the amount of water available for all users and that supplies can be managed on a more sustainable basis. In practice, Wiliam Blomquist, Edella Schlager, and Tanya Heikkila point out, existing institutions play a major role in determining the push for conjunctive use management and the form that it takes.

The emphasis on institutions is the focus of the book that makes this work appealing to a broad range of political scientists. While conjunctive use is the central theme of *Common Water, Diverging Streams,* water policy ends up being simply a vehicle that is used to explain public policy and public administration, and the role that institutions play in both.

The book is structured in three parts—"Common Waters: Managing Surface Water and Groundwater Resources Together," "How Institutions Matter: Institutions and Conjunctive Management in California, Arizona, and Colorado," and "Institutions and Policy Change: Analysis and Recommendations." The strength of the book is found in the first two parts. In Part I, the authors do an outstanding job of describing the whole practice of conjunctive use management—from the different forms it might take to a range of alternatives for storing water below ground. Given the geology of a given area, water storage may not be as simple as allowing water to wash over an aquifer with the expectation that the water will sink in. Conjunctive management often requires expensive items, such as injection wells, to move surface water underground. They also point out a number of obstacles that may hinder states in their efforts to engage in conjunctive use management. The obstacles include, but are not limited to, existing water law, a lack of statewide leadership, and a lack of trust among water organizations (p. 14).

Part II centers around three case studies in conjunctive use and the relationship between institutions and management. In their discussion of California, Arizona, and Colorado, the authors are very thorough in their treatment of each state's physical setting, current institutional arrangements (including surface and groundwater law), and the emergence of conjunctive use. They are convincing in their demonstration of how the institutional arrangements largely dictated the shape and form that conjunctive use took in each particular state, which reinforces the book's primary claim. That said, the book does have some weaknesses.

One is left to wonder why Blomquist, Schlager, and Heikkila chose to focus on these three states to the exclusion of other western states engaged in conjunctive use. The authors claim that "without question, differences among the states result partly from physical and historical distinctions, but to a greater degree they illustrate how institutions matter" (p. 115). While they do a wonderful job of demonstrating how institutions have played a role in each individual state, for example the leverage wielded by the Central Arizona Water Conservation District, their use of only three states does not allow them to make the interstate connections that they do. While the authors do attempt to discuss methodology and state selection in an appendix entitled "The Three States: Why We Chose Them and What We Did," the add-on comes across as an attempt at pleasing press reviewers, rather than something that truly gets to the heart of the matter—a wider range of states may have led to different findings. There is simply nothing to explain why other states, such as Nevada, which is facing even greater growth with similar water problems, are not included in the study.

Another weakness is that not enough emphasis is placed on politics and entrenched interests. While such institutions as laws, existing policies, and agencies/organizations clearly matter, so too do the interests that often drive these institutions. Laws, policies, and even organizations can change if external pressure is strong enough. The authors themselves discuss the decentralized nature of water management as being a tradition in California (pp. 53, 64, 67, 72). It is the entrenched local interests and those who benefit from the system that continue to push for this institutional arrangement—if enough new users were to push for a more centralized approach, it could happen. In fact, the entire push for conjunctive use has come about through the need to appease a wider number of water



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users while sustaining the resource. The treatment of institutions as static and unable to change is a problem.

Despite the limitations found in the three-state study and the treatment of institutions as being unable to change and adapt, *Common Waters, Diverging Streams* does make a significant contribution and deserves a positive recommendation on two fronts. Conjunctive use is going to be an important part of western water management for some time to come, and this work serves as a fine resource for anyone who wishes to understand how conjunctive management works. In addition, the way the three states are handled individually provides a good look at how institutions play a part in shaping policies and at how these policies are implemented—making the work a solid resource for anyone wishing to explore the complexities of public policy and management through the lens of western water management.

**Terra Incognita: Vacant Land and Urban Strategies.**
By Ann O'M. Bowman and Michael A. Pagano. Washington, DC: Georgetown University Press, 2004. 232 pages. $24.95.

— David Luberoff, *Kennedy School of Government, Harvard University*

Public policies for vacant land are among the least understood but most important aspects of urban policy and politics. In this book, Ann O'M. Bowman and Michael Pagano highlight important issues regarding such land and offer useful frameworks for understanding the policies and politics that shape its future uses. In doing so, they provide a unique and useful perspective on land use policies and politics, which are a key—if not the key—aspect of local policy and politics.

The subject merits special attention because land plays a unique role in local politics. Unable to regulate flows of either capital or labor and fiscally reliant in part on locally generated tax revenues, local governments in the United States are compelled to pay close attention to decisions that affect land use. As a result, in both ailing and growing regions, land-use policies are at the heart of local government.

The nature of those policies and politics varies greatly. When private investors are not proposing new projects, local policymakers and landowners often seek to stimulate development via such policies as tax abatements, land assembly, targeted infrastructure investments, publicly funded projects (such as convention centers), and public support for privately developed projects (such as festival marketplaces). In these arenas, the key issue is whether the decisions about public investments (including the decision to forgo tax revenues) benefit the community as a whole or whether they primarily represent narrow rent-seeking behavior by small, well-mobilized constituencies.

Conversely, localities attractive to private investors often use regulatory powers, such as zoning (and annexation of unincorporated land) to control the timing, scale, and character of new development. In these communities, the key issue is whether developers or homeowners have captured the regulatory system to the detriment of all of a region's residents.

Vacant land is particularly important in both ailing and growing communities because the most significant conflicts about land-use policies generally involve efforts to change the use of a particular parcel of land from one productive endeavor—such as farming or low-density housing—to a use likely to generate more money for the parcel's owner—such as higher-density commercial or residential developments. If, however, the parcels in question are seen as "vacant," they will be easier to develop because, as Bowman and Pagano point out, "the phrase 'vacant land' tends to evoke negative images: abandonment, decay, emptiness, and, in some instances, even danger" (p. 1). Moreover, the authors, who surveyed planners in all U.S. communities with more than 50,000 residents, found that such land is ubiquitous, comprising about 15% of all land in those locales (Table 2.2, p. 29).

All vacant land is not alike, however, as they note. In many Sunbelt communities, vacant land has never been developed and in some cases never even farmed. In these cities, vacant land is a resource to be managed. In older, economically ailing cities, vacant land generally consists of abandoned industrial and residential structures. In these locales, vacant land is a problem not only because it does not provide housing, jobs, or significant revenues but also because the abandoned structures are often used for a variety of illegal activities that can further weaken neighborhoods.

Bowman and Pagano devote the bulk of *Terra Incognita* to a rich discussion of specific policies for vacant parcels and abandoned buildings, which, they contend "are shaped by three imperatives . . . a fiscal need to generate resources and to keep a city's fiscal position strong, a social need to create stable neighborhoods and sectors, and to protect property values, and a development need to ensure and enhance the economic vitality of the community" (p. 45). They add that a variety of factors, most notably general economic circumstances, will lead policymakers in specific localities and specific times to give differing weights to each of these factors.

A variety of structural and legal factors also shape local decisions about vacant land and abandoned buildings. Most notably, Bowman and Pagano make a compelling case that the structure of municipal finance plays a major role in shaping particular decisions. Cities that depend on property taxes are more likely to encourage development closer to their core where the community will reap any spillover benefits. Conversely, communities that rely significantly on consumption taxes are more likely to locate developments near their borders, where nonresidents are more likely both to pay some of the taxes and to bear some of the new developments' negative externalities. Cities that



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rely on income taxes, finally, are more likely to focus on higher-end commercial and residential development.

While noting that this is an idealized framework, Bowman and Pagano offer numerous examples—drawn from locales as diverse as Tempe, Arizona; Columbia, South Carolina; Philadelphia; and Seattle in support of their argument. The mini case studies used in this and other sections are simultaneously one of the book's great strengths and larger weaknesses. On the plus side, they generally serve the desired function of illustrating a particular point. On the negative side, they do not provide enough detail on the political forces that shaped particular decisions or the specific financial information needed to assess the efficacy and wisdom of a community's particular policies.

In the absence of such information, it is difficult to connect the authors' important insights about vacant land to the central questions about urban politics—particularly the historic debates about who has power and how they use it, as well as the emerging questions about how urban politics is being changed as locally owned businesses are replaced by local branches of national and multinational corporations. The shortcomings, however, do not change the fact that they bring a long overdue and insightful focus to the unique role that vacant land plays in urban development.

**Separation of Powers in Practice.** By Tom Campbell. Stanford: Stanford University Press, 2004. 248p. $55.00 cloth, $21.95 paper.

— Murray Dry, *Middlebury College*

In his book, Tom Campbell, a former five-term member of Congress, argues that we ought to focus on the "comparative advantage" of each branch because by doing so, we improve the operation of American government. "The thesis of this text is that each branch has inherent advantages: for each branch not to use its advantage and, worse, to allow another branch to encroach upon it dulls the mechanisms of governmental action" (p. 58). After highlighting his case studies in his introduction, in Part One Campbell discusses the advantages of each branch, the rules of the legislative process, the process of statutory construction by courts, and the judicial practice of following precedent. In Part Two he presents 10 case studies, which exemplify his thesis.

Campbell begins his account of the "comparative advantages" of each branch with the judiciary. Courts "can . . . develop rules of evidence and presumptions that yield orderly resolution of disputes," which include the burden of proof. A judicial trial also provides for the development of procedural fairness to balance private claims that legislative hearings cannot duplicate (p. 19). When constitutional rights are involved, the judicial branch resists popular pressure to disregard the rights better than the political branches do. Campbell criticizes the judicial branch's reliance on precedent (*stare decisis*), which he distinguishes from a lower court's following the decisions of a higher court. He claims that the Supreme Court rarely decides a case on these grounds. This includes *Planned Parenthood v. Casey,* because "nowhere in her opinion does Justice O'Connor state her belief that, had she to rule on *Roe v. Wade,* she would have ruled with the dissent" (p. 78). But that opinion (a joint opinion of Justices Sandra Day O'Connor, Anthony Kennedy, and David Souter) does state that "the reservations any of us may have in reaffirming the central holding of *Roe* are outweighed by the explication of individual liberty we have given combined with the force of *stare decisis*" (505 U.S. 833, 853). Campbell would probably reply that this passage illustrates a rhetorical reliance on precedent and nothing more (p. 78).

The author regards the requirement that courts decide particular cases "by announcing a general principle" as another disadvantage (p. 21). He refers to his later discussion of the Fiesta Bowl, where a broad judicial decision, which involved cutting off federal funds to academic institutions, yielded bad policy results that Congress had to remedy (Chapter 9). A constitutional amendment can address a specific problem more narrowly than a judicial decision. Campbell uses flag burning as his example. After the Court invalidated a state law protecting the flag, Congress considered how to write legislation to get around the decision. He criticizes the Court for including language (perhaps to assure the vote of the late Justice Harry Blackmun) that encouraged Congress to try what turned out to be futile, as the Court invalidated the federal law by the same five-to-four vote. Without taking a stand on the merits, Campbell argues that a constitutional amendment would have been the more appropriate response; it would have trumped the Court on this particular issue only. Although the constitutional amendment is an important check on the judiciary, and it may be more targeted than a Supreme Court decision, he does not consider the likely unsettling effect of reliance on constitutional amendment to overturn unpopular judicial decisions.

Because the judiciary must wait to respond to the particulars of a case, Campbell favors private rights of action, and he favors standing for members of Congress, so long as they have demonstrated a clear legislative interest in the issue (see pp. 197–206). He cites the Second Amendment to illustrate that the judicial branch is under no obligation to act. Here, as with stare decisis, Campbell thinks the Supreme Court should change its behavior (Chapter 13). Finally, he cites the judiciary's limited ability to ascertain facts "not of the kind presented as evidence in trials" (p. 22). He criticizes that part of the Court's treatment of the exclusionary rule that tries to justify first the rule and then the exceptions on the basis of predictions concerning deterrence. Instead, he offers a different approach; a principled approach to rights would support the exclusionary rule,



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and the exceptions to it would grow out of a waiver of the right against self-incrimination (see Chapter 7).

Among the three branches, the legislative branch's chief advantage resides in its power to "order any kind of study it needs." The legislature is thus "the master of words," and it "can write the laws" (pp. 22–23). The legislative branch best balances interests. Here (p. 24) and elsewhere (pp. 101 and 112) Campbell argues that courts are best at protecting rights, whereas legislatures are best at treating interests. This is an important distinction for Campbell and it probably has merits in some cases. But in others, such as cases where the courts exercise "strict scrutiny," fundamental rights must be balanced against compelling state interests.

"The greatest advantage of the executive branch," according to Campbell, "is its flexibility. It can respond to a problem by instructing an agency to deal with it. It can order a rule to be promulgated . . . much faster than it takes for a typical law to be pass." Moreover, prosecutorial discretion "can ameliorate the potential injustice . . . from the application of a harsh rule to specific conduct" (p. 25). On the other hand, in the absence of a private right of action, the executive branch can nullify the intent of the legislature by failing to bring an enforcement action. Hence, Campbell favors legislative enactment of private rights of action, as he favors standing for legislators after they demonstrate legislative support for their position. He also argues that "[t]he executive's power to present the other branches with a fait accompli is another problem" (p. 26). Unity in the executive has the advantage of speed and resolve and the disadvantage of "frequency and gravity of error," which, however, it can correct (p. 26).

In *Separation of Powers in Practice,* Campbell's focus has the virtue of getting lawyers and judges to take the other two branches of government seriously. His chapter on the rules of the legislative process reveals how lawmakers' voting records need to be studied carefully, and his chapter on statutory construction argues that "subsequent legislative action, inaction, or commentary" (p. 55) does not say much about legislative intent. He also demonstrates to political officials and critics of the judiciary the important role that courts play in upholding the Constitution.

**The Democratic Constitution.** By Neal Devins and Louis Fisher. New York: Oxford University Press, 2004. 303p. $72.00 cloth, $19.95 paper.

— Howard Gillman, *University of Southern California*

For more than a quarter century, Louis Fisher has been arguing against court-centered approaches to constitutional studies by showing how constitutional law is "shaped both by judicial and nonjudicial forces," including the actions of "the elected branches, the states, interest groups, and the general public" (p. vii). Neal Devins joined this effort when he and Fisher coauthored *Political Dynamics of Constitutional Law* (1992, 1996, 2001). With this latest iteration, their basic storyline is still the same, although they have added new case studies and a new normative argument about why constitution law "*should* work that way" (p. vii).

After a very brief introduction that sketches out some of the justifications for incorporating extrajudicial elements into constitutional analysis, the authors address what they consider to be some of the more persistent misconceptions about our constitutional system. Their first target is the belief in "judicial supremacy," or the preoccupation among specialists and mass publics to think of the Constitution in terms of Supreme Court case law. To rebut this view, the authors repeat claims that the Framers did not contemplate judicial review of coordinate branches and that *Marbury v. Madison* established a relatively narrow and tame precedent for judicial authority. More generally, they share many statements from power holders in the early Republic who advocated the "coordinate construction" position—namely, that no one branch had any greater authority than any other to interpret the Constitution—and then review a variety of methods and practices that often prevent courts from having the final say. They elaborate the point with an additional chapter that provides examples of how "Congress, the White House, government agencies, the states, the general public, and interest groups" participate in the process of interpreting the Constitution (p. 51).

This is followed by a series of chapters covering federalism, separation of powers, the war power, privacy, race, speech, and religion. After a general introduction to the issue, the authors compile a series of fact-thick case studies designed to demonstrate that many disputes over these questions are resolved outside the courts, and that constitutional law in these areas is a by-product of a dialogue with the preferences of the rest of the political system (especially powerful interest groups and the evolving agendas of other national power holders). Much of this material will be very familiar to those who have read the authors' earlier work, although there are some useful expansions and updates.

The concluding chapter, entitled "The Ongoing Dialogue," is a revised and updated version of an article Devins and Fisher published in the late 1990s ("Judicial Exclusivity and Political Instability," *Virginia Law Review* 84 [1998]: 83–106). In it they elaborate on why judicial supremacy is both empirically inaccurate and normatively unattractive. Among other things, they argue that the Constitution privileges checks and balances over the supremacy of a single authoritative interpreter and that the "trial and error" of constitutional dialogue is a more effective way of ensuring political stability then a false and rigid certainty imposed by autonomous judges: "By agreeing to an open exchange among the branches, all three institutions are able to expose weaknesses, hold excesses in check,



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and gradually forge a consensus on constitutional values. By participating in this process, the public has an opportunity to add legitimacy, vitality, and meaning to what might otherwise be an alien and short-lived document" (p. 239).

Like many of the authors' previous collaborations, this book is well suited for use as a supplementary undergraduate text. The basic arguments about constitutional dialogue are laid out without too much fuss, and the case studies are packed with useful information. For those who still assume that our Constitution only lives in Supreme Court opinions, the book will be an eye-opening corrective.

As it turns out, it is getting increasingly difficult to find constitutional scholars who need to learn this lesson. There is now a fairly robust scholarship that focuses on non-Court-centered constitutional studies and on the role that courts play within the broader dynamic of constitutional politics. Scholars have moved past the metaphor of dialogue to talk more systematically about the particular ways in which courts might relate to the broader political regime, as well as the ways in which dominant governing coalitions might use courts to promote their agendas.

Unfortunately, in this book, these trend-setting authors do not address the latest trends in constitutional scholarship. References to the relevant literature are (at best) pro forma and incomplete. Important contributors to these debates—including Jack Balkin, Susan Burgess, Barry Friedman, Mark Graber, Sandy Levinson, Terri Peretti, and Keith Whittington—receive either no attention or (at best) just a passing mention. For example, in his book *Constitutional Construction* (2001), Whittington devoted an entire chapter to the ways in which politicians, rather than judges, determined the boundaries of federalism, yet these authors found no occasion to include even one reference to his account in their chapter on the very same topic. Michael Klarman's work gets no mention in their chapter on civil rights, even though no one has written more extensively about the political dynamics surrounding the Court's decisions in this area.

Fisher and Devins have spent many years expanding and enriching our understanding of constitutional politics in the United States. *The Democratic Constitution* is a faithful continuation of this body of work. However, after laboring so long to get others to pay attention to their lessons, the time has come for these authors to acknowledge and engage the research traditions that they helped set in motion.

**Globalizing L.A.: Trade, Infrastructure, and Regional Development.** By Steven P. Erie. Palo Alto, CA: Stanford University Press, 2004. 336p. $55.00 cloth, $21.95 paper.

— Charles Ellison, *University of Cincinnati*

Perhaps the dominant political rhetoric of our time is that which divides society into a self-correcting market and a government, one is the necessary and primary cause of our social ills. Government programs designed to mitigate or solve problems, we are told, unfailingly make the target problem worse and create additional, unforeseen problems. Better to leave well enough alone, even if later we lack the means to travel from our driveway to a workplace nearly one hour away on a good day.

Drawing on a wealth of archival materials from city agencies, Steven Erie's "thick description" of 125 years of Los Angeles political history is a refreshing rejection of these bromides. Erie's central claim, unlike recent analyses of the city's globalization by Soja and Kotkin, is that the semiautonomous development bureaucracies and key political and bureaucratic leaders of Los Angeles financed and built a local and regional infrastructure that is the foundation for one of the world's premiere trading regions. Erie finds that the success of this local development regime occurred despite occasional opposition from powerful corporations and the "natural" disadvantages of location.

Local governance and public entrepreneurship, not market and technological forces, are the primary forces in *Globalizing L.A.* Erie claims that Los Angeles is unique in that city government controlled the bureaucracies responsible for water and power, harbor, and airport development. These institutional arrangements enabled a local development state to mobilize and deploy both public and private resources to build an infrastructure that supports the region's global trade and competitiveness. City officials utilized three bureaucratic strategies in this effort: electoral mobilization and passage of numerous bond issues, clientele relations with the industrial beneficiaries of infrastructure development, and effective lobbying of state and federal officials. By 2001, the Los Angeles metropolitan area trailed only eight nation-states in gross domestic product, and almost 15% of all U.S. global trade flowed through its gateways.

Erie recounts the transformation and evolution of the local development state. Reformers' defeat of a laissez-faire growth regime and the powerful Southern Pacific Railroad provided the foundations of the local development state in the municipal airport and the ports of Los Angeles and Long Beach. Between 1933 and 1992, mayors and port and airport officials, with federal help, transformed the ports and airport from local facilities into major gateways for regional, national, and global commerce. The use of the two ports for expanded shipbuilding, airplane manufacturing, military transport, and oil drilling during World War II was critical to infrastructure development and the city's emergence as a force in global trade. New forms of autonomous governance of the airport, the eventual cooperation between harbor officials, and mayoral leadership also shaped the region's growth.

However, building the infrastructure for global trade became a contested and uneven political process. Community opposition to airport expansion and noise, the impact of federal environmental legislation, and the imposition of a new state fiscal regime beginning with Proposition 13 have



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shaped more recent trade-infrastructure projects in the post–Tom Bradley era. In the 1990s, container traffic in the ports nearly tripled, and ambitious port expansion plans continued. Yet port expansion plans had to overcome municipal transfers of agency funds in order to mitigate local fiscal problems, new environmental and governance reform challenges, and a harbor-area secession movement.

Landside initiatives—major rail and highway corridor development and regional airport plans—accompanied this expansion in global trade. Erie finds that regional approaches that employed joint-powers authority for project financing and governance were implemented with more success than highway corridor projects. Implementation of NAFTA-inspired cross-border highway development prevented new trade-transportation bottlenecks. He analyzes how political and community conflicts have undermined regional approaches to the planned expansion of Los Angeles International and construction of a new international airport in southern Orange County.

Erie concludes that although the global engagement of Los Angeles has brought many benefits to the region, its future as a world trade and transportation center is uncertain. Infrastructure and regional development policy now occurs in a new political-economic context. The policy environment contains more numerous and diverse trade constituencies, ethnic minorities and immigrants, and affected municipalities and communities. New costs, such as those associated with continued growth and security measures, have generated alternative concepts for the region's global future. Projected shortfalls in airport capacity have not been addressed. Finally, the ongoing conflicts between democratic accountability and the autonomy and entrepreneurial interests of agencies—in an era when a term-limited and fiscally constrained mayor enjoys greater control over development agencies—are not yet resolved.

The emergence of Los Angeles as a leading trade center provided substantial local, regional, and even national economic benefits in the twentieth century. Erie presents a relatively balanced, though by no means comprehensive, account of the economic, spatial, and sectoral costs and benefits of the city's globalization. This discussion leaves several questions unanswered. First, do the benefits of trade infrastructure development invariably produce more economic benefits than costs? Is there a point at which the costs and/or the inequities of restructuring are too great? Second, his analysis of environmental challenges to the local development state raises quality-of-life issues. In what circumstances does infrastructure development undermine a city's or region's quality of life? And, finally, he underscores the role of governmental institutional arrangements in local and regional development and of leadership in mobilizing voters to support these projects. What remains unclear is whether the local development regime bears much resemblance to what we call a democratic polity.

**Greasing the Wheels: Using Pork Barrel Projects to Build Majority Coalitions in Congress.** By Diana Evans. Cambridge: Cambridge University Press, 2004. 280p. $60.00 cloth, $21.99 paper.

— Jeff Worsham, *West Virginia University*

A variety of recent studies of American politics and public policy focuses on the role of entrepreneurs in the agenda-setting process. While the focus is quite often on the role of entrepreneurs as architects of policy change, or as sources of policy disequilibrium (Frank R. Baumgartner and Bryan D. Jones, *Agendas and Instability in American Politics,* 1993; Mark Schneider, Paul Teske and Michael Mintrom, *Public Entrepreneurs: Agents for Change in American Government,* 1995; Adam D. Sheingate, "Political Entrepreneurship, Institutional Change, and American Political Development," *Studies in American Political Development* 17 [Fall 2003]: 185–203), entrepreneurs may also work to maintain policy equilibria through the maintenance of majority coalitions. (Although this might be the difference between entrepreneurial and leadership activity, the former is interested in assembling a coalition to challenge the policy status quo, while the latter focuses on coalition maintenance in support of the status quo.) Whether they are engaged in "creative acts of destruction" (Joseph Schumpeter, *Capitalism, Socialism, and Democracy,* 1942) or are attempting to maintain a policy equilibrium, a major factor shaping the decision to pursue entrepreneurial activity, as well as the chances of success of such efforts, is the perceived costs associated with assembling a winning coalition. Successful entrepreneurs are those who are able to overcome the collective-action problem associated with coalition maintenance.

Diana Evans's new book picks up this line of research, focusing on how "policy coalition leaders"—primarily committee and subcommittee chairs and the president—use distributive goods, or pork, to assemble winning coalitions in support of general-interest legislation. Evans suggests that the concern with reelection creates a Congress in which the members are preoccupied with constituent service, and bringing home distributive projects in particular, thus presenting authors of general-interest legislation with a variation of the collective-action dilemma. As such, success in assembling a winning coalition is determined by the ability of legislative entrepreneurs to attach pork to their general-interest project.

Evans develops a vote-buying model and uses it to examine a coalition-building strategy that occurs under four conditions: Entrepreneurs "must have access to sufficient distributive benefits" so that they can effect a trade; entrepreneurs must be willing to use a vote-buying strategy; legislators must be willing to sell votes and see doing so as necessary to their reelection; and the actual target of passage is general-interest legislation (p. 31). In the end, these conditions narrow both the subject of her study and the



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impact of her findings. Even so, what she does accomplish is impressive and should make *Greasing the Wheels* of interest to students of Congress and public policy. Through a combination of interviews and quantitative analysis, Evans provides evidence that vote buying occurs, and that it can be used to bring in the final votes necessary to ensure the passage of legislation. Where she is not as convincing is in demonstrating that vote buying is being used to pass general-interest legislation, that is, that pork serves a "higher" purpose.

My primary complaint is with the definition or conceptualization of "general-interest" legislation as "broad based measures that affect the whole nation or a large segment of it" and provide a "collective benefit" (p. 3). I am not convinced that the definition itself gets at the real subject of interest (nondistributive policy), nor am I convinced that the legislation studied fits that definition (the 55 mph speed limit is the exception). The highway reauthorization bills and the NAFTA agreement are simply bundles of distributive goodies packed around the rhetoric of national need. I confess, part of my discomfort has to do with the definition of general-interest legislation. If such a category is useful, it should include regulatory policy, at least some redistributive policies, and most of what Theodore Lowi labeled constituent policy. While the original legislation that created the federal highway system may once have fit the criteria, by the time of Evan's study, it looks like a classic collection of pork.

The definitional problem arises in part from the decision to exclude some policy areas and committees. The author mentions international relations and judiciary as examples, because legislators working in those realms have less opportunity to distribute pork (p. 31). Yet if the purpose of the study is to examine how general-interest bills are often passed with the addition of pork, then committees that deal with issues that provide fewer such opportunities, as well as policy with a nondistributive bent as a matter of course, would seem to be the ideal subject of study. Instead, she limits her study to what is at its heart a distributive policy domain and bills that meet the fourth condition within that domain. While this is an acceptable strategy, I am not convinced that the highway reauthorization bills that are at the heart of her study of the House (Chapters 3 and 4) or the NAFTA case of presidential bargaining (Chapter 5) fit the criteria of general interest.

Do not get me wrong; this is a valuable study of how pork is used to pass legislation and maintain policy coalitions. And the discussion of the efforts to protect the 55 mph speed limit demonstrates how a regulatory policy concern is transformed into a distributive policy giveaway in order to maintain majority support (and hints at what might be a widespread phenomenon that involves more than the distributive policy realms examined in the study). The lesson is clear: Policy entrepreneurs who want to assemble a winning coalition in favor of contested regulatory policy would be wise to provide the opportunity for some members to transform the debate into distributive terms, which is accomplished by offering distributive benefits. This opens the possibility that entrepreneurs who seek to peel off members of a majority coalition may try to transform the argument into nondistributive or ideological terms. Similarly, Evans uncovers another interesting facet of the collective-action dilemma in her study of the highway reauthorization bills and NAFTA. The provision of selective benefits to aid in the passage of legislation at one point in time creates a taste for such benefits in all future negotiations.

This book is full of such keen observations and insights—and I have completely neglected the excellent chapter on earmarks in the Senate. While in my mind it is more focused on how coalition leaders go about adding the last few votes to distributive policy packages than it is on the mechanics of passing "general interest" legislation, it stands as an important and interesting study that adds to our understanding of Congress, policy entrepreneurs, and coalition formation.

**The Money Primary: The New Politics of the Early Presidential Nomination Process.** By Michael J. Goff. Lanham, MD: Rowman & Littlefield, 2004. 228p. $70.00 cloth, $26.95 paper.

— Bruce E. Altschuler, *SUNY Oswego*

As the amount of money raised by those who win the major parties' nominations for president has soared out of sight, scholars, the media, and even the general public have become seriously concerned. Michael Goff's book examines the "money primary," fund-raising before a single primary or caucus vote has even been cast. He sees it, along with media attention and poll rankings, as crucial in determining the viability and eventual success of candidates. The interrelatons of these factors can be seen in his three main hypotheses. Strong fund-raising predicts survival and "success in the early nomination process" (p. 11). Extensive early fund-raising organization and activity increase the possibility of success. Media coverage of these activities forecasts candidate viability and success. In other words, money is important not only for what it can buy but as an early indicator of comparative candidate strength.

*The Money Primary* provides useful detail about an important subject, but it limits itself to a small part of that subject. Despite focusing on the connections between fund-raising and media coverage, Goff notes that cause and effect are "beyond the scope of this work" (p. 8). Nor does he investigate such important questions as who gives the money, what they might expect in return, or what types of candidates are encouraged and discouraged, a topic he looks at in a chapter summarizing the literature on nominations since the reforms of the 1970s. The book does include a brief afterword on possible reform that is highly



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critical of the direction the nomination process has taken, while advocating measures such as reducing front loading to make it more democratic.

The main portion of the book consists of case studies of the early phases of the 1988 and 2000 nomination contests, which were selected because the lack of an incumbent resulted in competitive races in both parties. Although this is certainly a reasonable choice, the different dynamic for both the in and out parties of a contest focusing on a president seeking reelection makes it difficult to generalize from the cases selected.

Because political scientists are only beginning to examine these early stages systematically, the book's main virtue is the detail it provides about preprimary fund-raising. Goff notes that the few works examining this subject consider only those official presidential campaign committees that report to the Federal Elections Commission. By examining leadership political action committees, tax exempt foundations, state-based committees (which proved particularly important to Texas Governor George W. Bush in 2000), and other fund-raising devices used before (often years before) the official declaration of candidacy, he adds to our understanding of how the "invisible primary" winnows out candidates. Presidential aspirants who fail to organize entities that raise significant sums of money before official declarations of candidacy are doomed to fail, often withdrawing even before the Iowa caucuses. Other indicators of success are cash on hand, fund-raising momentum, and the level of matching funds, although the 2004 campaign suggests that the last of these may have outlived its usefulness inasmuch as the main-contenders declined the match in order to evade spending limits. Goff notes that this early fund-raising is a necessary condition of viability but not a sufficient one, as demonstrated by the short-lived 2000 candidacies of Dan Quayle and John Ashcroft, each of whom received millions of dollars in contributions in 1998. Nevertheless, by 2000, these early stages prior to the first votes "may well have become the decisive phase in presidential nominee selection" (p. 117).

The chapter on media coverage of the money primary is limited by its method. Its findings are based entirely on a content analysis of three elite newspapers, the *New York Times, Washington Post,* and *Wall Street Journal.* At a time when an overwhelming majority of the public gets its news primarily from television, local media are increasing in importance, and growing numbers are turning to the Internet, one has to be hesitant about Goff's generalizations. Because television is such a visual medium, its coverage of campaign funding is likely to be quite different from that of elite newspapers. Without information about coverage of other aspects of the campaign, there is no context in which to place coverage of the money primary. Nor can a reader be too confident about trends based on findings from only two elections, 1988 and 2000. Despite this, Goff's findings that the volume of coverage of early campaign finance doubled from 1988 to 2000 and that this coverage "is increasingly concentrated on candidates with successful fund-raising" (p. 178) are worth further examination.

For the most part, however, the confirmation of his three main hypotheses will come as little news to students of presidential campaigns. For example, in *The Presidential Nominating Process: A Place for Us?* (2004), Rhodes Cook writes that compared to early polls, "campaign fund-raising totals at the end of the year preceding the presidential election have proved a more reliable harbinger of success." Essentially, the book confirms the conventional wisdom that money, especially early money, has become more important than ever and fills in some details about how it is raised. Reaffirming these points does have the value of reminding us, as Goff concludes, that the promise of democratization through reforms in the presidential nomination process has been largely overturned by the "money primary," which "has returned control of presidential selection to political elites in general and, perhaps worse, to financial elites in particular" (p. 185).

**Expression vs. Equality: The Politics of Campaign Finance Reform.** By J. Tobin Grant and Thomas J. Rudolph. Columbus: The Ohio State University Press, 2004. 144p. $59.95 cloth, $21.95 paper.

— Joanne Connor Green, *Texas Christian University*

Through a series of experiments embedded in a national survey, J. Tobin Grant and Thomas J. Rudolph systematically examine attitudes toward campaign finance reform in a very interesting and compelling manner. In a thorough analysis, they examine the commitment citizens have to the competing democratic values of political equality and political expression, finding that opinion varies depending upon perceptions of whose rights are being threatened. The central thesis of the research, which is well established with a comprehensive literature review and theoretical analysis, is that public opinion on campaign finance reform is group-centric, a function of the views individuals have toward groups that are thought to be affected by the proposed reform. If individuals feel positively about a group that is seen as being threatened by the reform, they will be negatively inclined toward the reform. Conversely, if individuals view the group with suspicion or negativity, they will favor the reforms. The research represents a systematic analysis of public opinion on campaign finance reform on a nation level, examining the factors that impact opinions and the dynamic manner in which opinions are formed and maintained.

The authors designed and implemented a national survey to examine what the public thinks about campaign finance, how much the public knows about current campaign finance regulations (and how this information shapes attitudes toward reform proposals), the relative saliency of



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campaign finance reform, and how opinions on groups and issues impact opinion on campaign finance reform. The examination of the dynamic involved with the competition of two strongly supported democratic ideals (political equality and expression) provides a very interesting framework for the analysis. The book is logically organized in a manner that allows the reader to follow the development of the authors' theories and clearly evaluate their research methods and findings. In Chapter 2, they provide a clear and detailed discussion regarding the competing manner in which campaign finance reforms are framed. The thorough summary of the framing of campaign finance reform as either political speech or political equality would be especially helpful for readers who are not familiar with the complexity of these issues. The authors clearly and succinctly detail the root of each frame and discuss the implications of each, providing nice groundwork for the research presented in subsequent chapters. As with many complex and controversial issues, the manner in which the issue of campaign finance reform is framed is central to the opinion of the public. As they note, the two manners in which the issue has been framed often lead to opposing political preferences, with the "political equality" frame lending support to reform and the "political speech" frame leading to opposition to reform.

To test their theory of group-centrism forming attitudes toward campaign finance, Grant and Rudolph perform a three-step experiment (embedded in a national survey). On the first step, they relied upon work that has been done in measuring political tolerance to create content-controlled measures for the attitudes. By dividing their respondents into three groups, the authors are able to create self-anchored measures of group affect to test the theory that public opinion toward group rights, group influence, and consequently campaign finance reform is group-centered. After establishing "self-anchors" (in which the researchers determine the most favored and least favored interest groups of the respondents), they then examine if individuals' beliefs about group rights and influence are linked to feelings about the groups in question. Bivariate and multivariate analyses demonstrate that there is an empirical link between the manner in which the public envisions campaign finance and their opinions. The public, along with elites, makes distinctions between issues of group rights (political expression) and group influence (political equality and equal representation). One important element of this research is that respondents are provided contextual information that forces them to state their opinions on concrete political circumstances, rather than simply allowing respondents to agree with abstract democratic principles. As such, the research design provides a good opportunity to assess the respondents' true views of campaign finance reform. The authors find that the public is more supportive, in general, of group rights (even for groups that they do not like), while attitudes toward group influence are more group-centered.

With regard to campaign finance reform, Grant and Rudolph found that citizen attitudes are group-centered, with individuals' opinions of the groups impacted by the reforms significantly influencing their views of proposed reforms. However, they also found that political tolerance and political sophistication can both temper group-centrism, thus promoting more principled calculations that are less influenced by situational considerations. The authors find that citizens act in a rational manner in using group-centered decision calculations—they support reforms that are likely to help groups they view positively and oppose reforms that are thought to harm their preferred group. Moreover, they alter their calculations and standards when evaluating reform proposals that will impact groups they view negatively. While this is not an ideal manner in which to negotiate the conflict between the competing democratic values of expression and equality, it certainly is rational and reasonable. Both proponents and opponents of reform should read the concluding chapter as important practical implications of the authors' findings are discussed.

On the whole, *Expression vs. Equality* is an interesting and thorough analysis of a complex phenomenon that advances our understanding of the dynamic nature of this area of public opinion. The research is succinctly presented, written in an accessible and straightforward manner. The work is appropriate for practitioners and students alike as it provides solid analysis of the literature in the area, issues underlying the debate and a systematic analysis of the factors impacting opinions on campaign finance. Perhaps the most compelling aspect of the research is the manner in which the authors framed the analysis—the dynamic involved when the public is presented with compelling democratic values that compete with one another. This theme ties the book together, adding depth to a fascinating concept.

**Distorting the Law: Politics, Media, and the Litigation Crisis.** By William Haltom and Michael McCann. Chicago: University of Chicago Press, 2004. 332p. $55.00 cloth, $20.00 paper.

— Susan E. Lawrence, *Rutgers University*

If you, or anyone you know, believes that America is in the midst of a litigation crisis, then you must read this book.

William Haltom and Michael McCann set out to examine how "assumptions about an 'epidemic' of civil litigation, a surfeit of rights claiming and a legal system run amok became conventional wisdom" among political elites and plain common sense to everybody else (p. 6). This is an important and interesting question on its face, given the periodic campaigns for tort reform that have swept the



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country during the last three decades: 45 states have passed some type of tort reform measure since the 1970s (p. 51); both houses of Congress passed significant federal tort reform legislation for the first time in 1995 (later vetoed by Bill Clinton) (p. 7); the last two presidential campaigns featured calls for tort reform; and tort reform is the centerpiece in George W. Bush's rhetoric on the health-care crisis.

But there are two things that raise Haltom and McCann's question to the level of urgent and compelling. First, most of this common sense is, in fact, nonsense (p. 81). Over the last several decades, rigorous studies by sociolegal scholars have shown the following: American's modal response to injury is to do nothing; Americans consult lawyers far less often and file far fewer legal claims than the common sense of the tort reformers suggests; lawyers reject far more cases than they undertake; tort cases constitute less than 20% to 25% of federal civil cases; 98% of tort cases are filed in state courts where they represent less than 10% of civil filings; seven out of eight civil filings are settled before verdict; the tort system undercompensates the direct costs of nonfatal injuries; punitive damages are rarely awarded, and even more rarely collected, and they do systematically correlate with compensatory damages; smaller claims tend to be overcompensated while larger claims are undercompensated; and the greater the award, the more likely it will be reduced by the trial judge or an appellate judge, or both, and the more likely that the final award will be at least partially unpaid (pp. 82–99).

Second, one of Haltom and McCann's central contentions is that the "common sense" about an epidemic of litigation, a surfeit of rights claiming, and a legal system run amok is fundamentally linked to the broader culture wars raging throughout the United States (p. 71): "Frivolous litigants displacing responsibility for self-inflicted harm and greedy lawyers abusing the legal system have joined the growing catalog of Others—welfare queens, the chronically unemployed, street criminals, disorderly dissenters, amoral liberals and secular humanists, slackers of all kinds dependent on government help—stigmatized by neoconservatives and neoliberal proponents as undeserving and dangerous." This mass-produced legal knowledge makes an important difference in American political culture that cannot be measured solely by counting the legislative successes of the tort reformers, which, so far, have been held in reasonable check by the powerful trial lawyers. The tort reformer's legal lore has "narrowed the discursive terrain of politics" in significant and problematic ways: It diverts attention away from the corporations responsible for injuries; it reenforces American tendencies to evaluate social problems in terms of individual responsibility and moral character; it diverts attention from the failure of government to adequately provide social services to buffer the risks and harms endemic to modern life; and it puts a stigma on "right-claiming" across areas and forces progressives into defending a system they criticize as benefiting the "haves" over the "have nots" (pp. 282–90). It has made public and private administrators terrified and timid, while most ordinary citizens have been inoculated into a culture of law avoidance—save for the few opportunists who are encouraged into copycat litigation by the fantastic claims of the widely circulated "tort tales." Red herrings and nonsense are no way to make public policy.

But let me be clear, for better or worse; this book is no polemic. Haltom and McCann are deeply committed to the norms and methodologies of social science, with the former a realist partial to statistical data sets and the latter a social constructionist partial to theory-driven, qualitative, interpretive studies. They do a beautiful job of blending the two approaches into a triangulation that gives us uncommon leverage. And unlike a standard polemic, this book does not identify a simple villain. Instead, the authors' careful and nuanced analysis leads them to the general argument of the book "that the simplistic narratives disseminated by policy-driven tort reformers have at once reinforced and been reinforced by everyday news reporting along with enduring ideological commitments endorsing individual responsibility and disparaging legalistic state paternalism" (p. 24).

Haltom and McCann consider the social construction of legal knowledge along three dimensions of social practice: instrumental, institutional, and ideological. Framed by introductory and concluding chapters, *Distorting the Law* is divided into two parts. The first part devotes a chapter to each of the instrumental actors: pop tort reformers, realist sociolegal scholars, and trial lawyers and consumer groups. While one might quarrel with their characterization of sociolegal scholars as instrumental actors, and in fact they do argue that the institutional practices of the academy largely account for the failure of the sociolegal scholars to displace the tort reformers' myths, winning this point would make little difference in Haltom and McCann's overall analysis.

The second part of the book explores the institutional practices or standardized news reporting conventions as they shape the media's coverage of the civil justice system. The authors rely on an impressive quantitative content analysis of 3,300 stories on tort litigation published in five leading newspapers over 20 years (their data is available at www.lawslore.info), supplemented by two rich cases studies. The first examines how a 79-year-old woman suffering third-degree burns from McDonald's coffee became the poster child for tort reform. The second asks why the tobacco litigation has not produced popular counteraccounts of the virtues of the tort system. Demonstrating a number of what they call "homologies" between news stories and tort tales circulated by reformers, they argue that routine news production conventions result in news coverage that very much resembles the "everyday knowledge constructed by tort reformers" (p. 174), and together the



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two produce a nearly unshakable, though false, common sense about the American legal system.

Haltom and McCann weave a discussion of the third dimension—ideology—into their accounts of the instrumental and institutional dimensions of the social construction of knowledge, showing that the ideological component is key to understanding the success of the tort reformers' instrumental efforts and the power of the stories produced by the media's institutional conventions. They point to the American penchant for norms of individual responsibility and minimal state action and show how tort tales invoke these values and how news coverage reflects and reinforces them. The dominant legal lore resonates with the American ethics of self-reliance, toughness, and autonomy and, at the same time, with nostalgic myths of communal harmony. But while ideology is given substantial weight in their explanatory calculus, it retains a shadowy, misty, cirrus-like existence in their analysis. I am left uneasy. Is it simply a delicious, disastrous irony that our ideological commitments to individual responsibility and minimal state action lead us to adopt a system of adversarial legalism (Robert A. Kagan, *Adversarial Legalism,* 2001; Thomas F. Burke, *Lawyers, Lawsuits, and Legal Rights,* 2002), while at the same time vilifying those who turn to that system for relief? So we chase our tail while social welfare languishes—or maybe that is the Right's point after all.

In short, this is a terrific book, and it deserves a much wider audience than such rigorous scholarship usually gets. But it ought to at least be widely read, and taught, by scholars interested in the American public policy process, the limited rise—and possible demise—of the American welfare state, and the construction of political knowledge and, of course, by all students of law, politics, and society.

**Who Are We? The Challenges to America's National Identity.** By Samuel P. Huntington. New York: Simon and Schuster, 2004. 448p. $27.00.

— Gary M. Segura, *University of Iowa*

Samuel Huntington suggests in this book that American national identity is threatened by a tidal wave of Latino—primarily Mexican—immigrants who are refusing to assimilate to American "Anglo-Protestant" values, and who are facilitated in this resistance by the erosion of elite support for those very same values. That erosion is a consequence of the "cults of multiculturalism and diversity" (p. 144) that have collectively "denounced the idea of Americanization," "downgraded the centrality of English," and "advocated legal recognition of group rights and racial preferences" (p. 142), strong charges indeed.

*Who Are We?* makes four contentions, two with regard to the nature and role of Anglo-Protestant values and culture and two specifically focused on the values and behaviors of Latin American immigrants. With respect to Anglo-Protestant values, Huntington contends, first, that there exists an Anglo-Protestant national culture and value-set that has been largely stable for most of our national history, and second, that the universality of this culture—which he judges uniformly, normatively, good—is a necessary condition for the unique and successful elements of our national history.

The first two central points of Huntington's argument represent a serious misreading of American history, and a wholly unserious examination of the merits and demerits of our national character. First, it defies credulity to interpret the sweep of American history and the degree of social change here as little more than cosmetic variations on an enduring core. Likewise, there is considerable dispute in both the theory and comparative literatures over the necessity of a unitary national culture for the survival or success of democratic regimes. Similarly, critics have identified any number of less-than-attractive characteristics apparently endemic in the American identity. Huntington's claims merit serious testing.

They are, however, largely asserted rather than tested. The book never considers arguments and evidence to the contrary. Rather, Huntington's "method" is to appeal to an array of luminaries, including founders, theorists, and—naturally—Tocqueville, each attesting to his claim, that is, the connection between American ideals, especially Protestant Christianity, and national success. This, of course, does not establish the veracity of the claim but, rather, widespread belief in it. I could similarly cite dozens of historical figures, including popes and saints, testifying to the veracity of the Ptolemaic theory of the cosmos, but it would not make it so.

The latter two claims of Huntington's work are that Latin American, and particularly Mexican, immigrants do not share this culture nor subscribe to these values; and finally, that unlike previous waves of immigrants to our society, they do not acquire them over time, either. These two contentions, I would suggest, are similarly unexamined here and merely assertions on his part.

What is most troubling about the unsubstantiated nature of these latter claims is that each is perfectly testable with data available today. Suppose, for example, we identify individualism, a work ethic, religiosity, and belief in political equality as "core" American values. We could measure adherence to these values among both Huntington's Anglo-Protestants and newly arriving immigrants. Further, to assess the degree to which these are acquired during the assimilation process, we could compare the values of those newly arrived to those who have been here for 10 years, to second-generation offspring of migrants, and so on.

Nothing of the sort is done here. In fact, it is fair to say that at no point does the author attempt to sustain any of his claims with regard to what values immigrants do and do not possess, let alone demonstrate that the Anglo-Protestants themselves hold these values. Rather, the



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differences are either presumed or "established" by way of socioeconomic measures of assimilation. In the chapter on Latin American immigration, Huntington presents data and discussion on language usage, education, occupation and income, citizenship, and intermarriage, none of which directly measures adherence to any of the values we might generally regard as "American," and each of which is highly problematic as an indirect indicator of the same.

Data are available. Since 1989, there have been two large and oft-used data sets that directly measure some of the elements Huntington is concerned with, namely, the National Latino Immigrant Survey and the Latino National Political Survey. Moreover, what exactly they say about Latino values and value acquisition is in the scholarly literature but, curiously, unaddressed here (e.g., see Harry P. Pachon and Louis DeSipio, *New Americans By Choice: Political Perspectives of Latino Immigrants,* 1994; and Rodolfo O. de la Garza, Angelo Falcon, and F. Chris Garcia, "Will the Real Americans Please Stand Up: Anglo and Mexican-American Support of Core American Political Values," *American Journal of Political Science* Vol. 40, No. 2 [May 1996]: 335–51). I will return later to the question of this apparent disregard for previous scholarly work and contrary findings. The point, however, is to suggest that the last two of the four critical contentions of the book are untested and unsubstantiated.

Can we, then, really engage this as a scholarly work? The answer, I would suggest, is an unequivocal "no." Huntington's book is not political science in any manner that most political scientists would recognize. What there is here is an extensive, albeit highly selective, literature review. Now, literature reviews can be useful so far as they go, but their usefulness is generally tied to their comprehensiveness. It would be fair to say that the literature surveyed here is anything but comprehensive.

This is also not a critique rooted in methodological bias. Huntington has not gathered a single piece of new data, quantitative or qualitative, in the entire book, nor are there any new analyses of existing data. He has not spoken to any Mexican, immigrant, or multiculturalist to inform his thinking. There are no structured interviews, ethnographies, or new survey data regarding Latino values that he sets out to describe. There is no hypothesis testing to speak of, and no theory building. The entire project is the juxtaposition of assertions, old histories, arguments, and partial findings drawn from disparate sources.

With respect to his description of assimilation and Latino immigration, this effort, at best, could be considered a meta-analysis of existing work. The success and validity of meta-analyses, of course, rely principally on the exhaustive nature of the primary data analyses reexamined. That is, to glean new knowledge from comparing and contrasting the variety of extant findings, we need to consider all that has been "found." The range of research reconsidered by Huntington, though, is actually quite narrow. For example, he does not employ any of the numerous quantitative data sets in existence, collected by the Pew Hispanic Center, the *Washington Post,* and the Tomás Rivera Policy Institute, to name just a few. Many of these surveys, and the analyses and resulting published work, speak directly to questions of assimilation, acculturation, language usage, national and ethnic identity, political preferences, and political behavior. These are data that surely would facilitate Huntington's evaluation of his central contentions.

The range of published analysis considered is similarly narrow. Take, for instance, the central claims of the book with regard to assimilation, immigration, and the attitudes, beliefs and values of Latinos generally and Mexican Americans specifically. The literature on both assimilation and Latino political behavior has expanded dramatically in the last 20 years, as has the scope of inquiry. Works on Latino political behavior, attitudes, and immigration have appeared in all of the general interest journals of political science, including the *American Political Science Review, American Journal of Political Science, Journal of Politics, Political Research Quarterly,* and *Social Science Quarterly,* as well as a variety of more subfield-specific journals, including *Legislative Studies Quarterly, Public Opinion Quarterly,* and *Political Behavior.* A rough count of these articles suggest that as many as 30 refereed, political science journal articles directly related to these questions have been published since 1985, to say nothing of the comprehensive symposium on Latino politics published in *PS* in 2000.

It is telling, then, that in the central chapters whose themes include assimilation and Latino immigration, not a single one of these journal articles is cited (at least that I could find, given the absence of a bibliography in the book). Similarly, I can find reference to only a handful of university press books on this subject matter and an occasional book chapter. This, of course, is not to denigrate the value of the work that was cited but is more to point out a rather substantial gap in Huntington's survey of what is "known" about the subject matter which, in the last analysis, is the only "method" of his work.

The issue here is far greater than simply bruised academic egos. Rather, the omitted works have much to say regarding language acquisition, Latino values and whether they reflect American values, and the patterns of migration and settlement. For just one example to which I alluded earlier, in de la Garza, Falcon, and Garcia (1996, p. 335), the authors find that "[a]t all levels of acculturation, Mexican-Americans are no less likely, and often more likely to endorse values of individualism and patriotism than are Anglos." Now this finding indisputably speaks to Huntington's central thesis, is contradictory to its claims, and is published in a widely read and highly respected journal. It is not necessary, of course, that he accept the piece and its findings as accurate, but it *is* necessary that he engage the piece and its claims, highlighting his objections to its argument and methods, if



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he wants the contradictory conclusions of his effort to be deemed as having scholarly merit.

All of which is to say that, properly understood, *Who Are We?* is a polemic and not a work of scholarship. In large measure, Huntington stipulates this in the foreword, where he writes: "The motives of patriotism and of scholarship, however, may conflict. Recognizing this problem, I attempt to engage in as detached and thorough analysis of the evidence as I can, while warning the reader that my selection and presentation of that evidence may well be influenced by my patriotic desire to find meaning and virtue in America's past and in its possible future" (p. xvii). A patriot, Huntington appears to be saying, should be forgiven for the selective examination of data and preordained conclusions—in this instance, American virtue. A scholar, I would suggest, is not entitled to such forgiveness.

One should not underestimate the currency that highly visible books, such as this one, attain in political circles. The example of Huntington's last book, *The Clash of Civilizations,* and its frequent citation during the current international conflicts in which the United States today finds itself embroiled are, fittingly, the best example. When polemic is presented as scholarship, it bestows legitimacy on the claims—supported or unsupported by the evidence marshaled—that would not have been afforded works more clearly journalistic or ideological on their face. The claims of this tome, unchallenged in a scholarly manner, will be presumed by ideologues and decision makers alike as the product of social science inquiry. They are not.

**Oral Arguments and Decision Making on the United States Supreme Court.** By Timothy R. Johnson. Albany: State University of New York Press, 2004. 180p. $35.00.

— Robert W. Langran, *Villanova University*

This book makes a persuasive thesis that the oral arguments presented in cases before the United States Supreme Court are used by the justices to help them arrive at substantive legal and policy decisions that closely parallel their preferred outcomes. Although that would seem to be logical, the author documents that many scholars who write about the Court do not share this thesis. Those scholars, such as the so-called attitudinalists, posit that oral arguments have no effect on justices' votes. In order to reinforce his thesis, Timothy Johnson uses the strategic model of decision making, namely, that justices are goal oriented, they are strategic, and they account for institutional rules. He then goes on to explain that because the briefs presented to the Court from both the litigants and from amici curiae are understandably biased in behalf of their particular points of view, the oral arguments serve to solve this problem.

In order to prove his thesis, Johnson does cite evidence presented by other scholars who discuss the role of oral arguments, and he cites statements made about oral arguments by the justices themselves. For original data, he uses litigant and amicus briefs, oral argument transcripts, notes and memoranda from the private papers of some of the justices, and the actual Court decisions. His time frame for the data was the years between 1972 and 1986. He looked at the private papers of Justices William Brennan, William Douglas, and Lewis Powell, with the latter being the most useful because of the notes Powell took during oral arguments. He then augmented his data with that obtained from the Inter-University Consortium for Political and Social Research.

Johnson begins his substantive argument by examining the informational role played by oral arguments. He says that justices ask questions during oral arguments to determine the extent of their policy options, as well as to assess the preferences of external actors and how they might react to decisions. He also says that the justices are able to use oral arguments to obtain information not previously provided them.

After that initial argument, Johnson next makes a jump based on Powell's notes and suggests that Powell, at least, used oral arguments as a means to learn about the other justices' preferred outcomes in the cases at hand. This is important in that a justice is then able to effectively build a coalition when deciding on the merits of the case. He sees no reason why Powell's approach would not be one utilized by the other justices as well.

The author then looked at the conference deliberations done by the justices in the contentious case of *Roe* v. *Wade,* as well as the bargaining memoranda circulated by the justices concerning this case, and then follows that by looking at the same material in other cases. The result is that the justices indeed do raise issues at these times from the oral arguments. He reminds the reader that Justice John Paul Stevens has said that oral arguments provide a time to raise issues about which he wants his colleagues to think.

Once the author's thesis has reached this point, there remains one final topic to examine, and that is whether the oral arguments really do affect substantive and legal policy decisions. His data says yes. The Court cited issues discussed during oral arguments more than three times per case, and almost one-third of the references were not even discussed in the written briefs: thus, a unique information source upon which to arrive at decisions. Johnson admits that his data show that although the Court does not often turn to oral arguments when making substantive decisions, it does so under one key condition: when the outcome of a case is in doubt. His data also show that the Court is, indeed, a collegial body and that the justices do not act as separate entities. They also use oral arguments to raise questions about outside forces, namely, the preferences of political institutions, public opinion, and the impact of their decisions. Thus, the Court is not an unaccountable entity.



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In conclusion, Johnson has presented an interesting thesis and has admirably backed it up with data so that a reader does come away from the book with an understanding of the importance of oral arguments before the Supreme Court. The major drawback to the book is that it cites other scholars and works of which most students are unaware, and those citations tend to disrupt the arguments made. In a word, it tends to be difficult reading. It is a worthwhile book for scholars, but only graduate students studying the Court or highly Court-literate undergraduate students will find it holding their interest. *Oral Arguments and Decision Making on the United States Supreme Court* is obviously not geared for the general public. It is an important addition to Supreme Court literature and it is to be hoped that there will be more studies done on this topic.

**The Most Activist Supreme Court in History: The Road to Modern Judicial Conservatism.** By Thomas M. Keck. Chicago: University of Chicago Press, 2004. 370p. $65.00 cloth, $24.00 paper.

— William P. McLauchlan, *Purdue University*

The title of this work presents its thesis at the macro-level, but it is with limited support that the author claims that the current Supreme Court is the "most activist." The core argument of the book is actually presented in the last chapter in clear fashion. There, the author explicitly refers to the Court as the O'Connor Court since he determines that it is largely the blend of political conservatism and judicial moderation, long displayed by Sandra Day O'Connor, that encapsulates the modern Court. So the thesis is no more than stated as something of a straw man at the outset. Thomas Keck "reveals" that the political ideology (liberal/conservative) dimension of justices is not identical to the judicial activism/restraint dimension of their judicial perspectives. These are and always have been two separate dimensions, even if they may not be orthogonal. It is not clear why this is the first major point of the discussion.

The book is organized into three parts after the Introduction. The first three chapters focus on the post–New Deal Court and the Warren Court. This richly outlines the debates among the justices, ranging from Chief Justice Harlan Stone's Footnote 4 in the *Carolene Products* case, through Justice Hugo Black's total incorporation view, to Felix Frankfurter's traditional and strict judicial deference. By the end of this section, the Warren Court's rights-based activist jurisprudence is treated, in contrast with scholars and several justices. Each of these chapters follows a standard format, including a large number of short or excerpted quotations from the protagonists. Each chapter ends with some very limited treatment of the academic debate that reflects these same considerations. The value of this discussion is that it well provides the reader with an outline of the judicial debates that have driven the development of constitutional law in this country since the "switch-in-time that saved nine" in 1937.

Part II seems to be a prelude to the discussion of the Rehnquist Court (post-1994), and it could be shortened dramatically. The debate does change somewhat during this period of the Burger Court and the early Rehnquist Court. The part contains broad generalizations about the country's turn to the political right during the 1970s and 1980s. These may reflect a "trend" or a climate, but the discussion of these features in American politics adds little to the thesis.

These first two parts, Chapters 1–5, are interesting and heavily laden with the general outline of the activism and restraint debate over the past two-thirds of a century. They are largely supported by "clips"—brief quotations from various (selected) opinions or scholarly writings that Keck uses to illustrate or support the point he makes. There are too many of these quotations and not enough glue or text between them.

The last part is the core of the author's argument and his case for the Rehnquist Court's place in constitutional jurisprudence. It is the heart of the book, and after the Conclusions, it is the part I would assign next, for graduate students who are studying judicial activism or modern constitutional development. As noted below, it suffers from some difficulties. However, it is as clear a statement of the Rehnquist Court's jurisprudence as I have seen. It captures the essentials of the divisions and the tensions that underlie this court's decisions and the differences among its members.

It is not clear why the "Rehnquist Court" is divided into Early and Late periods, broken at 1994 when the current "natural court" came into existence. I am left with the question of what the differences between these two "courts" are in terms of constitutional jurisprudence and activism. Keck does not tell us. If nothing else, the division should be made when Justice Clarence Thomas assumed his position on the Court, since at that moment, the core conservative activists were in place (1991).

*The Most Activist Supreme Court in History* has some flaws. Small items include incorrectly cited references. Footnote 1 in the book refers to Tushnet 1996b, but there is no such reference listed. The endnotes (rather than footnotes), as always, make it difficult to follow the author's substantive discussion at places in the text. The larger issues relate to the use of tabular data with little reference and no discussion in the text of what they "prove." The material presented in Tables 2.1 and 2.2 supposedly prove that the Rehnquist Court is the most activist, but with little discussion of why unconstitutional holdings are the appropriate measure for activism. There are other measures. Table 2.2 indicates that the Taft Court, the Late Warren Court, and the entire Burger Court were more "activist" than the Early or the Late Rehnquist Courts, in



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terms of local and state statute invalidation. Table 6.1 is the core display of the thesis, and it would be better presented at the outset of the work, rather than at the end.

Lastly and most importantly, the selection and discussion of particular cases to illustrate the author's argument appear to be a "random," scattered selection. That is, it is not that the cases were selected randomly but that there is no evidence of clear, systematic, or comprehensive criteria for the presentation of these particular materials to support the thesis. (Tables 6.3 and 6.4 contain the list of all the cases in which the Rehnquist Court ruled federal or state statutes, respectively, unconstitutional from 1994 to 2002. What about the same information for the Early Rehnquist Court? The cases are not all treated or used in the text to support the thesis. They are not even ordered, chronologically or alphabetically, so far as I can tell in the tables.)

Still, with all this said, the author has captured the last two decades of Supreme Court decisions in an interesting and important way. He has developed the casual impressions of many observers that the Court has operated with a split personality, in an enlightening manner. He has captured the positions of Justices O'Connor and Anthony Kennedy—a distinct, if limited, view of activism and a unique jurisprudential case-by-case "minimalist" approach to deciding these issues. That is made quite clear in the latter portion of this book, which is an accessible presentation of material on the modern Supreme Court.

**Negative Campaigning: An Analysis of U.S. Senate Elections.** By Richard R. Lau and Gerald M. Pomper. Lanham, MD: Rowman & Littlefield, 2004. 177p. $26.95.

— Gary L. Crawley, *Ball State University*

If you think that negative campaigning has come to dominate the American electoral landscape, depresses voter turnout, is particularly effective in winning votes, and/or is detrimental to democracy, this book is a must read. Richard Lau and Gerald Pomper investigate a series of questions intended to illuminate these concerns. They utilize an extensive data set based on U.S. Senate elections from 1992 through 2002. Their findings are often quite startling. Negative campaigning does not appear to have the deleterious effects conventional wisdom would suggest.

The book is well organized and follows a natural progression. Beginning with a brief history of negative campaigning (going back to the Adams/Jefferson race of 1800), the authors note that many observers believe that negative campaigning is more prevalent today, works, and may have a "corrosive influence on participatory democracy" (p. 3). They define negative campaigning as "talking about the opponent—criticizing his or her programs, accomplishments, qualifications, and so on" (p. 4) (more on this later). To measure "campaign tone," they rely on newspaper accounts and statements coming from spokespersons of the campaigns, not authors of the articles.

Having set the context for their study, the authors then review the literature using meta-analysis—a technique seldom used in political science. Essentially, this approach standardizes the major findings from other studies, leading to conclusions as to the overall state of research in a given area. They find "no significant support for the suppositions that negative political ads are especially disliked, are especially effective, or substantially undermine public support for and participation in the electoral process" (p. 21). This review is one of the many strengths of this book. It provides a comprehensive look at the literature and points to the need for further research.

Next, the authors delve into their data and address the question "Who uses negative campaigning?" They find that about one-third of campaigning is negative (much less than one might assume) and that it comes about largely as a rational course of action for candidates responding to the campaign environment—primarily challengers, those with less money than their opponent, and those who are themselves attacked. They find that negative campaigning is not a response to a "general handbook of campaign rationality" (p. 37). Throughout their data analysis in this and later chapters, Lau and Pomper utilize two-stage least squares procedures to properly correct for the problem of endogeneity arising because of the relationship between campaign spending and campaign tone. They demonstrate several creative approaches in variable construction to explore the interactive effects of campaign tone with campaign spending and campaign exposure, analyze the effects of myriad variables on election outcomes, and present results from both aggregate and individual-level survey data. In addition, Senate contests with incumbents are treated separately from open-seat races. This methodology is valuable to the reader in and of itself.

Although the many findings from this study are too numerous to report here, overall Lau and Pomper conclude that negative campaigning may have an effect on election outcomes, but that that effect is inconsistent and can do more harm than good to a candidate. As they note, for example, negative campaigning does not "provide a ringing endorsement for the effectiveness of attacking the opponent as a campaign strategy in open seat elections" (p. 70). Similar conclusions emerge when the authors examine the effects of negative campaigning on voter turnout and attitudes of political efficacy and trust. Negative campaigning *can* matter, but its effects are modest to nonexistent and inconsistent in direction. Following their data analysis, some normative thoughts on negative campaigning are provided in the final chapter. They consider features of our system that underlie the use of negative campaigning and suggest several "reforms" to deal with them. This chapter (and their earlier work) provides considerable insight as to why negative campaigning occurs



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and what can be done about it. The reader may take issue with some of their suggestions, but they stimulate intelligent discourse and provide a solid wrap-up to their findings.

Although *Negative Campaigning* is an admirable work, there were several weaknesses that deserve mention. First, the authors define negative campaigning on the basis of any comments about the opponent. Thus, if I mention my opponent's record regarding an issue or his/her qualifications to serve in office, I am engaging in negative campaigning. Most observers would consider that "negative" only if the candidate lies, distorts, or engages in "mudslinging." There is simply no way to sort out what is truly negative in the conventional sense from what is, as the authors put it, comparative campaigning. As a consequence, this study loses some explanatory power. Second, while the "meta-analysis" used to provide a literature review is a useful tool, there is no explanation as to how other scholars have defined negative campaigning, which would give the reader additional insight as to how this topic has been approached. Finally, most of the methodology used in this study (such as the operationalization of variables and creation of new variables) is elaborated in the appendix. This leads to some frustration by forcing the reader to flip back and forth to interpret and understand the relevance of key variables.

Despite these shortcomings (and they are not severe), this work is a valuable addition to the research on negative campaigning. It is also a valuable addition to research into the effects of a host of more traditional factors on the outcome of elections and voting behavior. The use of an extended data set on Senate elections and survey data on voter behavior/attitudes provides a worthwhile addition to research in those areas. *Negative Campaigning* is thought provoking and would be an excellent supplemental reader for upper-level undergraduates and graduate students in courses on campaigning, voter behavior, and democratic theory. Indeed, it is a worthwhile read for anyone interested in the forces that shape the American electoral landscape.

**Congressional Communication: Content and Consequences.** By Daniel Lipinski. Ann Arbor: University of Michigan Press, 2004. 160p. $29.95.

— Linda L. Fowler, *Dartmouth College*

For a decade, scholars have debated the impact of rising partisanship on the internal workings of the Congress. With the publication of Daniel Lipinski's analysis of House members' communications to their constituents, it appears that they also need to consider the effects of party conflict on representation at home. Using newsletters and targeted mailings from 1991 to 1995, as well as surveys and interviews with House members and their staffs, Lipinski assesses the communication strategies of a sample of 100 lawmakers and arrives at conclusions that challenge some long-standing views about the interaction of House members and their constituents.

The incumbency effect motivated earlier work on the interaction of legislators with their districts, focusing particularly on the ability of House members to build personal connections with constituents that transcended party and provided electoral protection against adverse national tides. The research of David R. Mayhew, Richard Fenno, John D. Kingdon, Morris P. Fiorina, R. Douglas Arnold, Gary C. Jacobson and many others stressed the individualistic orientation of members, their capacity to shape the Congress to their own political ends, and their avoidance of collective responsibility. Today, the incumbency effect is even stronger, but the efforts of elites to nationalize congressional elections and the propensity for party voting within the electorate raises the possibility that such tactics have limits.

Lipinski's results provide a mixed verdict on the payoffs for legislative individualism. On the one hand, he demonstrates that "running against Congress" was still common in the 1990s but was no longer the dominant strategy for members that it appeared to be in the 1970s, when Fenno wrote *Home Style* (1978). Indeed, attacking the institution appears to have emerged as a tactic best suited to minority party members, whether Republican or Democrat. In addition, he shows how the likelihood that members will be critical of Congress is strongly related to the frequency with which they vote with the majority party and the district-level support for the majority party. On the other hand, Lipinski finds that Democratic members who stressed the "end of gridlock" and legislative accomplishments of the 103d Congress in their newsletters had a very high probability of defeat in 1994, especially in comparison to colleagues with similar vulnerabilities who separated themselves from the institution.

The unique period in contemporary congressional history that Lipinski studies may account for his findings. His focus on the early to mid-1990s had the fortunate benefit of creating a natural experiment in examining how members respond to shifts in party control of the institution. At the same time, the 1994 election was undeniably an unusual, watershed event. Republicans mounted an unprecedented attack on Congress as an institution in order to discredit an entrenched Democratic majority, and Democrats failed to appreciate the extent of their vulnerability. Lipinski's concluding remarks suggesting a need for a longitudinal analysis of member communications thus seems important for determining whether the mid-1990s were anomalous in terms of legislators' communications with their constituents.

An intriguing aspect of the analysis concerns the capacity of members to educate their constituents about roll call votes on major bills before Congress. Lipinski offers evidence that when lawmakers take the trouble to talk about their records, voters become better informed about



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what their representatives are doing in Washington. With questions from two different American National Election Studies surveys, Lipinski develops measures of voters' accuracy in identifying how individual members voted on both the 1991 Gulf War Resolution and the 1993 Budget and Reconciliation Act. He then combines this data, along with the usual individual controls, to assess whether residents of districts with communicative lawmakers were better able to identify their member's position. Both the congruence of the media market and the publicity effort of the member were positive and significant, especially for the Gulf War vote. For constituents with Republican representatives, however, publicity activity about the reconciliation bill appears to have been modest and to have had little effect because the news media had covered Republican opposition to the Clinton budget so heavily. This is admittedly a weak test because Lipinski has no way of determining whether or not particular constituents read or retained the content of congressional newsletters. It is consistent, however, with an emerging literature on voting behavior about citizens who can and do respond to personal communications from political actors.

Given the public accessibility and longitudinal nature of newsletters and targeted mailings, one wonders why congressional scholars have not mined these data in recent years to evaluate legislative behavior. Lipinski's discussion of the merits of such sources in comparison to news coverage or campaign ads is quite persuasive. Consequently, one wishes that he had not framed his question quite so narrowly. By focusing on the issue of institutional performance, he passed up opportunities to talk about other representational aspects of legislators' communications to constituents—differences in lawmakers' agendas, for example, or their relative propensities for credit claiming and position taking. In addition, a broader set of questions might have enabled him to exploit comparisons between mass newsletters to the entire district and targeted mailings more extensively.

In sum, *Congressional Communication* is a timely reminder of the dynamic nature of Congress and how members adapt to changing political contexts. This is not a "big" book in terms of its length or scope, but it is relevant to big questions of representation that are of perennial interest to legislative scholars.

**For the Many or the Few: The Initiative, Public Policy, and American Democracy.** By John G. Matsusaka. Chicago: The University of Chicago Press, 2004. 206p. $29.00.

— Daniel A. Smith, *University of Florida*

In the most recent wave of initiative use in the American states, citizens have passed an eclectic range of ballot measures. Voters have banned gay marriage, punished negligent doctors, prohibited the confinement of pregnant pigs, limited the taxation and spending powers of state governments, funded stem cell research, and ended affirmative action. Critics of the process claim that this plebiscitary system is out of control, with special interests dominating the process and threatening democratic governance. Defenders, in contrast, claim that ballot measure outcomes reflect the will of the people and are certainly more representative of public opinion than legislative bodies.

Versions of these competing visions have been rehashed since the triumvirate mechanisms of direct democracy—the initiative, popular referendum, and recall—were first introduced in the United States in the late nineteenth century. In several of the two dozen American states that permit the process, the initiative has become an important tool of policymaking. Instrumentally, ballot measures have been used by a wide array of citizens, interest groups, and even prospective and incumbent officeholders to achieve a variety of substantive ends, progressive and conservative alike.

In his largely dispassionate study, economist John Matsusaka attempts to establish whether the presence of the initiative benefits the many or the few. An avowed empiricist, Matsusaka examines a century's worth of tax and spending data from 48 of the American states (Alaska and Wyoming, two initiative states, are excluded from his analysis), as well as comparable data from more than 5,000 American cities in the 1980s. His aim is two-fold: 1) to isolate the effects (if any) of the initiative on fiscal policy in states and localities permitting the process, and 2) to determine whether fiscal policies in jurisdictions with the initiative reflect public opinion.

Matsusaka shows that since the 1970s, states with the initiative have had lower levels of taxation and state spending than those without the mechanism (although when controlling for the stringency of the petition threshold, there is only evidence of reduced expenditures, not lower levels of taxation in initiative states). While the key explanatory variable used in his study—a binary measure of whether or not a state has the initiative process—is crude and masks the tremendous variation in the actual use of the initiative across the states, his models are robust. His parsimonious cross-sectional time series models consistently reveal that the mere presence of the initiative is a predictor of the fiscal outcomes. He also finds that states with the initiative have decentralized much of their fiscal responsibilities to local governments and have shifted their sources of revenue from taxes to user fees. However, when the author turns his attention to state fiscal outcomes during the first half of the twentieth century (1902–42), he finds that states permitting the initiative had higher revenue and expenditure levels than their noninitiative counterparts. He also finds that in the 1980s, municipalities with the initiative relied more heavily on charges but did not have lower taxation rates than cities without the process. He chalks up these substantive policy differences to fluctuations in the will of the majority.



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For Matsusaka, the disparate findings indicate that the initiative is nonideological and that its only bias is "toward the majority, whether conservative or liberal" (p. 79). He argues that when public opinion began shifting to the right in the 1970s, the presence of the initiative was instrumental in reining in taxation and spending levels, bringing state fiscal policy in line with increasingly conservative public opinion. In contrast, during the first half of the century, Matsusaka speculates (since he lacks public opinion data), citizens in initiative states wanted to increase the amount of public resources spent on public schools, highways, and social welfare programs, but that state legislatures were unwilling to increase public spending. Although he does not empirically test these claims, he submits that elected officials were out of step with the dominant wishes of the public due to legislative malapportionment, but that in initiative states, the mechanism facilitated the expression and realization of majority public opinion.

The book has several limitations. Because much of the empirical analysis was first published more than a decade ago and draws heavily on social choice literature, Matsusaka ignores most of the recent work conducted by political scientists on direct democracy in the American states. He also does not include in his statistical models the control variables that direct democracy scholars have shown to be strong predictors of support for ballot measures dealing with fiscal matters. For instance, scholars have shown that in states with high racial diversity, white voters are less willing to pay higher taxes for social services, and that tax and expenditure limits (TELs) are more likely to be adopted in initiative states and have the direct effect of reducing taxation and spending. In addition, the author has an unorthodox penchant for interpreting as statistically significant the signs of coefficients that do not meet conventional levels of significance. As a result, he often expresses more confidence in the reliability of his coefficients than is warranted. Furthermore, while his key dichotomous explanatory variable does not measure the actual *use* of initiatives, he often infers such a link, summarizing his findings with statements, such as "Voters appear to have used the initiative to chip away at both revenue and spending" (p. 33). Although Matsusaka briefly touches on the normative question of direct democracy and minority rights, criticizing studies that "rest on intuition, not on convincing, systematic evidence" (p. 115), he offers no empirical evidence of his own before asserting that "an impressionistic look at the historical record does not give much reason to be concerned about the initiative process" (p. 126). Finally, several of the figures and tables in the text are inadequately notated. The data from one table, which Matsusaka interprets as showing how "surveys make it clear that Americans do not trust their representatives to do the right thing, and have much more confidence in the electorate at large" (pp. 130–31), is quite dated, though one has to sift through Appendix 3 to determine it is from 1986.

Matsusaka provides convincing evidence that fiscal outcomes in states and localities with the initiative vary from noninitiative states, and he shows that voting on ballot measures "is an important way citizens signal their policy preferences" (p. 143), as the passage of 11 ballot measures in the November 2004 election banning same-sex marriages makes all too obvious. The overarching claim that special interests do not distort the initiative process, though, goes largely unanswered. While ballot measures may signal the preferences of the voting public, Matsusaka does not probe how ballot measures receiving majority support may also benefit the interest groups that sponsor the initiatives; the two ends are by no means mutually exclusive. In the end, *For the Many or the Few* provides new empirical evidence about the fiscal impact of the initiative and is a useful point of departure for more general inquiries into the effects of the initiative on public policy in the American states.

**Politics in Time: History, Institutions, and Social Analysis.** By Paul Pierson. Princeton: Princeton University Press, 2004. 108p. $49.50 cloth, $19.95 paper.

— Richard Bensel, *Cornell University*

Paul Pierson opens his book with a fictional metaphor in which he imagines "the trendiest new restaurant in town, charmingly named 'The Modern Social Scientist.'" He invites you to tour the restaurant as "the chef explains that the kitchen is divided into two parts." On one side, the chef proudly points out the ingredients, called "variables." On the other side are lovingly arrayed an "extraordinary profusion of measuring devices." In order to prepare your favorite culinary delight, all the chef need do is combine the correct ingredients, properly measured and calibrated. In this kitchen, the length of time in which the variables are cooked, the order in which the ingredients are combined, and the relative skill of the chef are unimportant to the preparation of the meal. (Later on, the book makes it clear that the specialty of this particular establishment is something called the "multiple regression.") Pierson concludes this metaphor with the observation that "[f]ew would want to patronize a restaurant with such a philosophy of cooking, but most social scientists are working in that kind of kitchen" (p. 1).

This is a book about the place of "time" in social science research. But time should not be just another variable in the chef's kitchen. Instead, time is the primary constituent element underpinning the sequencing of events in social reality, the temporal order within which events take place. However, the theoretically important characteristics of this temporal order are much more than the mere sequence in which events occur (e.g., the "one damn thing after another" parody of historical narratives). There



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are four of these characteristics, and a chapter is devoted to each of them. The first and foremost is "path dependence," which highlights "the dynamics of self-reinforcing or positive feedback processes in a political system" (p. 10). A second and related property of the temporal order is the intermittent eruption of conjunctures, which are defined as "interaction effects between distinct causal sequences that become joined at particular points in time" (p. 12). Slowly evolving social processes compose the third characteristic of the temporal order. Such processes can only be detected and properly assigned analytical salience through close study of extended periods of social time. The fourth and last characteristic concerns the trajectory of particular institutions, their emergence, persistence, and termination in time.

Although Pierson repeatedly emphasizes that stereotyped descriptions of theoretical schools of social scientists (which he describes as "highly balkanized and tribal" [p. 7]) always involve serious overgeneralization, much of the book is framed as analytical comparisons of rational choice theory and historical institutionalism. In these comparisons, he privileges practice over theory: "I will often criticize strong *tendencies* associated with particular techniques or theoretical approaches, while accepting—indeed emphasizing—that there is nothing about these modes of inquiry that renders these tendencies logically *necessary*. . . . The question, after all, is not just what a particular technique or theory is capable of doing in principle, but how and to what extent it is actually used in practice—a distinction that is almost always glossed over in general discussions of method and theory" (p. 9, emphasis in the original). As a consequence, parts of the book assume the form of a discursive analysis of the political science profession as a particular community of practicing scholars. From that perspective, one of Pierson's objectives is to persuade his colleagues to pay more than lip service to the temporal order of the events and behavior they analyze. In this sense, he is absolutely correct when he says "I cannot emphasize enough that this is *not* a book about methods" (p. 7, emphasis in the original).

Although Pierson does not contrast rational choice and historical institutional in the way I am going to propose here, much of what he presents appears to rest upon a distinction between "preferences" and "expectations." From a highly stylized perspective, the contrast makes sense in that much of rational choice analysis assumes preferences and rules (institutions) as given and then analyzes the interaction between them. The spare architecture of the approach, as Pierson notes, decontextualizes the event or decision to be explained by basically assuming away the temporally driven processes through which preferences form and rules (as institutions) evolve. Although historical institutionalism is a more diverse research community (and Pierson faults some of the practitioners for indulging themselves in "history as the study of the past" [p. 4]), he sees the analysis of path-dependent processes as the common focus of study. In fact, all the analytically important characteristics of the temporal order can be more or less reduced to aspects of path-dependent processes.

This, of course, is obviously the case for path dependence itself. But it is also true for conjunctures. We can conceive path-dependent processes as autonomous galaxies of dynamically reinforcing social elements that move through time. As long as they are left undisturbed, the central object of these processes (e.g., two historically dominant parties in a first-past-the-post electoral system) is continually reproduced. The microfoundations for that reproduction reside in the expectations of individuals, often in a subconscious taken-for-granted acceptance of the status quo. As constituent features of the social reality in which we live, such path-dependent processes come in uncountably large numbers and inconceivably wide varieties. (This, incidentally, is one reason Pierson's book will have an impact much wider than our own discipline.) Anyone who has ever answered the questions of a three-year-old subsequently realizes just how much of the social world has no better explanation than "that is simply the way it is."

A conjuncture is the collision of two previously autonomous, often unremarkably reproducing, path-dependent processes (galaxies) such that the expectations supporting one or both of them are destabilized. Outside of such conjunctures, in fact, only close historical study can detect most path-dependent processes because their central characteristic is the slow-moving, incremental evolution of their relations to their central feature. Finally, the conjuncture of two or more previously autonomous processes often produces (and thus explains) the birth or death of social institutions. In their successful emergence, expectations become consciously attached to them in such a way as to reinforce their reproduction. In their decline and fall, expectations move, again consciously, in the reverse direction. In between, individual expectations are often subliminal (in part because our attention is usually focused on conjunctures that are much more dramatically exciting and in part because the sheer complexity of life demands that we accept most of social reality as given).

In sum, Pierson has provided historical institutionalism with a theoretical foundation that ultimately rests on individual expectations with respect to the future. In ways that I do not think would surprise him, such a foundation combines easily with at least some of the traditional concerns of rational choice theorists. For example, many collective action and coordination problems arise out of particular patterns of simultaneously held individual expectations and preferences. In addition, as irreducible units of social reality, almost all individual expectations and preferences are imputed rather than observed. Although *Politics in Time* takes us quite a way toward such a theoretical convergence, that project is still, of course, incomplete.



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**The Congressional Experience: Third Edition.** By David E. Price. Boulder, CO: Westview Press, 2004. 336p. $25.00 paper.

— Daniel P. Franklin, *Georgia State University*

Rarely do we have an opportunity to see the intimate workings of one of our major political institutions through the eyes of not just a participant but a participant who is a political scientist as well. And that is probably both the strength and weakness of the third edition of this excellent memoir by David Price. His account of running for and serving in Congress is often, at the same time, too in depth and too superficial to find the right audience.

I could build a course on the Congress around this book. However, I wish that Price would "punch up" illustrations of theoretical points. For example, on page 15 in describing his first primary race he writes: "Mainly, the organized groups and political action committees (PACs) stayed out of the primary. *Their rule of thumb is generally to support incumbents if they have been reasonably receptive to the group's concerns*" [my emphasis]. In fact, the legislative politics literature (e.g., see John W. Kingdon, *Congressmen's Voting Decisions,* 1989) shows that interest groups almost always support their friends who also happen to be incumbents, regardless of party or ideological affiliation. One might get the impression from reading Price's account that interest group behavior in this case was unique to his situation. But it is not. Why not use this opportunity as an illustration of a general and crucial characteristic of interest group behavior?

But maybe Price is not aiming his book at the classroom market. That is understandable given the tendency of textbook publishers to hermetically seal their textbook publishing off from their publishing activities aimed at a more general readership. Once you write a textbook, do not expect to see it at the local Barnes & Noble. But if Price is writing this book for the general reader, his pacing and attention to detail may assume that the reader knows too much. For example, in his sometimes dizzying discussion of three budget cycles on page 154, he writes: "The House passed the Democratic budget resolution on March 18, 1993, by a 243–183 margin, with no Republican support and eleven Democratic defections. But there were already indications, particularly in the Senate, that the votes would be harder to come by when particular tax increases and entitlement cuts were specified in a reconciliation bill. Indeed, the politicking that went into initial House passage of the reconciliation bill on May 27 (219–213) and final approval of the conference report on August 5 with one vote to spare (218–216) was the most intense I have experienced during my time in the House." I know what this means but how many other readers will? Not my students, certainly, and probably not someone without congressional experience who has a good general education. That leaves this book with a limited audience unless it is incorporated as part of an integrated course design.

But that is not to say that for people who study politics and American politics in particular this book is not a fascinating read. I enjoyed reading it in its first edition as much as I enjoyed reading it in its third. And now we have the added bonus of a comparison between the House as it was prior to the 1994 Republican takeover and the House as it is now. And it is in this comparison that the book has its greatest value.

Price's real strength is in his analysis and interpretation of recent trends. His chapter on parties and partisanship is one of the best commentaries available on the substance and tactics of the Republican "revolution" in Congress. And his chapters on ethics and religion in politics are worth the price of the book. These are the strongest chapters in the new edition.

American politics, it seems, is at a crossroad. Too little of the description of that phenomenon is consigned to the popular press. Scholars have not had time to get a handle on what is happening. So it is into that niche that the third edition of Price's book fits quite well, and it is in that capacity that *The Congressional Experience* performs a valuable service.

**Managing the President's Program: Presidential Leadership and Legislative Policy Formulation.** By Andrew Rudalevige. Princeton: Princeton University Press, 2002. 292p. $65.00.

— Erwin C. Hargrove, *Vanderbilt University*

This book responds effectively to a theoretical challenge with an empirical analysis. The author addresses the proposition that successive presidents have increasingly centralized policy development in the White House to the detriment of the departments. The linear trend is said to be in response to the political demands on presidents to control their own programs. Andrew Rudalevige qualifies the theory out of existence by so broadening it that it disappears. He supplies a rich, empirical model in which presidents employ centralized, balanced, and decentralized approaches for developing policy as deliberate strategies as they perceive the different kinds of political capital and expertise involved in given cases. He finds no linear trend at all. All presidents since Harry Truman have used the White House to develop policies, but not to an increasing degree. Centralization is contingent.

The author uses the language of bargaining and information costs, which owes more to Richard Neustadt than to rational choice approaches. Knowledge about institutions is crucial to presidential influence with Congress. For example, the evidence reveals that legislation prepared in the White House is less successful in Congress than if decentralized because White House staff members do not read Congress as well as departmental executives and staffs. Of course, some high priorities must be centralized, and a large number of initiatives that cut across departments



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must be managed by oversight from the White House. Decisions about strategy take a contingent course if presidents understand the politics of institutions.

The centralizing thesis of *Managing the President's Program* depicts presidents as reactive managers who respond to political incentives for control. Rudalevige sees presidents as potentially creative and flexible managers who may best serve their goals in a variety of ways. This is a refreshing way to depict leadership because some scholarship on the presidency runs the risk of burying the president in institutional processes. However, the author excludes presidential personal style from his analysis because the individuality of style is not evident in the model he develops. He focuses on recurring situational factors. This permits him to develop a model. I would add a caveat here. It is almost as if presidents must respond to the dictates of his model, and yet presidents make mistakes and they vary in strategic skill. Some are more perceptive than others by virtue of who they are. However, it is still the case that presidents almost always find the mean of mixed strategies in time, whether they begin by overcentralization or rely too heavily on decentralization. The imperatives of office educate all presidents.

This insight reveals an important difference between history and political science as disciplines. Historians can easily show that individual presidents have made important differences with major decisions about the selection and timing of policies, as well as choices which only they could make, especially in foreign policy. Political scientists wish to understand how presidents work in relation to institutions, and they discover patterns that are not within the historian's purview. One may generalize about the contributions of presidents to American historical development as either a historian or political scientist, but the analysis of presidents in institutions is best suited to the work of political science.

The author also explicitly excludes public opinion about presidents and the relative size and strength of the president's party in Congress from his model. He sees public opinion as of marginal importance, as suggested by the literature. This is perhaps true across the 400 cases from *The Public Papers of the Presidents* that he surveys. Again, he does not consider crucially important cases in history. One would presumably leave this to the historian. But there is plenty of empirical evidence that presidents recognize and tap latent or emerging opinion and exploit it effectively with Congress. It is also the case that low presidential approval harms a president with Congress. There may be no one-to-one link between presidential approval and legislative success, but this is not to rule out opinion in indirect and subtle ways. By the same token, he rules out the question of the place of the president's congressional party in getting the president's programs passed. We know better than this. It is also possible that political pressure from Congress may inhibit the strategies chosen by a president for the development of policy. I do not fault the author for not taking these things into account. The difficulty is that the effort to formulate a comprehensive model that would take presidential skill and strategies, public opinion, and the state of Congress into its bosom would collapse back into history. Our models are necessarily incomplete; we just need to recognize that this is so.

The author uses exemplary methods for sampling and reconstructing the legislative history of each case. The patterns reveal presidents to be rational actors but in a more complex field than the pattern of linear centralization hypotheses. It may be the case that political scientists often discover what experienced practitioners in government already know. If confronted with these findings, such people might claim that they understand all that. But they would also make strategic mistakes in terms of the model because the pressures of time and politics and their own shortcomings would mislead them. And certainly, they would not be able to formulate and write it themselves. Political scientists learn from practitioners, but they also tell us far more than they know.

**Divided Union: The Politics of War in the Early American Republic.** By Scott A. Silverstone. Ithaca, NY: Cornell University Press, 2004. 278p. $42.50.

— Donald L. Robinson, *Smith College*

Is it possible, by constitutional arrangements, to reduce the likelihood that a nation will go to war? This is the question to which Scott Silverstone addresses this informative and carefully argued book.

The author begins by reviewing the theoretical literature (realists versus democratic idealists, Kenneth Waltz versus Bruce Russett). Such introductory material is often turgid, sometimes irritatingly so, but Silverstone accomplishes this part of his task efficiently. He argues that neither side pays adequate attention to "the most important institutional feature [in the American system] shaping domestic competition over questions of war and peace" (p. 10), namely, the provisions that ensure that a diversity of regional perspectives will come to bear on questions of national security. Some theorists, he notes, argue that democracies tend to be less bellicose than other types of regime. However, such theorists give scant attention to the varying ways in which democratic impulses are registered and channeled. Theorists who do take institutional arrangements seriously tend to look at checks and balances and the separation of powers. They virtually ignore the feature that was central to the men of 1787 about how to restrain the impulse to war: that the United States is a *federal* union.

The legislature, both houses of Congress, was built upon federal principles (particularly when senators were elected by state legislatures), and so was the electoral system for



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the executive. The Framers, he shows, counted upon this factor—the unique federal character of the American union—more than any other to restrain the dogs of war.

The bulk of Silverstone's engagingly written book consists of case studies. It focuses on American foreign relations before the Civil War, a period in which the United States declared war twice (1812 and the Mexican War), but restrained the impulse to war on numerous other occasions. The author analyzes several of those episodes: confrontations with Great Britain and Spain between 1807 and 1815, the Oregon Crisis of 1845–46, the Mexican War, and conflicts with Cuba and Mexico in the 1850s. In each of these cases, he describes the pressures and passions that impelled the nation toward war, and he examines the countering tendencies as well. He discovers that restraint is often rooted in regions of the country where an aggressive policy seems unwise or threatening to vital interests. Often, during this first half century of the young republic's life, these perspectives, working through an institutional framework built on the American federal system, succeeded in dampening martial passions.

Silverstone's treatment is subtle and pays close attention to detail. In discussing the Mexican War, for example, he recounts the familiar story of how President Polk maneuvered the nation into war by ordering General Zachary Taylor and his army to march into disputed territory, then, when Mexico fought back, demanding that Congress recognize a state of war already existing. He shows that there was no majority in Congress for war with Mexico (Whigs opposed it solidly, and Polk's own party, the Democrats, were dubious, too). However, distracted by the threat of war over Oregon, leaders in Congress were caught unawares and did not begin to resist until the president had put facts on the ground.

Most analyses stop here. The Mexican War is understood to demonstrate that the Framers' institutional arrangements (reserving to Congress the power to declare war) did not work as intended. One man, using his powers as commander in chief, led the nation to war.

Silverstone grants that point but pursues the inquiry a step further, to show that the compound nature of the federal republic operated to restrain Polk's intention to add much more territory than eventually came from this war. Polk wanted to add Mexican territory east of the Sierra Madres, or perhaps all of Mexico, to the United States. But his plans were strenuously opposed, not only by the Whigs but also by southern Democrats, led by John C. Calhoun, who feared that Mexico would not be hospitable to slavery as practiced in the American South, and also by northern Democrats, led by Martin Van Buren, who opposed any augmentation of the slave interest. Polk was firmly convinced that the national interest would be best served by annexing as much of Mexico as possible, but regionally oriented politicians, firmly ensconced in Congress, refused to support him. They would not appropriate the money needed to accomplish the president's aims. Thus, Polk was able to bend the Constitution to expand the southern boundary of Texas, but he was frustrated in his larger territorial ambitions by political arrangements that gave power to regional politicians.

Anyone wishing to review the Framers' intent on these points, or to see how the American constitutional pattern worked in the early years of the republic, should read Silverstone's superb book. It is laudably disciplined. He does not permit himself to comment on the light his analysis might shed on current controversies.

Readers, though, may be impelled by *Divided Union* to take another look at the effect of institutional arrangements on national security policymaking. We have spent perhaps too much time lamenting the failure of checks and balances and the weakness of Congress, and not enough time thinking about our being a federal republic. Here at the start of the twenty-first century, as we take up our responsibilities as an imperial power, we need to pay attention to the Framers' most enduring legacy: not the separation of powers but the stubborn persistence of our federal system.

President George W. Bush may have been able to bend Congress to his will in taking the nation to war with Iraq, but he may yet be restrained by regionally oriented politicians in Congress from pursuing his grand design of a world of "democratic" regimes. Whether world peace and our national interests will be well served by these interactions within the American system remains to be seen.

**Educated by Initiative: The Effects of Direct Democracy on Citizens and Political Organizations in the American States.** By Daniel A. Smith and Caroline J. Tolbert. Ann Arbor: The University of Michigan Press, 2004. 252p. $65.00 cloth, $22.95 paper.

— Edward L. Lascher, Jr., *California State University, Sacramento*

Throughout the mid-1980s, empirical state-level initiative studies were sufficiently rare that David Magleby's now classic book (*Direct Legislation: Voting on Ballot Propositions in the United States,* 1984) addressed topics as varied as the status of direct democracy laws, the impact of various cues on voting choices, and the readability of ballot pamphlets. Research on the initiative process subsequently burgeoned, and direct democracy has become one of the most widely considered aspects of state politics. Quantitative studies have focused especially on the substantive impact of the initiative process on public policy, as well as on the extent to which campaign contributions influence electoral outcomes.

Yet as Daniel Smith and Caroline Tolbert emphasize, another aspect of the initiative process has received considerably less attention: the extent to which ballot-measure decisions educate citizens about the workings of democracy and interest them in democratic processes. This absence



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is notable because extant arguments about the virtues of direct democracy commonly stress its educative role. While a few recent articles have addressed selected topics in this area, there has been no book-length study. In short, Smith and Tolbert have identified an important niche to fill.

After a brief overview, the authors turn to an explication of the Progressive Era claims about the educative impact of direct democracy, or lack thereof. They mine a rich array of sources, including the flowering of works on ballot measures published in the then-young political science academic journals. Many Progressive Era scholars believed that adopting the initiative would bolster election turnout, enhance civic engagement, increase political knowledge, and promote a sense of governmental responsiveness. Some also believed that adoption of the initiative would curb the power of party bosses. Yet skeptical voices also were heard, with some commentators questioning the educative value of direct democracy, given such factors as the complexity of the initiative process, continued role of special interests, and potential for political parties to hijack ballot measures.

Chapters 2–4 of *Educated by Initiative* constitute its analytical heart. In these chapters, Smith and Tolbert examine quantitative evidence bearing on key claims of Progressive Era (and modern) initiative proponents. The authors compare citizens in the roughly half of American states that permit ballot initiatives with citizens in the other states. Consistent with much recent work on direct democracy, they consider the frequency of initiative use, rather than simply the presence of the direct democracy mechanism, recognizing that some states (e.g., Oregon) use the initiative much more frequently than others (e.g., Illinois). They draw on aggregate data, such as turnout rates, and individual data from surveys such as the National Election Studies.

In general, Smith and Tolbert find support for the claims made by initiative proponents regarding the effects of direct democracy on electoral turnout, information about and interest in politics, and civic engagement. For example, they find that, controlling for other variables, states with more initiatives on the ballot have higher turnout, especially in midterm elections. Additionally, states with more initiatives rank more highly on Robert Putnam's social capital index, a notable measure of civic engagement. As the authors indicate in their concluding chapter, initiative advocates will "likely be heartened" by these findings.

However, subsequent chapters find less support for a couple of other Progressive Era claims, notably the idea that the presence of the initiative undermines partisan politics. In fact, Smith and Tolbert amass evidence that initiatives have been used for partisan purposes in such states as California and Colorado. Furthermore, in contrast to Magleby, they find that party identification exercises a large impact on voting for a wide range of ballot measures.

A notable feature of the book is the clarity of presentation. Smith and Tolbert indicate in their preface that they intend to reach practitioners as well as academics; I believe their strong organization of material and stylistic choices should help them to do so. For example, they avoid cluttering the main text with regression tables, instead highlighting key statistical findings in short boxes while allowing the true connoisseurs of statistical analysis to turn to the appendices.

Although the overall project is commendable, I have two related concerns. First, the authors' data boxes and summary statements, if not the fine detail of the text, appear in places to overstate the evidence for initiatives' beneficial effects. Consider the results of nine regression analyses (contained in Tables A.5 to A.6) assessing the impact of the initiative on political knowledge, political interest, and political discussion in 1996, 1998, and 2000, based on NES surveys. While eight of the nine coefficients for the initiative variable have the "right sign," only one is statistically significant at the .05 level, and only four are statistically significant at the .10 level. By contrast, the claim about the positive impact of initiatives on voting obtains more consistent statistical support.

Second, the authors could give more consideration to how initiative states might differ from noninitiative states in unmeasured ways. For example, noninitiative states are disproportionately clustered in the group of states with a "traditional" political culture in Daniel Elazar's widely used classification scheme; most of these states are in the South. Previous literature has shown that states with a traditional political culture are less responsive to differences in public opinion (Robert S. Erikson, Gerald C. Wright, and John P. McIver, *Statehouse Democracy,* 1993, Chapter 7). In this regard, it is notable that Smith and Tolbert include a southern state control variable for many of the regression analyses related to whether people vote, but not for the analyses of the other potential educative effects of direct democracy.

Thus, more refinements in the data analysis may be needed. Nevertheless, Smith and Tolbert have pursued a valuable area of research and have provided a solid foundation for further work. They have helped to specify and examine noninstrumental effects of direct democracy that may ultimately prove as important as any instrumental ones. For this we owe them a debt.

**The Invention of the United States Senate.** By Daniel Wirls and Stephen Wirls. Baltimore: Johns Hopkins University Press, 2004. 288p. $49.95 cloth. $18.95 paper.

— Fred R. Harris, *University of New Mexico*

In a meeting in Moscow some years ago, the former Soviet Union's principal Americanologist, Georgi Arbatov, complained to me quite seriously, "We study your system, and we still do not know how it works." Probably one reason for Arbatov's expressed puzzlement had to do with the



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role played in the American governmental system by the United States Senate, of which, at the time of our conversation, I was a member.

There is no doubt that the Senate is a peculiar institution in our democracy (and part of the justification for our saying, if we are careful, that in our system the people rule "in some way"). The Senate is not a majoritarian body, of course—with two senators from each state, large or small (so that less than 17% of the nation's present population can elect a majority of the Senate) and with internal rules that allow a minority of senators to block action by a Senate majority.

There is no wonder that the Senate is something of a puzzlement. According to the authors of this excellent book, the Senate emerged from the Constitutional Convention, itself, as a "clumsy hybrid," a "committee's version of a horse" (p. 206)—that is, a kind of camel. And it was altered some, too, later on.

The unique value of this book by Daniel Wirls and Stephen Wirls is found in its combined discussions of three aspects of the "invention" of the Senate. The book begins with an interesting exposition of pre-Convention liberal-republican theory and practice. It then examines the crucial decisions made at the Convention about the Senate, decisions which are shown to have been more than the mere result of an often-called "political deal," but involved matters of substance, too. Finally, the book traces the later institutionalization of the Senate, from the ratification period through the early years of Congress.

American Founders such as James Madison wanted the Senate to be an equal, positive legislative actor with the House of Representatives, a liberal separation-of-powers check on the House and the executive, and a small, independent, selectively appointed republican bastion of thoroughly "national" prudence and wisdom. But the Convention decided also to make the Senate an embodiment of federalism and the rights of state governments (two senators from each state, originally elected by state legislatures). In operation, the Senate became more internally individualistic, while power in the House became more hierarchically organized. And the Senate became increasingly interest- and constituent-oriented, especially after a constitutional amendment that provided for the popular election of senators.

Congressional scholars, including the authors of this book, have been focusing more and more on the fact that the U.S. Congress is composed of *two* chambers and that the two are very different—in formation, representation, and operation. And all that is to the good, if we Americans are to understand better than Russian Georgi Arbatov how our system actually works.

*The Invention of the United States Senate* arose from a vacation-time friendly argument between the authors. One pronounced the Senate an "undemocratic constitutional atavism," while the other, defending bicameralism in general, heralded the Senate as "essential to moderate and free government" (p. ix).

The argument was never resolved. Maybe the two political scientists were both right. But you and I are fortunate that to bolster his own side of the argument, each of the authors soon plunged into some serious research about the United States Senate, and then both finally pooled the results of their efforts to produce this highly scholarly but also highly engaging work.

**That Eminent Tribunal: Judicial Supremacy and the Constitution.** Edited by Christopher Wolfe. Princeton: Princeton University Press, 2004. 256 p. $55.00 cloth, $19.95 paper.

— D. Grier Stephenson, Jr., *Franklin and Marshall College*

"Constitutional law professors have been very upset lately with the U.S. Supreme Court" (p. 181). So begins Keith E. Whittington's contribution to this collection of 11 essays on the subject of, in the words of the subtitle, "judicial supremacy and the Constitution." Many in the unhappy chorus mentioned by Whittington are mainstream legal academicians who decry the conservative judicial activism sometimes practiced by the Rehnquist Court; they long for a robust activism in defense of liberal political values that characterized Warren and some Burger Era rulings. But the contributors to this book are not members of that chorus. Instead, most question or condemn the liberal activism preferred by the legal mainstream, while a few reject both styles of activism and hanker for a modest judicial role characterized by editor Christopher Wolfe as "traditional judicial review" (p. 202).

Nine of the essays—those by Hadley Arkes, Gerard V. Bradley, George W. Liebmann, Michael W. McConnell, Robert F. Nagel, Jack Wade Nowlin, Steven D. Smith, Jeremy Waldron, and Michael Zuckert—were originally delivered as papers at the American Public Philosophy Institute conference on "Reining in Judicial Imperialism" (p. 9). While the year of the conference seems not to be noted in the book, a statement in Arkes's essay, referring to *Bowers v. Hardwick* as having been decided "[a]bout a dozen years ago" (p. 59), indicates a date of about 1998. Endnotes for the conference essays suggest that updating for publication was done only very sparingly.

According to the editor, the central problem addressed by the authors is the "judicial imperialism" (p. 3) that has"profound[ly] transform[ed]" the role of the Supreme Court in a way "fundamentally inconsistent" with the Framers' scheme of separation of powers (p. 1). "[E]xtreme notions of judicial power" have encouraged Americans to perceive the Court as "the *final* or *ultimate* authority on constitutional issues" (p. 1, emphasis in the original)—the precise situation that Abraham Lincoln, in his first inaugural address, cautioned against in the aftermath of the Dred Scott decision, a warning that inspired the book's title. "[I]f the policy of the government, upon



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vital questions, . . . is to be irrevocably fixed by decisions of the Supreme Court," declared the sixteenth president, "the people will have ceased, to be their own rulers, having . . . practically resigned their government, into the hands of *that eminent tribunal*" (p. 1, emphasis added). The central question thus becomes how to limit judicial power "effectively in order to reestablish a full measure of republican government" (p. 2). In Bradley's words, the "Framers did not endorse—they could scarcely imagine—an insulated judicial prerogative to determine what the law should be" (p. 15).

The contributors have far more to say about the problem than about solutions. Changing course will be no easy task, Wolfe admits, for "elite intellectual opinion is strongly behind the courts," because the judiciary gives elites "the political power that they cannot win through elections." Moreover, the legal profession has been educated by those who are "committed to modern notions of judicial power." Finally, "'new class' professions," such as journalists and entertainers, have aligned themselves with "liberal intellectuals" and so lend their influence to those who would oppose efforts to rein in ambitious judges (pp. 2–3).

Of course, laments about federal judges are nearly as old as the Republic. Judicial review, an absence of political accountability, and lengthy tenures in many instances soon combined in the nineteenth century to make the Court a frequent center of controversy. This was true not only when the justices negated legislative acts but also sometimes when they did not. *McCulloch v. Maryland* (1819) was a contentious decision not merely because the Court invalidated the state's tax on the Second Bank of the United States but because the Court sustained the statute chartering the bank. By 1893, concerns over an expanded judicial power were so widespread that Harvard's James Bradley Thayer cautioned judges not to step into the shoes of the lawmaker. Sixteen years later, Samuel Gompers railed against judicial intrusion into the American Federation of Labor's dispute with the Buck Stove and Range Co.—and implicitly disputed Charles Evans Hughes's counterassertion in 1907—by insisting that the Constitution and a judge's interpretation of it were not necessarily the same: "I still believe that the Constitution . . . is greater than any Judge" ("Gompers Raps the Courts," *New York Times,* April 6, 1909, p. 18).

Even against that turbulent background, a prominent theme of *That Eminent Tribunal* is that the present situation is without parallel. The example cited most often in a majority of the essays is the opinion of Justices Anthony Kennedy, Sandra Day O'Connor, and David Souter in *Planned Parenthood v. Casey* (1992), particularly the so-called mystery-of-life passage. Zuckert finds in that paragraph and surrounding ones an "apparent endorsement of a nihilistic view of the universe—what meaning there is is meaning we supply (each of us) for ourselves" (p. 44). "A Constitution evolving to match the changing moral and metaphysical conceptions of the nation is one thing," he maintains, "but a Constitution evolving to impose exotic ideas on the nation is quite another" (p. 45). As portrayed by several authors, the tone of the "*Casey* Three" (p. 54) is one of arrogance and presumptuousness, quite unbecoming of the servants of a republican nation. As Bradley contends, "The Court is telling us: 'We will be your Court and you will be our people'" (p. 18).

Ironically, while preserving the "core" holding of *Roe v. Wade* (1973), that abortion should be allowed through at least the second trimester of pregnancy, *Casey* upheld nearly all of the Pennsylvania abortion regulations challenged in that case. These were provisions clearly designed to discourage abortions, and prior to Kennedy's arrival in 1987, most of those provisions would almost certainly have been struck down. Alongside ambivalent opinion showing that the public is troubled by abortion but desires that the option remain available, the decision tacked to starboard. The contrast between what the Court did and what it said in *Casey* and similar cases suggests that Congress and the people are probably less moved by judicial style and words than by judicial actions. Contributors to *This Eminent Tribunal* are concerned about both. The eventual impact of thoughtful commentaries such as theirs may depend on persuading the people that as possible parents of future acts, words and style matter, too.

**Explaining Foreign Policy: U.S. Decision-Making and the Persian Gulf War.** By Steven A. Yetiv. Baltimore: Johns Hopkins University Press, 2004. 296p. $49.95 cloth, $19.95 paper.

— Steven W. Hook, *Kent State University*

Steven A. Yetiv faces a daunting task in this case study of the 1990–91 Persian Gulf War: applying and reconciling multiple approaches to foreign policy analysis in a single volume. He also hopes to clarify the historical record regarding President George H. W. Bush's performance in the diplomatic and military clash. While this ambitious study advances our understanding in both respects, it demonstrates how much further the field of foreign policy analysis must still progress to see clearly within the "black box" of state decision making.

The book self-consciously builds upon Graham Allison's *Essence of Decision* (1971), which led a counteroffensive in the scholarly literature against the prevailing neorealism of the early Cold War and its analytic offspring, the rational actor model (RAM) of foreign policy behavior. Like Allison, Yetiv is determined to reveal the limitations of the RAM and to integrate the impact of domestic pressures on policy choices. His study expands on Allison's study, which examined the Cuban Missile Crisis, by looking beyond bureaucratic behavior to possible electoral calculations and psychological factors (cognitive processing and "groupthink").



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Yetiv generally succeeds in identifying the strengths and weaknesses of the different approaches as they apply to the Gulf War. He finds the RAM's emphasis on system-level cost–benefit analysis useful in explaining the U.S. government's initial evaluation of Iraq's threat to the Persian Gulf. Also in keeping with the rational model, he denotes the structural obstacles, inherent to the anarchic state system, that prevented the two sides from establishing mutual trust through strategic interactions.

The study finds these insights necessary, but not sufficient, in making sense of U.S. decisions leading up to the war: "Nonrational" forces also shaped the White House's increasingly strident posture during this period. Specifically, President Bush was swayed by domestic political factors, including his need to deflect attention from the budget deficit and enhance his image as a visionary leader at the dawn of the New World Order. Bush's strong presence within his inner circle of advisers discouraged consideration of alternative solutions, a pattern first explicated in Irving Janis's *Groupthink* (1982). In addition, the president's frequent depiction of Saddam Hussein as a present-day Adolph Hitler is described as a common form of perceptual "satisficing." These and other findings are derived from a variety of sources, including interviews conducted between 1996 and 1999 with many of the cabinet-level U.S. officials engaged in the crisis. While gaining such access to key decision makers is impressive, however, neither the interview methods nor a classification of the responses is disclosed in the analysis, which consequently takes on an anecdotal quality. National Security Adviser Brent Scowcroft, for example, tells Yetiv that Bush "felt strongly about reversing the invasion, and the real question then became how it would be done" (p. 133). Colin Powell, chairman of the Joint Chiefs of Staff, recalls being "surprised" (p. 144) by the lack of internal debate over Bush's statement that Iraq's occupation of Kuwait "would not stand." While these are revealing comments, their presentation in the absence of a systematic research design does not yield persuasive tests of the various hypotheses concerning rational action, institutional biases, cognitive dysfunctions, or other behavioral patterns.

The presentation of other evidence could also be more rigorous. Yetiv relies too heavily, and uncritically, on memoirs and quotations from media reports and documentaries. The application of cognitive theory focuses too narrowly on analogical reasoning, and more primary sources would bolster the testing of the government- and domestic-politics models. The analysis is further clouded by unsubstantiated assertions. Noting that Congress historically follows the president's lead in matters of war and peace, for example, Yetiv suggests that the January 1991 war resolutions approved by both chambers were superfluous, as "a negative vote most probably would not have stopped the president from launching war" (p. 184). This may well be true, but what is the author's basis for this claim?

To his credit, Yetiv acknowledges that his reach extends broadly, and at times uneasily, across the many frontiers of foreign policy analysis, and he concedes that his findings often contradict his own expectations. Indeed, despite the study's overarching goal of cutting RAM down to size, the historical record is often consistent with rationalist expectations: a direct threat to regional security in a strategically vital area, a strong leader who forces consensus in his inner circle while suppressing bureaucratic rivalries, the dismissing of a skeptical public opinion and Congress, and a favorable outcome that resembles the cost–benefit calculations of decision makers. Even the vilification of Saddam, whether due to satisficing or deliberate social construction, may be considered rational action under the circumstances.

Still, Yetiv finds much of this record "nonrational," and he is on the safest ground when examining the president's domestic political benefits to be gained from forceful action against Saddam and his refusal to consider alternative responses. This assertion raises another puzzle, however: nonrational behavior producing a "non-negative" outcome (p. 198) in the form of Kuwait's liberation. The author attributes this to "mitigating" factors, such as unusually energetic leadership, ample resources and allies, and a challenge that was easily overcome. Thus, the book produces admittedly mixed results, "a complex continuum of behaviors, some rational and some not, some quasi-rational and some simply ambiguous or opaque" (p. 206).

While such anomalies are an inescapable part of political life, the roots of this paradox may be found in the case selection. It is widely accepted that the management of national security issues produces governmental behaviors that are more consistent with realist expectations than decision making in routine situations or on matters of "low politics." This distinction was made decades ago by Stephen Krasner, whose book *Defending the National Interest* (1978) found a pattern of autonomous state action in U.S. national security policy. More recently, Amy Zegart's *Designed to Fail* (1999) provided a new twist by highlighting differential institutional dynamics in the creation of domestic and national security agencies, respectively.

Yetiv properly concludes that future research should place issue domain at the center of foreign policy analysis. And he justifiably states that his study of the Persian Gulf, despite its contingent findings and implications, can still be useful in generating "heuristic devices" for analysts and students (p. 215). Whether or not *Explaining Foreign Policy* ultimately takes its place beside *Essence of Decision* as a seminal work in this field, the book serves the same function in challenging analysts to question conventional models and accommodate complexity in the scholarly study of foreign policy.



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**Helping Children Left Behind: State Aid and the Pursuit of Educational Equality.** Edited by John Yinger. Cambridge, MA: MIT Press, 2004. 145p. $40.00.

— Frederick M. Wirt, *University of Illinois*

The role of state courts in defining educational finances is the focus of this book that results from a 2001 conference in the Maxwell School of Citizenship at Syracuse University. The author explores the role of state appellate courts since the seminal case of *Serrano v. Priest* (1971) challenging the constitutionality of local property taxation to fund local schools. About one-half of subsequent state courts followed suit, but the other half did not.

The book reports overall and with variety the school finance schemes, differences that reflect the diversity of our federal system and the great variation among the states. Five state experiences illustrate this variety when appellate courts set standards for achieving equality in finances, for example, using such tests as "adequacy" of resources provided.

Against that diversity, John Yinger and colleagues explore the general and specific aspects of state financial reform under these court definitions. Moreover, they undertake individual analyses of reform in the five states in considerable economic analysis. Useful "guides" at the book's end summarize state court decisions and legislative programs in response, all set within a strong empirical economic analysis of school finances.

Over time, these state reforms report the decentralizing forces on policy that are drawn from the political and financial pressures of low- and high-spending districts. These pressures work against the uniform legislative efforts designed to equalize finances against the decentralized pull of local control of schools that still exists. Moreover, this research shows that the main goal of reform, namely, improving student learning, is unclearly settled.

While *Helping Children Left Behind* focuses primarily upon economic analysis of different equalizing reforms, it is also clear that the reform took place within a highly political context that is the norm of American education today. First, the reform arose out of citizen dissatisfaction with the distribution of public school resources. Second, the primary outlet for that dissatisfaction lay in the courts, as these reports point out. Third, there are political dimensions that arose within the legislative and administrative programs designed by the states. This response arose within a tension between state controls of increased spending versus the pull of local control of schools. The clash of downward and upward control was seen in all the cases studied here.

This clash had the differential effect of altering the balance found among the states. It was not surprising that the authors of state responses found a continuing gap between the budgets of high- and low-income districts even after new state laws had been designed to reduce it. The political result was the diversity of laws seeking to reduce this gap. Each side had its own political justification that reflected differing interests about schooling. Federalism theory would expect exactly such differences, due to the socioeconomic structures of the states that found different solutions in programmatic responses.

Looking ahead in this political context, one can see already the forces that work to weaken or increase such effects in school finances. Add to that the centralizing effects of the Leave No Child Behind law as it focuses on raising tests scores and economic penalties. Thus, in late 2004, many states were having problems following the law's control in implementation. This federal law had focused on the value of equality in achievement by use of test results, rather than on adequacy of funds for learning. Those value goals are often in basic conflict, as state experiences report in this book.

Overall, then, Younger and his colleagues have provided an illuminating focus on the conflicting school interests that arise in courts, legislatures, and administration. Its scholarship is first class, its analysis is precise, and its data use compelling. Let the Maxwell School try this approach a decade from now in exploring the role of policy driven by court action.

## COMPARATIVE POLITICS

**Civil Society and Political Change in Asia: Expanding and Contracting Democratic Space.** Edited by Muthiah Alagappa. Stanford: Stanford University Press, 2004. 552p. $85.00 cloth, $34.95 paper.

— Germaine A. Hoston, *University of California, San Diego*

Muthiah Alagappa has assembled an impressive number of essays in this volume, which is the product of two workshops in Honolulu and Phnom Penh held in 2002. It offers contributions covering 12 countries by 14 contributors, organized by country.

Alagappa opens the volume with an extraordinarily lucid critical analysis of competing perspectives on the notion of *civil society* and their relevance to political change in contemporary East Asia. Since the fall of the Berlin Wall in 1989, the notion of "civil society" as the sine qua non of democracy has become the dominant trope in comparative political studies, first of Eastern Europe and then in the self-congratulatory rewriting of the history of democracy in the West, most notably in the United States. Many of these studies have been based on an insufficiently



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reflective acceptance of Alexis de Tocqueville's account of the role of voluntary, nonstate associations in creating democracy. By contrast, Alagappa proceeds from an appreciation of the insights offered by both the Tocquevillean and the "New Left" treatment inspired by Antonio Gramsci's work on hegemony and civil society, and the contributors to the volume consider seriously the possibility that Gramsci's insights on the mutual dependence of state and civil society might offer a more accurate account of the dynamics of the state–society relationship in East Asia. Alagappa defines civil society as "a realm in the interstices of the state, political society, the market, and society at large for organization by nonstate, nonmarket groups that take collective action in the pursuit of the public good; second, a distinct sphere for discourse and construction of normative ideals through interaction among nonstate groups . . . ; autonomous self-governance by nonstate actors in certain issue areas; and, fourth, an instrument for collective action to protect the autonomy of the nonstate public realm" (p. 32).

The findings in this volume cast a critical eye on the common presumption that the state and civil society are necessarily in opposition to one another and the notion that civil society is a category applicable only to the West. In both China and Japan, early in the twentieth century, not only were there labor unions and social movements (which are, inexplicably, excluded from the category of civil society organizations by many analysts) but also study groups, "new religions," and their publications, providing for open public debate. Nevertheless, in late-industrializing Asian societies, which experienced state-led development, the boundaries between state and political society, and between civil society and the state, have been porous. Indeed, there has been a synergistic relationship between civil and political society in many Asian countries, offering evidence that a thriving civil society has, in fact, impeded democratization there. The evidence suggests that democratization may well require a strong state to impose limitations on the kind of violence that renders a civil society quite uncivil.

It is perhaps the South Korea case that offers the most straightforward analogue to the optimistic portrait of the role of civil society organizations as the midwife of democratization. Sunhyuk Kim's article on "South Korea: Confrontational Legacy and Democratic Contributions" also notes that South Korea's democratic transition, along with those of Taiwan and the Philippines (and some Eastern European and African cases), differed significantly from the cases in South Europe and Latin America that have so influenced the literature on democratic transitions. The former were not primarily the product of "conflicts, negotiations, and pacts among political elites" (p. 139), but rather the product of civil society groups that opposed the 1980s of Chun Doo Hwan. (p. 139).

Much of the strength of Korea's civil society derived from resistance to Japanese colonial rule. This point underscores the error of excluding liberationist groups as legitimate civil society groups in a colonial or semicolonial setting on the basis of their refusal to repudiate violence. Where the state is prepared to use violence to suppress all opposition, what chance does civil society have without groups prepared to fight for their rights (as in the American Revolution)? A definition of civil society that excludes, for example, those who made the democratizing revolution in South Africa would condemn the antiapartheid movement as *un*civil, even though it cut across class divisions and ascriptive ties of ethnicity to bring a new democratic system to the land. Such a definition would affirm the injustice of an authoritarian regime rather than support the transition to democracy. It also underlines the significance of international context as a factor determining the growth or inhibition of civil society groups.

In Taiwan, too, we have an example of how an indigenous people's movement invigorated civil society and brought an end to authoritarian rule. Moreover, we see the effect not only of internal civil society organizations but also of the indirect experience of overseas Taiwanese recruiting on U.S. campuses and the lobbying of the U.S. Congress. Yun Fan's essay argues that civil society played a determinative role in the transition "by mobilizing political resistance, constructing counternarratives and ideologies, and marshaling international support for alternative elites" (p. 165). The essay highlights the role of unaligned women's organizations and overseas native Taiwanese, underscoring the limitations of a view of "civil society" as a category that can be isolated from transnational and international influences. These and other civil society groups propelled democratization through involvement in international and transnational organizations, and through efforts by overseas Taiwanese to lobby the U.S. Congress and the United Nations.

This role of international influences recurs throughout the volume. Civil rights groups opposed to British rule, which mushroomed in pre-Hindutva India, are rightly categorized as part of civil society. International nongovernmental organizations forged ties to local humanitarian groups that nurtured the former as they struggled against authoritarian rule; and alienated Malays in Britain became receptive to affirmations of their identity as Muslims emanating from Pakistan.

Where the need to reaffirm cultural identity leads individuals and groups to align themselves with ethnically or religiously exclusive programs, the implications for civil society are far from clear. On the one hand, liberationist groups in colonial Korea, Malaysia, pre-Hindutva India, and the Philippines engendered the emergence of civil society groups that four or five decades later played a major role in the democratization of their respective societies. Yet it is also clear that many such groups laid the



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groundwork for the deepening and institutionalization of ascriptive religious and ethnic ties that would subsequently threaten democratization. This certainly applies to Indonesia (per Edward Aspinall), India (as portrayed by Amitab Behar and Aseem Prakash), Malaysia (see the essay by Meredith L. Weiss), Sri Lanka (in Neil Davotta's contribution), and Pakistan (in the essay by Aqil Shah).

Pakistan, Malaysia, and India offer the most dramatic instances of a key finding of this volume: Civil society organizations can impede as much as encourage democratization. Certainly, fear of this outcome looms large in the eyes of the leadership of contemporary China, as they struggle against the alternative hegemonies counterpoised by religious and minority cultural groups (Mary E. Gallagher, "China," pp. 438ff).

We can only scratch the surface of the impressive studies that form this volume in this review. One wishes for more focused treatments of the major themes, which is impossible to attain in a 500-page volume treating so many different societies. *Civil Society and Political Change in Asia* not only constitutes a major contribution to the study of civil society in East Asia but also helps to blaze the trail for contributions to be made in the future.

**Electoral Competition and Institutional Change in Mexico.** By Caroline C. Beer. Notre Dame: University of Notre Dame Press, 2003. 208p. $45.00 cloth, $20.00 paper.

— Joseph L. Klesner, *Kenyon College*

A growing literature argues that understanding Mexico's protracted transition from one-party rule requires exploring the dynamics of political competition as they emerged at the state and local levels. Caroline Beer makes an important contribution to this approach to Mexican democratization by examining state-level electoral competition in the 1990s and its impact on institutional development, particularly of state legislatures, and on political recruitment, especially of gubernatorial candidates. She argues that democratization resulted from a "complex interplay between opposition victories in subnational elections and important democratic advances in the national political arena" (pp. 9–10).

Beer makes four important contributions in this short monograph. First, she provides valuable evidence of the role of subnational electoral competition in pushing forward Mexico's democratization. Here her work complements the recent scholarship of Todd Eisenstadt, Jonathan Hiskey, and Vikram Chand, as well as the long-standing arguments of such Mexican scholars as Alberto Aziz Nassif and Tonatiuh Guillén López, who suggested that Mexico's transition would be propelled by anticentrist forces. Beer does not argue that subnational competition has developed evenly across the nation. Indeed, she offers evidence that some states remained bastions of the Institutional Revolutionary Party (PRI), even while others became sites of intense electoral competition and, in many cases, opposition victory. She advances the literature on subnational competition, however, by demonstrating the consequences of electoral competition for development of more responsive and accountable institutions at the state and local levels: States with more competition became states with more active legislatures and states in which politicians could begin to build political bases without a reliance on Mexico City–based party leaders. States without competition remained dominated by the PRI's national leadership.

Second, Beer shows that state-level electoral competition has perceptible impact on public policy, particularly in the provision of greater volumes of public goods to citizens of states with higher levels of contestation. This result obtains regardless of whether the PRI or the opposition parties won those state-level elections for governor or the state legislature. To provide the public goods to satisfy an electorate increasingly demanding of its local and state elected officials, those authorities must develop new sources of public revenue. By finding new revenue sources, those authorities also increase their autonomy from Mexico City, which can be a democratic result if state-level politics is itself competitive.

Third, by focusing on the electoral sources of institutional development at the state level, Beer offers a new angle on the study of institutions. Throughout her work, she strives to use an analytical approach that draws heavily on the new institutionalist literature. Where she differs from many proponents of that approach, however, is in her focus on the sources of institutional change, rather than on the political and policy consequences of institutions. The emergence of electoral competition changes the incentives of politicians, thereby leading to institutional changes. In the Mexican context, "electoral competition redistributes power from executives to legislatures, from unaccountable bureaucracies to elected officials, and from party leaders to electorates, thereby creating institutionalized opportunities from the opposition to be represented and to monitor the government in order to check the power of the executive" (p. 21).

Fourth, most scholarship on democratization has centered on the sources of democratic transitions or on democratic consolidation. However, by focusing on subnational processes, Beer's account of Mexican democratization more effectively explores the *consequences* of the democratic transition for such key dimensions of political life as political recruitment and institutional change, especially in regard to executive–legislative relations. Combining cross-sectional analysis of the 31 Mexican states and in-depth case studies of three states—Guanajuato, Hidalgo, and San Luis Potosí—Beer demonstrates that growing electoral competition—again, regardless of which party wins election—has impact on who governs and how. With the advent of effective electoral competition, the state



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legislatures of Guanajuato and San Luis Potosí became more active, sitting for longer sessions, approving larger budgets for their own operations, and increasing the powers of committees. Hidalgo, lacking electoral competition, saw no such parallel developments in the 1990s. In a similar fashion, growing electoral competitiveness has forced the PRI to pay much greater attention to recruiting gubernatorial candidates with more local characteristics so as to more effectively appeal to local electorates. Whereas gubernatorial candidates were once imposed on states by the national PRI and, in particular, by the president, today the PRI uses internal primaries to select its gubernatorial candidates. Opposition candidates have usually been more linked to local communities, too. As a result, today's governors, whether from the PRI or the other parties, are much more representative of the states they govern than the proconsuls once imposed on states by national-level leaders.

The study of Mexico's transition has generated an unusually rich scholarly literature, incorporating many theoretical and methodological perspectives. As yet, no study fully integrates national-level political events and forces with the subnational changes that in many cases led the transition process. Beer has provided a conceptually lucid and carefully researched analysis of the subnational electoral competition and its implications for institutional development at the state level. Any study that seeks to offer a comprehensive view of Mexican democratization must incorporate her insights.

**Interpreting British Governance.** By Mark Bevir and R. A. W. Rhodes. New York: Routledge, 2003. 256p. $150.00 cloth, $40.95 paper.

— Andrew M. Appleton, *Washington State University*

Those tempted to pick up this volume and read a straightforward and unambiguous analysis of British governance in the modern era should consider the following: By the seventh page of the introductory chapter, the authors are engaged in a discussion of the contributions of Michel Foucault to postmodern and interpretive theory (giant, by the way), while narry a drop of ink has been spilled about either Britain or governance. In fact, this book will be of interest to many political scientists who are less than interested in the substantive case, but who are engaged in reflections about the epistemological characteristics of modern political science and the ontological claims that it makes. *Interpreting British Governance* is a fascinating entry into the debates that are raging in our profession, and it stakes out a clear set of claims in unequivocal terms.

The book marries the career interests of the two authors. R. A. W Rhodes is a familiar name to students of British politics and public administration, and has produced an impressive body of work that, read as a whole, promotes a rather agent-centric understanding of the British political system. Mark Bevir is a normative theorist and self-declared critic of positivism, who has written about the epistemology of the social sciences, most notably in *The Logic of the History of Ideas* (1999). Together, they have set out with the goal to "ask, after Coleridge, 'what is the meaning of it,' where 'it' is British governance" (p. 2). Rejecting theoretical approaches more widely used in comparative politics (behavioral, institutional, etc.), they adopt "an anti-foundational epistemology and an interpretive approach to understand changes in British government" (p. 2).

The argument of the book is that the *meanings* (for they are plural) of the British system of governance are actually contained in the multiple traditions of politics in that country. From these traditions, narratives can be woven that encapsulate the contextual frame in which agents are embedded. Indeed, the "anti-foundational epistemology" of the authors lead them to claim the superiority of the concept of the narrative over that of institutions; for example, what countless students of comparative politics have dutifully learned and digested as the Westminster Model of parliamentary government is, in fact, we learn, "a set of ideas that bear strong family resemblances" (p. 25). Just as the narratives of governance are derived from tradition, so too, it appears, can those traditions be inferred from the narratives themselves. The salient traditions for our purposes are Tory, Liberal, Whig, and Socialist, and each spawns different narratives with which agents are infused and which structure their belief systems. As befits the interests of the authors, most of the substantive focus on governance is on the civil service and local government reform.

Ultimately, they make four claims about the superiority of their approach. First, they argue, it is a more complex and more dynamic portrayal of parliamentary government than is to be found in the standard account of the Westminster Model. Second, it frees individuals from the theoretical cage of the institution. Third, it opens up new research agendas for political scientists, while finally, it identifies key theoretical issues such as the pluralizing of decision making.

Taking these claims in reverse order, most students of British politics would strenuously object that other approaches and works have attained the level of claims three and four. Indeed, if the success of any book were to be judged solely on whether it had achieved these objectives, I would hazard that it would be a very modest, though not trivial, contribution at best. The second claim, of course, is one that is part of the most fiercely contested terrain in the study of politics, one that has pitted interpretivist against interpretivist, rational choice theorist against rational choice theorist, and so on. It will probably not endear the authors to many committed institutionalists when they dismiss the New Institutional version of agency as an "unacceptable suggestion" (pp. 198–99), or path dependency as an "unhelpful



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phrase" (p. 199). Nonetheless, I doubt that this volume will convince many institutionalists of the incorrectness of their position.

So what about the first claim? Are we really to accept that the authors have laid the static, unitary gremlins of the Westminster Model to rest and have given us a new and exciting postmodern lens through which we can understand British governance? The biggest value of the book is indeed that it gives a much more complex and nuanced version of the distribution of power in the British system, and that it persuasively argues that narrative views of power are rooted in old traditions. Yet I fear that the dragon that is being slain is really more akin to the Wizard of Oz, a tired and sad caricature. For example, the authors portray (accurately, I think) devolution as one of the most profound reforms of the recent era (p. 59), and seem to triumphantly claim that this demonstrates how the unitary Westminster Model (and by implication, institutionalism) is ill adapted to explaining British governance. Yet reading this carefully, nowhere did I find an explanation of how narratives can explain this momentous shift in British political institutions.

Indeed, for all its ambition, *Interpreting British Governance* is finally rather unsatisfying. One does not have to be an irredentist institutionalist to wish for a stronger and more compelling link to be drawn between the images and narratives held by agents and aggregate and institutional outcomes. Scottish and Welsh readers of this review irked by my comment about the slaying of the dragon will be even more dismayed to find the scant attention paid to their nations in a book that promotes as a virtue its portrayal of the pluralization of power. To this reviewer, the book could be portrayed as much about political culture as anything else.

Ultimately, the book has much to commend it. Those engaged in the battle against the perceived stranglehold that positivism has upon our profession will be delighted by it and will no doubt embrace it as an exemplar of antifoundational scholarship. Those more schooled in the positivist tradition will, I fear, be singularly unimpressed with the claims made of epistemological superiority. All who care about such matters should read it and be the judge for themselves. The authors are to be commended for a sharp polemic written with wisdom and authority; now let the debate be enjoined.

**The Nationalization of Politics: The Formation of National Electorates and Party Systems in Western Europe.** By Daniele Caramani. Cambridge: Cambridge University Press, 2004. 347p. $75.00 cloth, $28.99 paper.

— Alice H. Cooper, *University of Mississippi*

Daniele Caramani endeavors to document and explain the nationalization of party systems in Western Europe, the process by which the localized and territorialized politics of the nineteenth century (clientelistic politics dominated by local personalities) was replaced by nationwide functional alignments on the basis of class, in particular. This nationalization process was a crucial step in the structuring of party systems in the first century of electoral (democratic) politics. Although central to Western European electoral developments, it has received little scholarly attention, a gap that Caramani means to fill through empirical verification.

This study rests on an enormous data set, published separately by the author (*Elections in Western Europe Since 1815,* 2000), which encompasses electoral history in 17 Western European countries since 1815 and is disaggregated down to the constituency level. The data permit him to conduct extensive longitudinal analyses. He thereby studies territorial variations in electoral behavior (turnout and party support) within countries—across time, countries, and party families. The party systems of each of the 17 countries are analyzed in these terms, a project that comprises about one third of the book.

Caramani analyzes political cleavages as they are configured on the territorial space. The data attest to a general process of national political integration, a gradual homogenization of electoral behavior within countries and across Europe. Nationalization of electorates and party systems within each country, as well as the growing similarity between countries, he argues, largely resulted from the ascendance of the left–right class cleavage over the main preindustrial cleavages of religion, ethnicity, and so on; this ascendance was in turn the consequence of industrialization and the mobilization associated with the extension of suffrage. These processes led to liberal, conservative, and social democratic party families that were not territorially based. This process was relatively rapid up until World War I, after which political alignments were largely "frozen" from the 1920s on. On the other hand, regional cleavages have not entirely disappeared from European party systems. State formation and industrialization did not completely eliminate territorial diversity. Instead, several preindustrial cultural cleavages (religious and ethnolinguistic) have maintained at least some strength in European party systems despite general homogenization, resulting in the establishment and survival of religious, regional, and agrarian parties. Such parties have the highest levels of territoriality. The survival of such cleavages varies across countries according to patterns of state formation and nation building. Caramani traces the historical reasons for these developments, based on an admirably comprehensive grasp of the literatures on state building, secularization, industrialization, and democratization processes of European history.

But nationalization was also, as he further argues, the result of parties' competitive strategies as they tried to expand their reach throughout their respective national territories. In addition to the supremacy of functional



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left–right alignments, the nationalization of European electorates and party systems also resulted from electoral competition and parties' electoral strategies. In order to mobilize peripheral electorates, parties developed networks of local organizations controlled by national party headquarters. This happened early on; functional alignments arose even before mass suffrage, allowing liberal and conservative parties to cover national territories before suffrage extension. Liberals and conservatives penetrated each other's strongholds in search of votes. Competition guided strategic behavior of parties, starting with the advent of free elections, even before the rise of the social democrats or institutional modifications, such as proportional representation (PR). Thus, nationalization of electorates and party systems, Caramani argues, took place early as the original two-party families competed against each other, before the changes that previous literature identified as the main causes of nationalization: universal suffrage, PR, the Great Depression, and so on. Instead, nationalization dates back to the early stages of cleavage formation and party systems, with territorial diversity of voting behavior falling steeply until World War I and stabilizing thereafter. Only in a few cases, such as Belgium, Britain and Italy, does significant "re-regionalization" reappear after World War II.

*The Nationalization of Politics* rests squarely in the tradition of the cleavage model of party systems originally set out by Seymour Martin Lipset and Stein Rokkan (*Party Systems and Voter Alignments,* 1967). Caramani is deeply versed in the multidisciplinary literature relevant to this model, and his interpretation of the data makes a nuanced and welcome contribution to the party systems literature.

**Native to the Nation: Disciplining Landscapes and Bodies in Australia.** By Allaine Cerwonka. Minneapolis: University of Minnesota Press, 2004. 269p. $74.95 cloth, $24.95 paper.

— James Jupp, *Australian National University*

It is unusual for a book on Australia to be written by an outsider and published by an American press. This is to be welcomed. Outsiders are often free of local myths and attitudes, which are well entrenched in Australia, despite its character as a relatively new nation. Allaine Cerwonka did intensive fieldwork in two inner-city suburbs of Melbourne, Australia's second city. As a resident for 10 years in neighboring Carlton, I found her sensitivity to the nuances of the complex areas of East Melbourne and Fitzroy quite unusual.

However, she was also sold the particular minority ideology of the importance of "native" plants to defining Australian identity. This view is strongly held by one group of her respondents, the middle-class "gentrifiers" of the East Melbourne Garden Club. Though she does not say so, I suspect that her other reference group, the Fitzroy police, would have responded with skepticism to most of the attitudes of those from East Melbourne, just across the road.

This interesting and innovative book can be read on several levels. It is essentially a postmodernist text, with frequent references to Pierre Bourdieu and Michel Foucault and the local Ghassan Hage. Australia is described as a "postcolonial society"—which is historically correct but not necessarily very helpful. Three quarters of its people descend from British and Irish immigrants who brought older cultures with them, which is not the case in "postcolonial" India or Africa. Indeed, that is why they planted "English cottage gardens," as the author puts it.

Like many radical and, indeed, moderate Australians, the author is concerned with the severely disadvantaged state of the Aboriginal people who lived throughout the continent until 1788. But modern Aborigines make up less than 0.4% of the East Melbourne and Fitzroy population. Concern for them is a very abstract issue. It is possible to live in the Melbourne suburbs for many years and never encounter an Aborigine.

The extreme view that Australians can only find themselves by identifying with Aboriginal culture—as developed recently by Australian-born feminist Germaine Greer—is dangerously close to nonsense. The author seems at least sympathetic to this idea, if not completely convinced. The Fitzroy police, on the other hand, saw the small local Aboriginal community as a social and criminal problem.

The author's attempt to explain this tension in terms of Aboriginal alienation from their traditional land is stretching reality. Fitzroy has for a long time attracted Aborigines because it contains the central offices of several Aboriginal and general welfare agencies—not because it is in any way a "sacred site." In fact, Fitzroy does not "belong" to anyone. Its population has been recycled several times since it was built up between 1850 and 1890. It now includes many teachers and public servants, who have replaced its previous manual-laboring tenants, and many Vietnamese and Hispanics, who have replaced its British and Irish descendants. These transformations are less true for East Melbourne, which retained a middle-class core even when its neighboring suburbs encroached on it.

But this is not essentially a study of urban ecology or even of community relations. The Fitzroy police live in other suburbs, while the East Melbourne gardeners are rather contemptuous of their more proletarian (and less British) neighbors. The central theme concerns national identity and the attempts to assert it by the majority British-derived population. This is an old obsession in Australia, but the (outsider) author brings new understanding to it. Her focus is on the movement to replace "exotic" plants and gardens with "indigenous" trees and shrubs, and the prejudices of the Fitzroy police against Aborigines and



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Vietnamese. Samuel Huntington caused some amusement in Australia by describing it as a society "torn" between Asia and the Atlantic world. The analysis here is more sensitive and closer to reality.

The author naturally has her own interpretation of what she observes, but this is tempered by the strongly held views of her informants. She accepts much of the anti-English blame attached to everything from exotic gardens to the rectangular layout of most Australian towns, including Melbourne. But rectangular towns are quite "unEnglish" and largely confined to military centers like Aldershot or Catterick. This military planning was the cause of rectangular layouts, rather than anything especially "English."

The notion of the English love for gardens is much more plausible. That this is somehow a denial of the Australian environment is much more controversial. Towns, cities, and even fixed housing are all alien to Australia before 1788. The gum tree—a noble sight in eastern Australia—is unsuitable for densely populated urban areas. It dehydrates the soil, its leaves and bark fall year-round, and its limbs drop off without warning—a threat not only to passersby but to a tram and rail system dependent on overhead wiring.

Because *Native to the Nation* is polemical, it is also annoying. Because it overtheorizes, it can sometimes ignore simple explanations. It can be historically wrong, for example, in seeing Asians as uniquely singled out as "dirty and criminal." The Irish, Jews, Greeks, and Italians had all previously been through this denigration. It gives too much credence to the convict influence, which was very weak in Victoria and South Australia. But it is very thorough and accurately represents the views of those with whom it came into contact. It is a refreshing and thought-provoking work that brightens up the rather tired debate on Australian national identity.

**Shocking Mother Russia: Democratization, Social Rights, and Pension Reform in Russia, 1990–2001.**
By Andrea Chandler. Toronto: University of Toronto Press, 2004. 260p. $60.00.

— Aron Tannenbaum, *Lander University*

The pension system of a given state is a mirror that reflects many dimensions of that state's political processes, political and social values, historical experience, and economic conditions. Had Andrea Chandler presented her analysis of Russian pension politics oriented toward those four categories or analogous ones, her study would have been much more understandable and would have made a greater contribution to our understanding of post–Soviet Russian politics. Instead, Chandler presents a mostly chronologically based account of Russian pension politics that makes a significant contribution to the subject but one that is harder to understand than it needs to be. The reader must tease out the important themes, such as democratization, from a book that is more chronological than thematic. *Shocking Mother Russia* is less than the sum of its valuable parts.

The first and most significant contribution of Chandler's book is demonstrating that the Russian pension crises of the post-Soviet period are not entirely due to poor economic conditions. True, economic conditions were weak, but the author convincingly argues that state institutions were even weaker.

Chandler's decision to take a state-building approach to Russian pension reform contributes greatly to our knowledge of state building in Russia in general. As the author notes, her book makes its contribution by "presenting a concrete case study of a specific problem in Russian governance" (p. 158). Especially strong are the detailed discussions of executive-legislative relations and of bureaucratic obstacles to effective pension administration that bedeviled the Russian pensioners' ability to survive shock therapy. (Shock therapy is the "shocking" of the book's title.) One entire chapter is devoted to pension reform law FZ-113, passed in 1997. The chapter explores the intricacies of executive-legislative relations so well that it stands as a very good case study of a Russian version of "how a bill becomes a law."

Chandler presents a well-researched appreciation of the role of the Duma in pension reform during the 1990s. The Duma of that period, poorly regarded in the West, actually acted in pension matters with greater overall responsibility, she finds, than the executive branch did. But it is difficult to speak of the Duma as a coherent institution when it was riven by so many political and ideological factions, some of which did not accept the legitimacy of the post–Soviet Russian state, much less the legitimacy of other Duma factions. Therefore, it would have been valuable to supplement a discussion of the Duma as a whole with an analysis of the major political parties, such as they were, on the question of pension reform. Unfortunately, there is no such analysis. Perhaps the state-building model unduly limited this study's scope. There are a half-dozen interesting charts of party discipline voting on pension reform bills (pp. 99–104) but, maddeningly, no discussion of which parties supported or opposed proposed pension reforms. There is no strong discussion of internal debates within political parties on pension politics. Nearly completely missing is the use of voting studies data, including exit polling, that could have shed an enormous amount of light on pension politics. When one remembers that pensioners comprised a very large proportion of the 1996 presidential election vote for Gennadi Zyuganov of the Communist Party of the Russian Federation, the lack of discussion of political party pension politics is all the more regrettable.

Chandler's work on pension politics sheds new light on Soviet ideological debates in the New Economic Policy (NEP) 1920s, the Stalinist 1930s, the de-Stalinizing



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1950s, and the perestroika 1980s. This discussion is an important contribution in its own right. Soviet pension legacies are of more than historical interest. The author reasonably contends that the Soviet historical experience with pensions is important in molding the post-Soviet public's support for a given pension system or reform thereof. She raises this issue most directly in the last two pages of the last chapter of the book: "Finally, pension reformers must not ignore or dismiss the attachment that people may have to the existing pension system. This fondness transcends simple material interest. Pension systems make a normative statement about what the state owes its citizens. . . . Perhaps the most serious problem with the capitalist approach to pension reform was that it insufficiently considered the importance of history" (p. 160). If this is indeed an important lesson of the Russian experience, then one would expect the author to explore the perceptions and values of the Russian public toward their Soviet legacy system, as well as their expectations for a new post-Soviet social pension contract. There is good exploration at the level of ideology, very good exploration at the level of policy, but hardly any exploration at the level of mass opinion or behavior. We have public opinion data, of varying quality, going back at least to the Smolensk Archives, to say nothing of perestroika and postcommunist mass polling. There is also voting data from the late 1980s onward. But these data sources are not mined in this book. Once again its significant contribution could have been even greater.

Is it fair to criticize a significant contribution to the literature because it adopts one analytic framework (in this case, state building) rather than another (such as voting behavior or public opinion analysis)? Yes, if the emphasis in the book is on a substantive policy area that requires the use of all relevant data. Even though shock therapy turned out, in the well-worn phrase, to be "all shock and no therapy," the shock percolated throughout post–Soviet Russian politics, not just institutionally but behaviorally and normatively as well.

**Voter Turnout and the Dynamics of Electoral Competition in Established Democracies Since 1945.**
By Mark N. Franklin. Cambridge: Cambridge University Press, 2004. 294p. $70.00 cloth, $24.99 paper.

— Joop J. M. Van Holsteyn, *Leiden University*

According to Mark Franklin, "As the vexing questions of political science can be regarded as puzzles, the particular topic of voter turnout could be called the 'grand enchilada' of puzzles of political science. . . . [A]lmost everything about voter turnout is puzzling, from the question of why anyone bothers to vote at all to the question of why certain variables appear to explain voter turnout in some circumstances but not in others." He makes this bold but true statement in the preface (p. xi) of his comparative study of turnout in more than 20 established democracies since World War II. Voter turnout is one of the more frequently studied topics in political science, but the results of all these scholarly efforts remain rather poor and unsatisfactory. We still know distressingly little about why some people vote and others do not, why turnout is much higher in some countries than in others, and why in so many countries there seems to be a steady and irrevocable decline in turnout in national elections.

"Me too," Mark Franklin must have thought. With the assistance of a handful of colleagues he attacked the challenge of solving the turnout puzzle. And it may be stated immediately that he has to a considerable extent succeeded. He has managed to put many of the pieces of the turnout puzzle in the right place. That is no small achievement.

For some decades now, the study of turnout has been dominated by the rational choice approach. As others have done earlier, Franklin shows that this particular perspective is incomplete. The rational choice approach—"A Theory on the Calculus of Voting" by William H. Riker and Peter C. Ordeshook (*American Political Science Review* 62 [1968]: 25–42) seems to represent this school of thought—should be amended and complemented.

Franklin does so by suggesting that the political context of elections, the social contexts of the individual voter, and the socialization of voters are of crucial importance. He acknowledges the simple fact that "people get set in their ways. The longer some pattern of behavior has been pursued, the harder it is to change" (p. 21). This general rule can be applied to elections and voting behavior as well. As a consequence, in the study of turnout, special attention should be paid to new cohorts of voters. What do they experience in their first elections? At what age are they eligible to vote and how important are elections for them at that stage of their lives? Is there much at stake in these elections, are the elections competitive and is there a high degree of "executive responsiveness," and can people as a result of this easily be mobilized when they are eligible to vote for the very first time? If their second and third elections have the same excitement and stakes, the cohort will for the rest of their electoral lives have a high turnout. "In the end there is no escaping the fact that both rationality and socialization must play a role in turnout" the author states (p. 25). Via his line of reasoning that is reminiscent of the analysis of value orientations and priorities as the driving force of the Silent Revolution once proclaimed by Ronald Inglehart, we can understand contemporary turnout and trends in turnout if we know what happened in the past: "[T]hose whose first opportunity to vote comes at a low-turnout election will retain a profile of lower turnout in subsequent elections, even elections in which yet newer cohorts vote at a higher rate; so the past leaves a 'footprint' in subsequent elections that



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reflects the low turnout of an earlier period" (p. 43). And while the voter in the stereotypical rational choice approach is an atomized individual with no good reason to vote, for the socially connected voter, the benefits of voting, not exclusively in terms of the outcome of the election, and the costs of nonvoting may be relatively high. In the real world with high uncertainty and competitive elections—that is, the result is close and/or the outcome of the election may lead to a major policy change—this may result in high turnout among the cohort of new voters in particular.

In the perspective advocated by Franklin, the focus is not so much on the characteristics of individual voters, as is often the case in the explanation of turnout, but primarily on the character of elections: "[O]ur model implies that turnout will be predictable, in the case of any specific election, on the basis of the character of that election, not the character of the individuals voting in that election" (p. 58). Turnout can best be understood when we know the character of elections and the influence this has on new cohorts of voters.

This basic idea and some of its implications are tested in various ways on three different levels of analysis and make use of different models and data sets. Special attention is paid to some of the oldest democracies in the world, the United States and Switzerland, and the "problems" these two countries have with regard to turnout. Franklin shows that an interpretation in which executive responsiveness—that is, "the extent to which the political complexion of the executive is responsive to the choices made at the time of an election" (p. 96)—plays (via short-term effects as well as cumulative effects) a vital role is plausible and is supported by the data. Also in the analysis of turnout change in 22 countries in the postwar period, this aspect is of utmost importance: "Turnout change is not brought about by changes in the character of society or of its members. . . . Turnout declines, if it does, because elections change their character" (p. 147). A more exploratory case study of turnout in Germany at the individual level only supports this finding.

In the concluding chapter, Franklin sums up the three most important lessons of his analysis: There is nothing inevitable about declining voter turnout, as it has occurred partly as a reaction to political reasons; turnout decline is "in no way due to any decline in civic virtue or increase in political disaffection" (p. 215); and as is often the case, the future is in the hands of the young, because they are the ones who react to new conditions and the specific circumstances when they enter the electorate. Yes, this may sound a bit obvious. But it is not—Franklin only makes it seem obvious through the lucid description of his interesting theoretical notions and a thorough but transparent data analysis. Of course, the turnout puzzle is not solved completely, but it has to be admitted that Franklin has gone a long way.

**Judicial Review and Bureaucratic Impact: International and Interdisciplinary Perspectives.**
Edited by Marc Hertogh and Simon Halliday. New York: Cambridge University Press, 2004. 328p. $90.00 cloth, $39.99 paper.

— Susan Rose-Ackerman, *Yale University*

The impact of judicial decisions on bureaucracy is poorly understood. Some claim that court judgments have little or no effect. In contrast, others worry that judges will usurp the policymaking role of the political branches and override the expertise and experience of civil servants. As this thoughtful and wide-ranging collection of papers demonstrates, both claims are exaggerated.

"Judicial review" is not a uniform activity, especially in a comparative context. The role of courts depends on other features of the constitutional and statutory structure and on the nature of the public programs and policies subject to review. For American scholars who study the relationship between courts and bureaucracies, the chief interest of the book will be the comparative chapters that provide insights into the situation in several other industrialized countries, mostly in the British Commonwealth (the United Kingdom, Canada, and Australia, plus Israel).

In the United States, the concept of judicial review is often reserved for federal court review of statutes, regulations, and other agency actions for conformity with the U.S. Constitution. That is not how the term is used in this volume. Rather, it refers to the whole range of judicial oversight of bureaucratic behavior for conformity with the law. It covers both constitutional and nonconstitutional review and ranges from challenges to individual bureaucratic actions to claims that broad policies expressed in rules and regulations are legally defective. The volume, however, concentrates on judicial review of organizations whose programs touch a large number of people, such as immigration authorities, prisons, and welfare programs for the poor. The emphasis is not on the substance of court decisions but on the way bureaucracies respond to losses in court, both with respect to the individual case in question and in terms of the overall program.

The conceptual chapters by Peter Cane, Maurice Sunkin, and Bradley C. Canon clarify the alternative meanings of judicial review and canvass ways to study its impact. Cane makes an important distinction between judicial review under the English parliamentary system with no written constitution and under the U.S. separation-of-powers Constitution. However, he overemphasizes the role of independent regulatory agencies in the American context. Judicial review in the United States is not, at present, "overwhelmingly preoccupied with controlling the activities of independent regulatory agencies" (p. 20). Rather, many of the most important recent cases have concerned agencies such as the Environmental Protection Agency or the Occupational Safety and Health Administration that are part of the cabinet structure of government under the



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president. Nevertheless, Cane does draw an important contrast between the willingness of U.S. courts to review agency interpretation of statutes and the focus of English judges on protecting the rights and interests of individuals. In a parliamentary system, if a regulation does not conform to a law, the law can easily be changed to conform to the regulation. Thus, the UK courts are not much concerned with statutory interpretation. Judicial review in the United States also seeks to protect individual rights, as Malcolm Feeley's chapter on several prison-reform cases makes clear, but Cane's observation about the relative importance of statutory interpretation is surely correct. Australia is a hybrid, a country with a written constitution and a Westminster political system. However, judicial review there is mostly about the redress of individual grievances, suggesting that it is the parliamentary structure, not the unwritten constitution, that is the key. Martin Shapiro's chapter contrasting the European Union and the United States supports this hunch. Shapiro claims that the EU, with its complex constitutional structure, will be pushed by the European Court of Justice and public opinion toward a more participatory and transparent form of rulemaking that approaches the United States model.

The major differences in what the courts do when they review bureaucratic actions obviously have implications for analyses of the impact of judicial review. Cane is ultimately rather pessimistic that impact studies can be done successfully because of the role of noninstrumental factors. Sunkin is more optimistic, perhaps because he has actually attempted to measure impact in a study of social security appeals in the UK. He shows both that bureaucracies do sometimes reform in light of court judgments and that these actions can be studied systematically. Canon reviews research on the bureaucratic implementation of court judgments in the United States with a focus on "indifferent or hostile agencies" (p. 81). He shows how agency officials can resist and undermine the impact of court decisions if they are incompatible with their own views.

Subsequent chapters summarize case studies that demonstrate that agencies do respond to court judgments but that the responses are diverse and context specific. Sometimes an agency head actually welcomes an adverse judgment because it may accord with his or her view of the law or may give the agency added ammunition to seek additional funding—for example, to reduce overcrowding in prisons or hire additional professional staff. Feeley's review of aggressive efforts by U.S. federal judges to reform state prison systems provides some examples where agency personnel assisted the courts. In other cases, however, court impact is limited. For example, Geneva Richardson's study of the judicial review of the UK's Mental Health Review Tribunal (MHRT) shows that judicial review had only a patchy impact, in part because of the ongoing role of MHRT psychiatrists whose medical norms of decision making differed from those of judges. Lorne Sossin's study of Canada found a number of cases where judicial decisions helped increase the coherence and accountability of street-level bureaucrats. This result occurred when the decisions were communicated transparently and backed up by clear changes in the way superiors made decisions. Robin Creye and John McMillan show that in Australia, adverse decisions in individual cases do often benefit the plaintiff. Although many complained of time delays, in more than half the cases the applicant obtained a favorable outcome. This result, although encouraging, does not mean that the agency moved to rethink its overall practices. Almost half the agency personnel claimed to have done so, however, although the survey responses suggest that this may merely imply greater efforts to settle with recalcitrant applicants to avoid a court judgment. Finally, Yoav Dotan's study of the period in Israel during which individual cases of torture were subject to court approval is a particularly dramatic example of the problems that arise when courts get involved in the routine balancing of incommensurate values. The end result was a supreme court decision to ban torture altogether.

In short, the collection of essays in *Judicial Review and Bureaucratic Impact* is a fine overview of the state of research in the law and society tradition with a nod to political science and administrative law. As such, it draws mostly on models from sociology that focus on the operation of bureaucracies dispensing benefits or imposing costs on large numbers of people. The issue of whether such agencies can be induced to change through lawsuits brought by individual clients is in the background of the essays collected here, coming to the fore in Feeley's report on the U.S. courts' use of special masters to oversee structural change. In the absence of such draconian measures, which have their own problems in practice, the limits of judicial review are clearly on view in many of the chapters. Nevertheless, the hypothesis of no impact is clearly rejected, even if one's evaluation of the actual impacts in particular cases is mixed and context specific.

**Becoming Europe: Immigration, Integration, and the Welfare State.** By Patrick Ireland. Pittsburgh: Pittsburgh University Press, 2004. 288p. $27.95.

— Antje Wiener, *Queen's University Belfast*

Patrick Ireland begins this detailed and clearly presented discussion about belonging in relation to patterns of European Union migration by raising a critical issue with the concept of ethnicity, defined as a "collective group consciousness that imparts a sense of belonging derived from membership in a community bound putatively by common descent and culture" (pp. 2–3). Ireland's study finds that belonging is context specific, rather than derived from common descent. This finding implies that there is by definition no readily available general policy applicable to one type of immigrants, say, Turks or Moroccans



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across all EU member states. Instead, each country's possibilities for integration differ. To Ireland, ethnicity "is not so much a category as a dynamic, elastic entity. Its value as a social, economic, and political resource varies; the appraisal depends considerably on institutions and policies." (pp. 4–5).

This citation places the book's approach in a conceptual context that allows two key issues of central importance for migration research to be addressed: first, the challenge to theorize belonging in a way that avoids the modern ontologies of political borders or cultural myths; and second, the normative proposition that research on immigration theorizes migrants as citizens-to-be, rather than human resources to be controlled. This is a task with considerable potential for migration and citizenship studies alike. The author engages in raising the standard of migration studies from institutionalist stock taking, which usually prefers snapshots, toward a normative dimension that entails the possibility of change. To do that, he asks how institutional factors and social policies have shaped opportunities for ethnic identity formation and ethnic maintenance (p. 5). The answer to this question is pursued in some empirical detail over six chapters, including case studies on Germany (Chapter 2), German cities and city-states (Chapter 3), the Netherlands (Chapter 4) and Belgium (Chapter 5).

The shift in research focus from managing migration at the gates of entry toward understanding conditions for integration based on daily practices implies a change of method. That is, instead of analyzing state decision making and government policies stressing external state borders, Ireland sets out to investigate what he calls "street-level dynamics" (p. 14). By analyzing social policy data, including educational and social welfare systems, as well as political participation in local communities, he highlights the role of internal cultural boundaries. While political boundaries are the stable remnants of the Westphalian state system, cultural boundaries are flexible and reflect the specific local contextual conditions. This view enables and encourages thinking and policy making that considers the possibility of cultural diversity based on "peaceful ethnic relations" within political communities and "promoted by the postwar European welfare state" (p. 15). It offers an approach to the study of integration based on structural (labor market, education, social services, etc.) and political-cultural (modes of participation, inclusion, and cultural exchange) dimensions for both academics and practitioners (pp. 17–18).

Practitioners will find this book's insight into the connection between "social policy restructuring and the transformation of migration" (p. 14) a particularly welcome addition to a research field that has predominantly addressed either migration or citizenship and rarely sought to reconcile the significant and, as this book argues, consequential interrelations between both. Academics are recommended to consider the book's findings as an invitation to leave behind exclusive research on either migration or citizenship and begin to merge not only research programs but also the academic discussion about collaborative research building, among others, on the work of William Barbieri, Klaus Eder, Adrian Favell, Martin Heisler, James Hollifield, Rey Koslowski, and Yasemin Soysal. Ireland's "street-level" perspective on immigrant group behavior follows the insight that "rights do not equal practice" (p. 25). A key *axiom* of reflexive citizenship studies holds that "the rule lies essentially *in* the practice" (Charles Taylor, "To Follow a Rule . . ." in Calhoun et al., eds., *Critical Perspectives,* 1993) and evolves from it, as critical citizenship studies have pointed out (especially Seyla Benhabib, Jane Jenson, Ruth Lister, Carol Pateman, Anna Yeatman, and Iris Marion Young).

The book's three national (Germany, Belgium and the Netherlands) and eight city case studies (Bremen, Berlin, Essen, Nuremberg; Liege, Ghent; Rotterdam, The Hague) produce the insight that "specific ethnic traits could not be responsible for the shape of social relations in the cases considered" (p. 210). Ireland contends that this finding "contradicts widely held cultural explanations for the ethnic conflict rightly or wrongly associated with the presence of immigrants" (p. 210). The proposition that institutional factors and policies are more influential than, for example, national identities has lead to the important insight that the "Turks who were seen as difficult in German cities were viewed as relatively unproblematic in their Dutch and Belgian equivalents" (p. 211). This conclusion is consistent with a reflexive approach to institutional change and identity formation, which holds that as subject positions, identities and hence feelings of belonging are always part of social practice and cultural representation (Eder). They are therefore not readily transferable from one political community to another.

It would have been interesting to see a more detailed discussion of the five "visions of multiculturalism" presented in Chapter 1, including liberal nationalism, liberal neutrality, liberal multiculturalism, cultural pluralism, and communitarian pluralism (pp. 14, 222–24) and a link developed between the conceptual significance of these models and the conclusive observation that "foreigners are becoming Europe" (p. 234). It is here where the research program laid out by Ireland's excellent work could embark into an even closer collaboration with the citizenship scholarship. While keeping with institutional change and social policy, such a focus would allow for a more consistent and rigorous assessment of the possibilities of migration by drawing more explicitly on the normative dimension. Minor issues such as the potential inclusion of a chapter on methodology and the addition of an appendix including the list of expert interviews conducted prior to and throughout the duration of the fieldwork notwithstanding, *Becoming Europe* is recommended as required reading



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for both undergraduate and graduate coursework. In addition, it should be widely consulted by practitioners.

**Before Norms: Institutions and Civic Culture.** By Robert W. Jackman and Ross A. Miller. Ann Arbor: University of Michigan Press, 2004. 262p. $70.00 cloth, $22.95 paper.

— Marc Hooghe, *Catholic University of Leuven*

With this volume, Robert Jackman and Ross Miller pursue a twofold ambition. First, they want to demonstrate that most of the research on political culture and the impact of values on political behavior can be questioned, both on theoretical as on methodological grounds. Second, they want to demonstrate that their theoretical model, based on the assumption that actors optimize their behavior within durable institutional restraints, is better able to explain what people do with regard to politics.

The book starts with an all-out attack on the work of various authors using cultural traditions and values as a determinant of political behavior. Jackman and Miller question the research results of authors as diverse as Max Weber, Gabriel Almond, and Sidney Verba, Robert Putnam, and Ronald Inglehart. Subsequently, the authors present some of their own research in an effort to demonstrate that an approach based on institutions can predict voter turnout or the success of extreme right-wing parties. It is important to note here that they do not adhere to a strict rational choice paradigm, and they are indeed quite critical about the more narrow interpretations of this paradigm. Their assumption is that the goals that actors pursue are not preestablished, and therefore these goals will show a considerable degree of variance. According to Jackman and Miller, however, actors will try to optimize their pursuit of these goals, given a specific set of institutional constraints that they are not able to change themselves. In this respect, their work fits quite closely in the "embedded rationalism" approach.

While in the introduction and the conclusion the authors claim that both of their goals are equally important, one cannot help noticing a certain imbalance in the book. Almost three quarters is devoted to their criticism of what Alejandro Portes labeled the "value enactment" approach, leaving only one quarter of the text to present the merits of their own institutionalist approach. Somehow, this is reminiscent of the reports one sometimes receives from academic journals, where the comments made by the reviewer are longer and more elaborated than the original manuscript itself. A further weakness is that the evidence they present to substantiate their claim is rather limited. First, they demonstrate that institutional constraints determine voter turnout, and second, they explain the success of extreme right parties in Europe using various institutional measurements. So in both instances they explain electoral behavior, and this form of political participation is notoriously easy to manipulate, using electoral engineering. It is self-evident that in a disproportional electoral system, it is not an attractive option to vote for a small extremist party, while this is more easily done within a proportional system.

Nevertheless, the chapters in which Jackman and Miller offer criticism against the cultural approach are very instructive (some of this material was already published in various journals). The authors do not leave a stone unturned to demonstrate that the cultural approach does not just lead to cultural conservatism, but that it is also based on wrong interpretations of the data at hand.

For example, Max Weber based his thesis about the economic consequences of Protestantism, among other things, on the observation that in the German state of Baden, Protestant children were enrolled in more job market–oriented schools than Catholic children. The authors demonstrate, quite correctly, that Weber did not take into account the fact that there happened to be more of these schools in the predominantly Protestant areas of Baden than in the predominantly Catholic ones. Based on this observation, they offer the institutionalist explanation that Protestant parents simply sent their children to these schools because they happened to be more readily available in their neighborhoods. Their observations no doubt are correct, and indeed they have been made earlier, and one should have no illusions about the statistical expertise available to Weber almost a century ago. The question, however, is whether they entail the theoretical implications that they claim they have. First, Jackman and Miller do not offer any explanation for the presence of these schools in these districts. One could assume that more of them were established in Protestant districts exactly because of the larger demand for this kind of education in these areas. The general point that is missing is that if their claim that institutions constrain human behavior is correct, this just leads to the research question: How were these institutions established in the first place? Second, even if Weber got his figures about school enrollment in the state of Baden completely wrong, does this invalidate his theoretical claim in *The Protestant Ethic and the Spirit of Capitalism* (1905)? During the past century, this book has proved to be immensely influential and inspiring, and this will remain the case, no matter what the correct school enrollment figures in Baden might have been. Some classic volumes remain inspiring, even if the quality of the fieldwork does not live up to current standards.

Despite the methodological rigor and expertise, the material presented in *Before Norms* does not live up entirely to the expectations the authors created themselves. Jackman and Miller clearly claim that their embedded rationalism model is superior in explaining political behavior than the civic culture model: "We believe that the institutional account easily outperforms the cultural interpretation" (p. 200). What they do in this volume, however, is to offer criticism on some of the work of Weber, Putnam,



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and Inglehart, three authors who dealt with very large questions of social transformation. They limit the application of their own model, on the other hand, to two rather limited cases of electoral behavior. The real test for the alleged superiority of their institutionalist paradigm would be to know whether they are better able to explain the variance in government performance of regional Italian governments than Putnam did using cultural explanations in *Making Democracy Work* (1993), a volume that is heavily criticized by them. In this book at least, Jackman and Miller, do not make any effort to deliver that proof.

**The Rules of Federalism: Institutions and Regulatory Politics in the EU and Beyond.** By Daniel Kelemen. Cambridge, MA: Harvard University Press, 2004. 256p. $49.95.

— Jefferey M. Sellers, *University of Southern California*

In this engaging, clearly written volume, Daniel Kelemen synthesizes and elaborates a theoretical model that contributes to several literatures. As a study of the European Union, it stands at the logical endpoint of a growing trend toward the application of analytical tools drawn from conventional comparative politics. As an account of governmental institutions, the broad analytic narrative breaks new ground in sorting out the consequences of federalism. As an analysis of public policy, especially environmental policy, the book offers a new argument about the institutional sources of regulatory politics.

At the core of Kelemen's account lies a theory of what he calls "regulatory federalism." By this he means the ways regulation develops under a government with two tiers of institutions. Synthesizing accounts of executive-legislative relations, national and subnational governments, and judicial institutions, the theory highlights two basic dynamics. The first resides in the hierarchical relations between the federal government and lower-level units. The second, termed by Kelemen the "politics of discretion," takes place as governments at both levels, as well as courts and interested parties, jockey with one another to implement regulation.

In the development of a new sphere of regulation, these dynamics follow one of two paths. In systems in which power at the federal level is fragmented, the federal government employs detailed statutes to constrict the discretion of both the federal executive and the subnational governments in implementation. In the face of a relatively weak executive, courts step in and encourage litigation. In centralized federal systems, contrasting dynamics take place. The government enacts vague enabling statutes, assuring both the federal executive and subnational governments wide discretion. In the absence of legal grounds for intervention, courts defer more to the executive and litigation plays a minimal role. A series of broadly framed case studies focused on the development of environmental regulation since the postwar era demonstrate these alternative dynamics. Kelemen finds the first trajectory in the United States and the EU, and the second in Canada and Australia. In Germany, the path he discovers lies between his two main types.

As a comparative study of the institutional politics of regulation, *The Rules of Federalism* is in several ways an exemplary work. Rarely has a comparative framework done more to illuminate the properties of the European Union, or to put the case of the EU to work in the service of more general theoretical analyses of politics and policy. The theoretical synthesis also makes more general contributions to the comparative study of national political institutions as well as that of regulatory politics. The recent revival of work on federalism in comparative politics has remained confined mostly to its fiscal and electoral dynamics. In placing regulatory and legal dynamics front and center, Kelemen's analysis shows the politics of authority to be crucial as well. More narrowly tailored theories, such as those of John Huber and Charles Shipan or George Tsebelis, have already developed specific elements of Kelemen's theory. Yet his account demonstrates the potential of a more eclectic, less formalized institutional analysis to illuminate crucial interactions between multiple institutional elements. The analysis also takes a decisive step beyond descriptive comparison of regulatory styles toward historical institutional explanation of their sources.

The ambition and clarity of the argument, combined with these multiple innovations, help to expose a number of conceptual and methodological limitations. Even as Kelemen casts the account in terms that suggest a more general theory of regulatory development, both the analysis and the research design remain confined to federal systems. More attention to the politics of implementation and multilevel governance in unitary countries would have enabled a more convincing account of what is distinctive about regulatory federalism. In further limiting the comparative case analyses to selected issues in the single domain of environmental regulation, the author also restricts the empirical support for his theory. A brief chapter on other health and social regulation in the EU suggests similar dynamics in other sectors, but further comparative evidence remains necessary to bear this out. Students of the comparative political economy of regulation will also find little attention to alternative explanations beyond the formal institutional logics of Kelemen's own theory. Despite a regard for statutory texts that is all too rare among political scientists, he adduces little specific textual evidence to bolster crucial points about the comparative character of legislation.

The most striking and distinctive contention of the book lies in the author's insistence that regulation in the EU has now become essentially that of a federal state. Kelemen distinguishes this argument not only from the increasingly outmoded approaches of traditional international relations scholarship, but also from "supranational" accounts



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that portray the EU as a unique type of entity with more limited resemblances to nation-states (pp. 6–7). In developing this case, and in bolstering it with side-by-side comparisons to federal nation-states, Kelemen elaborates a point of view that should help recast debates about the nature of European integration. The case he makes is plausible as well as provocative. It also plays on significant ambiguities in the evidence. In choosing to focus his analysis on the environmental sector, for instance, he has selected an arena in which the EU has been unusually assertive. Within the environmental arena, moreover, his analysis stresses the detail of legislation with little attention to its effective scope or substance. If the detail of EU environmental directives often resembles that of domestic legislation and regulation under, say, the U.S. Clean Air Act, EU rules are less likely to establish comprehensive regulatory frameworks of the sort found in domestic rule systems.

The theoretical contributions and innovative comparative design of this study make it an important addition to the literature on comparative public policy and comparative federalism. Although it is unlikely to settle the vigorous ongoing debates about the nature of the EU, it offers a significant new perspective that is must reading for scholars working on the subject.

**Immigration and Politics in the New Europe: Reinventing Borders.** By Gallya Lahav. New York: Cambridge University Press, 2004. 334p. $75.00 cloth, $27.99 paper.

— Alan E. Kessler, *University of Texas at Austin*

In this book, Gallya Lahav offers a nuanced account of the nascent and dynamic Europeanization of migration policy. Moving beyond scholarship that narrowly casts migration as a contest between supranational and domestic constraints, Lahav depicts the prospects for collective European migration policymaking as a product of "emergent consensual attitudes on restrictive policies, among elites and mass publics alike" (p. 9). By "bringing attitudes back in," her treatment offers a welcome complement to intergovernmentalist and neofunctionalist accounts of migration policy and reminds readers that as policymakers grapple with the domestic and international challenges of contemporary immigration, cooperation may be of a negative or restrictive sort.

The book begins with a theoretical chapter, introducing the analytical framework, and then provides a brief sketch of the institutionalization of European migration policy in Chapter 2. The theoretical chapter departs from supranational and intergovernmental accounts of European migration policy by focusing not on the role of "the state" but rather on the determinants of actual state policy mechanisms in managing migration. Lahav contends that attitudinal cleavages and the organization of competing interests, considerations largely absent from state-centered realist and neofunctionalist scholarship, are key to understanding Europe's recent movements toward supranationalism in matters of migration. She explores the diverse mix of national, ideological, and European interests underlying state incentives to cooperate on immigration and views institutional developments as a product of evolving, national-level immigration norms.

Chapters 3 through 5 draw on a wealth of original and secondary attitudinal data sources to depict, in great empirical detail, the cognitive and normative components of immigration policy. Although the link between individual-level attitudinal data and national policy preferences is often murky, Lahav admirably sorts through Eurobarometer surveys to provide a rich attitudinal portrait of public opinion regarding immigration policy over the 1988–98 period. She further draws upon her own elite survey of members of the European Parliament (MEPs) to offer rich insights into parliamentarians' attitudes toward immigration (in 1992). Collectively, the mass and elite surveys contribute to an empirically grounded analysis that puts attitudes at the center of European migration policy.

In Chapter 3, Lahav contends that European publics are more informed about immigration matters than often thought and that MEPs and publics view the issue as both very significant and problematic. She notes that publics and elites favor decreasing immigration and that both groups slightly prefer admission of European Union nationals to migrants from Eastern Europe or "south of the Mediterranean." Clear differences between publics and elites emerge on matters of immigrant integration, with MEPs more supportive of the extension of rights to immigrants than mass publics. Responses to survey questions about the proper level of policy authority (national government, European Community/EU, or both) reveal mass and elite ambivalence, a finding consistent with the tepid progress toward Europeanization of migration and asylum documented in Chapter 2.

Chapter 4 examines national and ideological sources of attitudes as inputs in the process of transnational interest articulation and possible European policy cooperation. Lahav finds that the proportion of non-EU foreigners in EU member states underlies national perceptions of immigration, but she identifies politics, rather than demographic trends, as the critical determinant of national preferences. Indeed, she presents convincing evidence that the venerable left–right ideological continuum predicts MEPs' positions on immigration levels and migrant group preferences. European Parliament members from left parties are significantly more inclined to favor immigration, extend immigrant rights, and delegate migration policy authority to the EU than are their counterparts on the right.

Chapter 5 turns to the "European factor" and examines how European integration affects and, in turn, is affected by immigration attitudes. Lahav contends that the EU's burgeoning development broadens the institutional and psychological basis of support for immigration, as well as



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possible support for the delegation of migration policymaking authority. She finds a link between support for European integration and openness to immigration among MEPs and European publics, and she addresses how the complexities associated with integration generate new political fault lines. In this regard, Lahav closely reviews how postmaterialism, cognitive mobilization, and other notions of "new politics" challenge traditional cleavages, noting that the European factor offers a new framework for debate and alliance formation in migration policymaking, though the relevance of the traditional left–right distinction remains.

*Immigration and Politics in the New Europe* brings a vast range of depth, knowledge, and empirical detail to the question of European migration policy. Lahav's study is a notable effort to weave institutional narrative and quantitative analysis together in order to tackle a theoretically and substantively pressing concern. At the same time, several of the author's findings invite further scrutiny and clarification. Public opinion and elite attitudes are, to start, critical for understanding national migration policies, as the author well details. Yet how precisely individual-level attitudes are translated into national-level policy preferences often remains obscure. Attitudes toward non-EU residents and increasing foreign populations in the early 1990s (amidst adverse national economic conditions) clearly shape national immigration preferences, but how such concerns, in turn, drive national positions toward cooperation on migration at the EU level is not straightforward. Nor is the role of the European Parliament (a more significant actor today than in 1992) in the broader process of the Europeanization of migration policy. The author provides original and insightful analysis of MEPs' attitudes, yet the role of the EP and its importance in transforming debate or shaping national positions toward immigration remains underdeveloped and the subject for additional productive research.

Despite such minor qualms, *Immigration and Politics in the New Europe* is a promising effort to push scholars of migration, international relations, and comparative politics to rethink the linkages among immigration, public opinion, and economic integration. The author's elite interviews and parliamentary survey, as well as careful culling of secondary opinion studies, are testimony to the project's ambition and importance. It is a welcome addition to the burgeoning literature and will surely spark continued research and debate.

**Transatlantic Policymaking in an Age of Austerity: Diversity and Drift.** Edited by Martin A. Levin and Martin Shapiro. Washington, DC: Georgetown University Press, 2004. 344p. $29.95 paper.

— Ronnie L. Korosec, *University of Central Florida*

In an age when we are aware of our increasing global connections with one another, it is only fitting that we should observe the larger similarities that we may share with other nations in the realm of policymaking. In this book, editors Martin A. Levin and Martin Shapiro argue that policymakers on both the conservative and liberal side of the spectrum are so concerned about courting undecided voters who take a "middle of the road" stance that they often compromise bold or innovative policy initiatives in the name of moderation to attract these neutral voters. To complicate matters, most governments are facing austere conditions—which suggests that while demographic and political forces shape policymaking, economic variables play a pivotal role as well. The end result of this complex scenario is that policymakers are more likely to be conformists than trailblazers.

This book highlights reform efforts in many nations that are marked by divergent patterns, fragmented political power, and an incremental mix of "reform without change." The contributors claim that this is true not only in the United States but in Germany, France, Britain, and other parts of the European Union as well. They maintain that bold regulatory and social welfare policy initiatives are compromised because parties and policymakers are weaker, more divided, and more competitive than ever before. As a result, policymaking drifts reactively, rather than taking a proactive or predetermined course of action. To the editors and authors, this is not only frustrating but also suboptimal.

The book includes discussions from nine authors dealing with specific issues in regulatory and social issue areas. Jacob Hacker indicates that health-care policy reform in the United States, Canada, Germany, and the UK has been marked by changes that are not truly reformatory in nature, but rather limited by plummeting public faith, stagnant economies, and anxiety about equality. Kent Weaver suggests that pension plans in the wealthiest countries are determined by "path-dependent" policies resulting in limited options for change. Jonah Levy discusses different strategies for activating labor markets in different societies, including Sweden, Holland, and Britian, and stresses that this activation results in different policies—each with divergent economic and social implications. In the fourth chapter, Virginie Guiraudon looks at immigration reform and argues that this movement has been more effective in Europe than in the United States within the last 10 years, but she notes that immigration reform has been more positive in the United States since September 11. Thomas Burke maintains that a key group of smart, creative activists is responsible for reframing policy issues regarding disability rights. David Vogel suggests that the policy preferences of Americans and Europeans have diverged within the realm of risk regulation, and that this has resulted in policy gridlock in the United States, with the same result expected in the European community. With respect to environmental policy, Daniel Kelemen indicates that fragmented systems in both the United States and the European Union result in a "legalistic" approach



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to environmental policymaking, and Adam Sheingate maintains that politicians in the reform movement overplay their strengths. John Cioffi suggests that U.S.-based private organizations have been less impacted by policy reform movements stressing transparency and disclosure than their counterparts in Europe, and that this has resulted in less comprehensive policy reform in our nation.

*Transatlantic Policymaking in an Age of Austerity* is a must read for anyone interested in domestic or international policymaking. It includes concise yet well-written vignettes of policymaking in action, and challenges the reader to consider thinking outside of the box by comparing policy initiatives in the United States with similar efforts in other nations. The scope of topics is interesting and surprisingly comprehensive—covering a wide range of issues from immigration and environmental policy to public management and pension reform. To this end, the individual authors are to be complemented for their efforts in finding common threads along a wide and frequently divergent field of policymaking initiatives, and for highlighting the global connectivity of policymaking.

There are also several shortcomings associated with this work. First, the authors start with a premise that policy "drift" is necessarily a bad thing. I would point out that the lack of policy action is not *necessarily* a reflection of "uncertainty about what to do"—but could also be a reflection that society, as a whole, has simply not made up its mind on this issue. Why is that necessarily bad? Under such conditions, prudent policymaking may actually be a positive force—as we would not want to make sweeping changes in an environment where there is not full or widespread consensus on action. In this situation, a middle-of-the-road stance, lukewarm or moderate as it may be, might be better than no action at all.

In addition, the sheer volume and scope of issues included in this book can be seen as a negative. While it is large in scope and controversial in nature, one must caution that the discussion in each chapter is fairly limited. Ideally, each chapter could be a book unto itself. Although the central premise is clearly illustrated through the various chapters, one is left feeling that some of authors are overly pessimistic by stating that policymaking could be better, and that moderate decisions are necessarily negative. At times, the reader may conclude that these authors are working too hard to find similarities between systems that are just fundamentally different. To provide more balance on this issue, the editors might have considered including a vigorous argument on behalf of policymaking that is strong, directed, and innovative in nature.

Overall, however, this is a worthwhile, unique, and intriguing book for the practitioner and theorist alike. It brings a comparative perspective to policymaking and suggests how nations may learn from one another in order to create more effective policy outcomes.

**Downsizing the State: Privatization and the Limits of Neoliberal Reform in Mexico.** By Dag MacLeod. University Park: The Pennsylvania State University Press, 2004. 320p. $65.00 cloth, $29.00 paper.

**Democratization Without Representation: The Politics of Small Industry in Mexico.** By Kenneth C. Shadlen. University Park: The Pennsylvania State University Press, 2004. 224p. $65.00 cloth, $27.00 paper.

— Caroline Beer, *University of Vermont*

Mexico's dramatic shift from state-led development to neoliberalism in the 1980s and subsequent transition to democracy in the 1990s has been the subject of extensive scholarly research. These two well-researched and empirically rich books further our understanding of these important processes by providing in-depth analyses of privatization and the decline of small industry. They represent a new generation of scholarship that presents a long-term perspective on Mexico's political economy, beginning with the emergence of state-led development and corporatism in the early twentieth century through to the contemporary era of neoliberalism and multiparty democracy.

The central argument of Kenneth Shadlen's *Democratization Without Representation* is that small industry was more effectively represented under the authoritarian corporatist system, and that democratization has weakened the voice of small industry in government. Thus, Shadlen challenges the common wisdom that democratization improves representation, and in fact argues that democratization can actually diminish representation. He highlights the ways in which representation is particularly difficult for small industry. Because of their lack of resources and relatively weak position in the economy, small firms are especially dependent on formal organizations to represent their interests, but for these same reasons, collective action dilemmas make it difficult for large numbers of small firms to form effective associations. Although many small firms belong to peak business organizations, these groups tend to be controlled by larger companies with greater capacity to manage them. Moreover, small and large industries often have different interests; small firms, for example, tend to oppose foreign direct investment and free trade because they have fewer connections to multinational corporations and they depend more on domestic sales than do larger businesses. This distinction brings small business organizations much closer in line with civil society organizations; however, disagreements over labor and regulatory policies often prevent effective coalitions between these two interests. Small firms are typically marginalized in political parties as well. Unlike big business, they do not have the capital to finance parties. Unlike labor, they cannot deliver large numbers of votes. Given these obstacles to effective representation, Shadlen argues, small industry fared better under Mexico's authoritarian corporatism than under democracy.



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The first empirical chapter explains the structure of business representation in the Mexican corporatist system and describes the formation and strategies of the National Chamber of Manufacturing Industry (CANACINTRA), the main corporatist institution to represent small industry. The organization depended on laws requiring all firms to be members and pay dues to the chamber. It pursued a strategy of accommodation with the state, never publicly criticizing government policies, and hoping in return for favors from contacts inside the state. Leaders were co-opted and often went on to political careers within the ruling party. Shadlen contends that small industry benefited substantially from this relationship during authoritarian rule.

Chapter 3 analyzes the conflicts within CANACINTRA that developed during the early 1980s in the midst of economic crisis and the economic reforms that hurt many small industries. The author argues that democratization facilitated a split in the organization because it allowed the dissidents to pursue new forms of protest and new alliance partners, thus weakening small industry representation. The evidence he presents, however, seems to suggest that the split was the result of the authoritarianism of both CANACINTRA and the state. The chapter describes how the accomodationist leaders turned to the authoritarian state to disenfranchise and harass their challengers. Shadlen suggests that the state's interference was an example of its commitment to small industry, but it seems more likely that the state intervened to keep dissidents from within CANACINTRA from gaining control of the organization. He is somewhat uncritical in his implicit assumption that the authoritarian leaders of CANACINTRA were faithfully representing the interests of its members.

The last two empirical chapters examine the representation of small industry in the later stages of democratization. Shadlen shows how small industry was squeezed between big business, which was trying to reconfigure corporatism to better represent its interests, and National Association of Manufacturing Industrialists (ANIT) (the dissident movement that split from CANACINTRA), which was trying to overturn the law calling for compulsory membership in corporatist business confederations. Small industry found little reprieve in the increasing electoral competition of the late 1990s. The Chambers Law providing for compulsory membership was abolished, and the membership and resources of CANACINTRA plummeted. Small industry did not find reliable representation in civil society organizations or in political parties.

The book represents a thorough and comprehensive study of small industry politics in Mexico, drawing from more than 100 interviews and extensive archival research. Nevertheless, I remain skeptical of the author's central claim that the problems of small industry in the 1980s and 1990s were primarily a consequence of democratization. Rather, it seems that CANACINTRA's declining influence was the result of the state's decision to abandon small industry and embrace big business as part of a new development model. CANACINTRA's weakness comes from its dependence on the state. The state raised the profile of big business and marginalized CANACINTRA because it could; CANACINTRA had no real influence over the policymaking of the authoritarian state. In the postwar period, small industry's interests happened to coincide with those of the developmentalist state. But once the interests of the state changed, it abandoned small industry. Shadlen does a good job of demonstrating that small industry was better off under an authoritarian developmentalist state than under a democratic neoliberal state, and this is an important critique of neoliberalism. But it does not necessarily follow that small industry would have been better off under continued authoritarian rule and neoliberalism. Moreover, if the government had been more democratic during the early 1980s, and CANACINTRA had been more democratic internally, CANACINTRA might have more forcefully articulated the interests of its members, and small industry might have had a greater voice in the reform process.

*Downsizing the State* by Dag MacLeod is an ambitious book about privatization in Mexico. MacLeod's main objective is to reformulate the way scholars think about the relationship between markets and governance. He focuses on the concepts of property rights, hard budget constraints, market failure, and transaction costs, arguing that states create free markets by enforcing property rights and hard budget constraints, but the problems of market failure and transaction costs demonstrate that not all resources are best allocated by the market. Moreover, actors struggle to create markets for some exchanges while simultaneously seeking to avoid market allocation in others. The author argues that paradoxically, during the 1980s the state enforced harder budget constraints on the public sector than on many large private companies. While the public sector as a whole mostly lived within its means from 1965 to 1982, when newly privatized firms went bankrupt in 1995, the government absorbed their debt through the Bank Fund for Savings Protection.

One important strand in MacLeod's argument is a critique of the prevailing wisdom that privatization requires a strong state that is insulated from social pressures. The case studies demonstrate how the state used its control over labor to suppress labor demands and restructure labor relations more favorable to capital. While the state exhibited growing independence from labor, it simultaneously developed greater dependence on capital. The author also shows that state capacity was not necessary for economic reform because the complex tasks of privatization were



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turned over to international consulting firms, thus "privatizing privatization."

The empirical chapters are based on 78 interviews with 58 key informants. They examine the rise of the parastate sector in Mexico from 1930 to 1982 and the privatization since 1982. Chapters 4 through 6 provide in-depth case studies of the privatization of the airlines, telecommunications, and the railroads. MacLeod argues that the nationalizations were driven by a desire to appease capital, control labor, and defend sovereignty. State ownership was often motivated by labor struggles and allowed the state to rein in labor. Parastate firms primarily benefited private firms with subsidized transportation and inputs, while the profits from parastate firms allowed for low taxes on private firms. The state was most proactive in sectors dominated by foreign capital, and so confrontation with domestic capital was rare. The state had trouble controlling the parastate sector. Often the parastate directors were more powerful than the ministers they supposedly answered to, and there was also a tension between the use of parastate firms for patronage and their contribution to national development. Privatization was a response to these problems of control, but also a result of the debt crisis, pressure from international financial institutions, and capital flight. The key components of the privatizations were organizational restructuring, confrontation with labor unions, and foreign investment. MacLeod argues that the telecommunications privatization was relatively successful compared to the others because the state gave Telmex monopoly control over the local phone market and there was huge potential for growth in the telecom industry.

Both books are empirically rich, rely on rigorous field research, and provide important new insights into Mexico's economic reform. Shadlen's book has a crisp and parsimonious theoretical model and clearly demonstrates that democratization hurt the corporatist CANACINTRA, but if CANACINTRA was not effectively representing small industry, it may not matter. He is less convincing in demonstrating that given the context of neoliberal reform, democratization further weakened small industry. Nevertheless, the book draws attention to the importance of small industry as a political actor that must be analyzed apart from larger businesses, political parties, and civil society organizations. MacLeod's book lacks theoretical focus and a clear research question. The empirical chapters are impressive in their depth and detail, but the comparative value of the case studies is not fully exploited. The author could have set up the comparisons more effectively to better address important questions about the causes and consequences of privatization. Nevertheless, the case studies provide many new insights into privatization in Mexico. Both books make an important contribution to the study of Mexican political economy and will be of great interest to scholars of that discipline.

**Trust and Democratic Transition in Post-Communist Europe.** Edited by Ivana Markova. Oxford: Oxford University Press, 2004. 232p. $65.00.

— David Ost, *Hobart & William Smith Colleges*

This is a frustrating volume. That trust is important to democratic transition is its central assumption, but only the final chapter asks whether it is true or why it should be. Instead, most of the chapters discuss how the communist experience affected relations of trust. But some chapters make claims about communist society that generalize solely from the Stalinist period, while others concern the experiences of particular countries to a point where the precise relationship to communism is left unclear. In the end, the only unifying aspect of the book is each chapter's discussion of some aspect of trust in relation to some aspect of the former Soviet bloc.

The first two chapters, written or cowritten by the editor, offer an interesting theoretical discussion of trust and its role in different types of societies. Ivana Markova seeks to develop Georg Simmel's claim that trust is the precondition of social relations, and she argues that in democratic societies trust is related to risk and that in totalitarian societies it is related to fear. The problem, though, is that she claims that "totalitarianism . . . collapsed in 1989" (p. 18), suggesting that fear was all-pervasive until then, as if the passing of Stalinism more than two decades earlier was of no great consequence. But as signified by the emergence of dissident movements whose activists knew what might face them, and by the increased responsiveness of late communist systems to the economic needs of citizens, state–society relations changed markedly in the post-Stalinist period in a way that affected feelings of trust. Even when it comes to Stalinism, claims that the state engaged in a "persistent effort to stimulate mutual distrust among citizens" (p. 9) ignore the importance new ruling parties placed in gaining trust from average citizens. For since they did not legitimate their rule through elections, they could only hope to do so through gaining popularity. The massive class-based affirmative action programs carried out by all new communist parties were an important attempt to build bonds of trust between the poorest, especially rural youth, and the state. A discussion of the relationship of dependence to trust would have been useful.

While no chapter in *Trust and Democratic Transition in Post-Communist Europe* tackles this issue, some do offer a more historicized view of the communist era. Consequently, they put more emphasis on the durability of networks. In an excellent chapter, Geoffrey Hosking, for example, notes how communism's totalitarian aspirations *produced* a multitude of networks in which trust was a central component. Precisely because the state was unable to control everything, it required that people learn to depend on one another. He speaks of various "forms of social solidarity" in Russia and the Soviet Union, and shows



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how that old Russian village commune, the Soviet communal apartment, and the Soviet industrial enterprise itself all required a considerable degree of trust and cooperation. Indeed, it is the demise of these "communities of egalitarian poverty" (p. 60) in the postcommunist era, he argues, that has proven quite damaging. In this account, it is democratization that has sundered trust. In Alena Ledenova's account, "forced trust" is a long-standing concept in Russia: She traces the concept of *krugovaya poruka,* or collective responsibility, from tsarist to post-Soviet times.

Jacek Kochanowicz argues, similarly, that lack of trust toward the state in Poland has been accompanied by high levels of trust in the family and, particularly during the Solidarity period, within the workplace as well. Because of the long history of foreign domination, family had long been a place of retreat, while the same logic of failed totalitarianism as in Russia led to strong horizontal bonds on the job. Strong interpersonal relations substituted for vertical ties of trust, and Kochanowicz argues reasonably that this helped small business growth after 1989, without building political legitimacy.

Wanda Dressler offers a chapter on the effort to build up trust among diverse ethnic groups in Estonia, Moldova, and Kazakhstan. Arguing that "trust lead to cooperation, participation, and reciprocity even if institutions are weak" (p. 129), she suggests that there is greater trust between ethnic groups in the latter two countries than in Estonia because it is reinforced there by clan systems and a more legitimate Soviet heritage.

In the one essay that attempts to measure distrust, based on surveys from the 1990s, William Miller, Tatyana Koshechkina and Ase Grodeland argue that people in postcommunist Europe do not distrust the system as a whole but only specific institutions, and for specific reasons. Challenging an unstated but implicit claim of the book, Miller et al. suggest that high levels of distrust in state institutions may be a problem not of citizens but of institutions. And this leads directly to John Dunn's concluding chapter, which asks why we should even expect that democratization facilitates trust, or why the lack of trust should be seen as dangerous to democracy: "The experience of democracy anywhere is as apt to prove inflammatory as it is consoling—to provoke anger, or even outrage," particularly "for those who lack virtually any market power" and are constantly being told "that they have no rational alternative to endorsing . . . their resulting impotence" (pp. 203–4).

Dunn thus suggests that distrust can be a healthy sign: "What is the point of democracy if not to be free to distrust for oneself and to act on one's distrust?" (p. 204). From this different perspective, appearing unfortunately only in the conclusion, the communist record does not look to be so damaging to civic judgment after all. The distrust of state institutions that use "democratization" as a pretext for policies that impoverish and disempower the weak may be a sign that people fully have their wits about them, and that communist rule was not "bad for the political intelligence" (p. 205). The book's final chapter thus calls into question the volume's assumptions that widespread distrust is a blight on people's democratic capabilities and a bad omen for liberal democratic success. Had alternative hypotheses concerning the nature of the trust/democracy connection been raised in the beginning, the various chapters might have contributed to an important debate. Future studies on trust in changing political systems need to interrogate, rather than accept, current assumptions.

**After the Collapse of Communism: Comparative Lessons of Transitions.** Edited by Michael McFaul and Kathryn Stoner-Weiss. New York: Cambridge University Press, 2004. 272p. $60.00.

— Herbert Kitschelt, *Duke University*

Michael McFaul and Kathryn Stoner-Weiss's edited volume constitutes a useful introduction to important research frontiers in the analysis of postcommunist polities. The editors explicitly set the contributors the task of transcending conventional (post-)Soviet Studies and contributing to a social science analysis of formerly communist polities. Through that lens, "legacies" of the old regimes are important inasmuch as causal mechanisms can be spelled out that show their continuing relevance for the dynamic reconfiguration of politics in the region.

Philip Roeder's essay reflects this theme well. He convincingly demonstrates how the administrative shaping, if not creation, of ethnocultural regions and subpolities under communist rule set the stage for the breakup of the Soviet Union and Yugoslavia in the early 1990s. Not even in the Baltic republics was it pure nationalist fervor that promoted the independence movements, but a complex interaction of popular sentiments with the cumulative crafting of Soviet governance structures and elites that ultimately made even the leaders of the respective union republics embrace National Front movements. This article in exemplary ways demonstrates how long-term, cumulative institutional and social developments during the communist era left their imprint on the final episode of dramatic rupture with the fabric of a multiethnic political regime.

McFaul's causal account of postcommunist political regime diversification, by contrast, emphasizes short-term power alignments at the time of the Soviet Union's collapse in ways pretty much identical to his 2002 *World Politics* article. It may be questioned whether this exclusive preoccupation with short-term mechanisms offers substantial explanatory insights. The author comes around to this question rather late in the essay. His discussion allows for a useful new formulation of the explanatory challenge that can be pursued in future research: How do we explain the instances of postcommunist regime formation that are *not* path dependent and endogenous to institutional



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arrangements and strategic configurations developed cumulatively over previous decades and eras of political regimes? One option is the "punctuated equilibrium" perspective implicitly subscribed to by McFaul, namely, the release of creative capacities in the struggles resulting from the collapse of the old regime. But a different perspective begs for consideration as well. Liberation from path-dependent political learning and bargaining may rather be a matter of Max Weber's proverbial "slow boring of hard boards" through incremental reform and tenacious resistance to remnants of old-system practices. They may bear fruit only with considerable delay, as evidenced by the late tremors and earthquakes that resulted in further regime changes and new political-economic strategies in countries such as Bulgaria, Croatia, Romania, Slovakia, Georgia, Ukraine, or Kyrgyzstan in the 1990s and the new millennium.

The basic message that countries emerge from path dependence only slowly can be taken away from Vladimir Popov's instructive analysis of postcommunist political-economic liberalization. Whereas structural accounts emphasizing preexisting conditions are highly effective for explaining the cross-national variance of initial policies and performance among postcommunist polities, they provide relatively little analytical leverage for illuminating subsequent economic policy moves and payoffs in these very same countries in the second half of the 1990s. At that point, the analytical focus must shift to processes of institution building and to policy choice. Neither legacies nor the alternatives of incremental or "big bang" economic reform at the inception of the reform process explain good or bad economic performance, but the institutional robustness and the substantive economic policy choices made by different configurations of policymakers in the 1990s leave an independent imprint on the economic results.

The remaining four chapters of the book employ Russia as their anchor and reference point for comparative analysis. The advantage is a more precise conceptual and empirical treatment of a limited range of observations that the authors can supply on what is arguably the economically and militarily most important postcommunist country, setting aside China for a number of reasons. The disadvantage is that the contours of other postcommunist polities recede into the background and make descriptive comparison, let alone causal analysis, more hazardous. Stoner-Weiss's article provides intriguing evidence on the centrifugal dynamics of political governance in Russia. But without a comparative analysis of federal–subnational relations in other polities, it remains ultimately unclear whether the glass of central state control in Russia is half full or half empty. Without being able to compare Russia's subnational practices of policymaking to those of other federations, it is difficult for the reader to decide whether her critique of Daniel Treisman's thesis is appropriate: that Russia's national rulers intentionally and in calculated, carefully calibrated ways delegated considerable authority to subnational power structures in order to prevent further secessions and violent conflict (cf. pp. 164–65). We do observe considerable subnational leeway in policymaking, but we do not know whether this is all that different from the practices of other federations.

Timothy Colton provides a detailed analysis of the opportunities and difficulties in bringing measures of party identification to bear on Russian electoral politics. The results, as evidenced by the regression analysis (p. 201), confirm existing research on postcommunist partisanship and electoral choice, but the article could have done more to draw out this convergence of findings. Valerie Bunce's contribution presents Russia as the "median" postcommunist polity on a number of analytical dimensions, and therefore treats the fragility of democracy, state building, and political-economic reform in Russia as indicative of mechanisms also encountered in most other postcommunist successor regimes. By treating Russia as the median case, however, she tends to overstate differences in regime trajectories in other regions of the world. For example, one might object to her characterization of East Central European transitions as "breakage, not bridging" (and pacting) between old and new elites (p. 222). Were not the communist incumbents in Poland, Hungary, Slovenia, and the Baltic states, though not in Czechoslovakia and the German Democratic Republic, eager interlocutors and even participants in the process of regime change, indeed often securing for themselves a political and economic place of respect and leverage in the postcommunist order?

The final essay by James Goldgeier and McFaul offers a detailed critique of neorealist interpretations of the post–Cold War world order, instead advancing the distinction between a core region and a periphery of countries in which core countries no longer have incentives to enforce domestic and regional political order, with the possible exception of peripheral countries geographically situated in the proximity of the core (p. 239). This perspective on international relations feeds back into comparative domestic politics with the proposition that proximity to the core is a predictor of domestic economic and political liberalization in some of those polities whose purely path-dependent trajectory would have set them on a course toward perpetuating authoritarianism and state interventionism in economics. These authors offer the plausible suggestion that this proposition even explains why Russia has adopted domestic and foreign policies that have been rather accommodating to Western interests.

Overall, *After the Collapse of Communism* offers an interesting menu of analytical perspectives on critical issues of postcommunist politics, but variants and components of many of the theoretically most engaging contributions have previously appeared in journals. It is therefore doubtful that the edited volume offers enough value added—whether theoretically or as a textbook synthesis of the research frontier—to attract many buyers.



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**Decentralization and Democracy in Latin America.** Edited by Alfred P. Montero and David J Samuels. Notre Dame: University of Notre Dame Press, 2004. 320p. $47.50 cloth, $27.50 paper.

— Alan Angell, *St Antony's College, Oxford*

The study of decentralization has become something of a fashion, not least because it seems to encapsulate the process of democratization in Latin America and the shift to a neoliberal policy framework, and because it has been adopted by the political Left in sharp contrast to the state-centered policies of previous decades. Decentralization has been seen as the answer to improving the quality of popular participation in Latin America, to improving the quality of public services, and to improving the efficiency in the allocation of public resources.

The book under review carefully avoids overloading the concept and adopts a predominantly political focus in its analysis of decentralization. The concentration is on process rather than outcome—and what outcomes are discussed are those at the level of intergovernmental relations, rather than the results on the ground. This approach has the virtue of concentrating the analytical focus, and the editors have done a splendid job in ensuring that the various contributors not only focus on the same issues and adopt broadly but not entirely the same approach, but even engage in a certain level of interchange—occasionally a mild disagreement—between the chapters. The book also has a strong sense of history—something often lacking among the more committed advocates of decentralization.

The editors introduce the volume in a thorough and illuminating discussion of the various approaches to the study of decentralization. One strong theme is that there is no necessary link between democracy and decentralization, and they stress that it has a long history in Latin America, but that there is no reason to think that the current reforms might not be reversible, as they have in certain cases. They ask the question under what conditions national politicians would surrender power to subnational units. Their answer revolves around the interaction between national and local elites, and is placed in specific institutional and electoral settings. They are not very impressed by the arguments of economic efficiency used to justify decentralization, and are skeptical about the argument that the process owes its origins to prompting by agencies such as the World Bank. They are concerned, above all, with the incentives that face politicians at all levels, the linkages between the levels, and the career paths of politicians—what the editors call the institutional and electoral approaches.

The remaining chapters adopt or modify these main arguments, and there are case studies of Bolivia, Brazil, Chile, Venezuela, and Mexico; Argentina is discussed at length in some of the general chapters. The three general chapters look at the links between political and fiscal decentralization, decentralization and market reform, and intergovernmental fiscal relations. The result is a particularly well-focused volume of essays.

*Decentralization and Democracy in Latin America* leans heavily toward the study of federal systems and includes only two countries with a unitary state (though Venezuela is an odd and unstable hybrid). The choice of Chile is in some ways a little odd as the process there is very tentative at the political level (less so at the level of the delivery of social services). Colombia, which saw a dramatic process of decentralization after the Constitution of 1991 might have made a better choice. The book might have more sharply contrasted decentralization in federal states with unitary ones.

Politicians have incentives to behave in certain ways, but although the authors also consider demographical factors in explaining the trend toward decentralization, there is little stress on the ideological attractiveness of the process at present. Politicians also have ideas, even ideologies, and to explain, for example, why the Workers Party has become not just the main advocate of decentralization but of a certain kind of radical and participatory style of decentralization is a topic worth considering. Indeed, the dominant ideological belief in decentralization shared by International Financial Institutions, local technocrats, the political Left, indigenous groups, church-based groups, and so on does represent a significant shift in political thinking.

But these are mild criticisms set against the undoubted virtues of a book that is a model of how to present a rigorous argument and then to sustain it in the comparative analysis of specific experiences.

**Misplaced Distrust: Policy Networks and the Environment in France, the United States, and Canada.** By Éric Montpetit. Vancouver, BC: UBC Press, 2003. 168p. $75.00 cloth, $24.95 paper.

— Sheldon Kamieniecki, *University of Southern California*

Given the enormous impact that agricultural practices have on the natural environment, it is surprising that so little research has been done on the subject. In addition to problems associated with runoff from feedlots, farming generally requires the use of large amounts of pesticides, herbicides, and chemical fertilizers. The chemicals that are used in farming, if not adequately controlled, pollute the air and water and negatively impact plants, wildlife, animals, and humans. Many environmental scientists believe that agriculture has become too dependent on chemicals to raise animals and grow crops and that it relies too little on effective natural alternatives.

At the same time, agricultural politics, especially when environmental issues are involved, can be extremely conflictual. The agricultural industry in the United States and



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in many other nations employs many people, is quite powerful, and wields a great deal of influence in government. Trying to convince owners of huge agricultural companies to change their practices in order to protect the ecosystem is no easy task. Nevertheless, environmental groups have had some success in persuading government to adopt necessary regulations along these lines. One would think political scientists would be more interested in studying the politics of agriculture and environmental protection.

This book is a welcome addition to the environmental policy literature, primarily because it addresses an important gap in our understanding of the relationship among agricultural practices, environmental protection, and public policy. The author begins with the premise that citizens of industrialized nations overwhelmingly believe that current national governance is inadequate and ineffective, that political leaders are incapable of making the right policy decisions, and that the entire network of state and civil society actors is untrustworthy. He relies primarily on the writings of others rather than on empirical data to support these arguments. He presents several possible explanations why so much distrust in government exists, and he uses agro-environmental policy development in France, the United States, and Canada to examine the validity of this distrust. In other words, is it possible that citizens in these countries significantly misperceive the ability of government to make good public policy, in this case good agro-environmental policy?

The author employs policy network theory to explain the development of agro-environmental policy in the three countries over time. The data on agenda setting and policy formulation in the countries he investigates were obtained through nearly 100 interviews conducted between 1997 and 1998 with government officials representing all the primary organizations involved in agro-environmental policy. Government documents were also used, mainly to update the analysis to spring 2002. Policymaking within each country is discussed in separate chapters. Along the way, a measure of policy performance is conceptualized and operationalized. The author concludes that Canada displays the lowest level of policy performance and that France moved from poor to high policy performance between the 1980s and 1990s. The level of policy success in the United States falls in between those two nations. He concludes that the distrust-inspiring theories introduced in Chapter 3 of the book unjustifiably fuel the present crisis of confidence and misplace the responsibility for policymaking failures, even in Canada. Government leaders are doing a good job addressing environmental problems associated with agriculture.

Although the book provides an in-depth explanation of how policies in France, the United States, and Canada developed over time, it contains three major flaws. First, the author never explains why the handling of agro-environmental issues by public officials is a valid indicator of level of government performance in the three nations. What particular characteristics does this issue area possess that it should be treated as a critical benchmark of policy success of nations in general? Surely, it is easier to understand how critical issues such as national defense, education, health care, and the economy are closely related to trust in government. If this is the case, then it would make more sense to study policymaking across these issue domains, rather than within the agro-environmental issue area. At the very least, the study could have examined additional environmental policy issues, such as clean air and water policy and the handling of hazardous waste, in order to assess policy success more broadly within the environmental policy arena. The issue of trust in government, while important, is an unnecessary distraction in this research and should have been de-emphasized or excluded.

Second, the author pays a great deal of attention to agenda building throughout the investigation. Unfortunately, very little theory addressing agenda building is introduced in the research. The one study that is discussed (John W. Kingdon, *Agendas, Alternatives, and Public Policies,* 1995) is criticized and found to be lacking in its ability to explain policy performance. Much has been written on agenda building, including on agenda blocking, yet none of this literature is reviewed in the book.

Third, readers are told very little about the confidential interviews on which the study—and its conclusions—are based. Who exactly was interviewed for this study and how were respondents chosen (sampled)? What positions did they have? What were their backgrounds, and which political forces did they represent? Exactly what questions were they asked, and how were these questions coded? These issues are not also addressed.

Despite these flaws, *Misplaced Distrust* contains a valuable discussion of how France, the United States, and Canada have developed policies to control the negative impacts of agricultural practices on the ecosystem. Perhaps this investigation will inspire other political scientists to conduct additional comparative research in this important policy area.

**The Quality of Democracy: Theory and Applications.**
Edited by Guillermo O'Donnell, Jorge Vargas Cullell, and Osvaldo M. Iazzetta. Notre Dame: University of Notre Dame Press, 2004. 288p. $55.00 cloth, $25.00 paper.

— Kent Eaton, *Naval Postgraduate School*

The primary goal of this unique and compelling book is to provide the theoretical and empirical foundations for what the authors hope will be a new wave of interest in the quality of democracy. At the heart of the book is a reaction against the minimalist definitions of democracy that in the past two decades have come to dominate not just the regime literature but also the democracy



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promotion work of a whole host of external donors in what Guillermo O'Donnell calls the East and South. While more countries have been able to sustain minimally democratic political regimes than early pessimists feared, the paramount failings of democracy nevertheless have been of growing concern to academics and citizens alike. According to the argument advanced in this book, understanding the performance of these new democracies—and identifying strategies for strengthening democracy—requires that we move beyond regime type "toward other spheres where political power is exercised" (p. 1). In much of Latin America, for example, political regimes experienced democratization while many of the state's authoritarian legacies were maintained more or less intact. In addition to conceptualizing democracy in a richer and more complex way, the book goes on to show how this more comprehensive view of democracy can be measured and evaluated, which it does by describing an actual democracy audit in one important Latin American case: Costa Rica.

The three-part organization is quite unusual for books that are published in political science. The first chapter presents O'Donnell's conceptual and theoretical work on democracy, exploring the strong elective affinity that he sees among the fields of human development, human rights, and democracy, each of which understands "the human being as an agent carrier of rights that define, and support, his/her very distinctiveness as a human being" (p. 62). In the second chapter, one of O'Donnell's former students, Jorge Vargas, explains the mechanics of a pathbreaking citizens' audit that he and others directed in Costa Rica, which resulted in part from ideas discussed in O'Donnell's seminar at the University of Notre Dame. The third section of the book is a collection of short and engaging essays written by leading Latin Americanists in response to various points raised in the O'Donnell and Vargas chapters.

O'Donnell is deservedly famous for a number of reasons, including work with Philippe Schmitter and Laurence Whitehead 20 years ago that questioned the importance of structural variables in the regime transitions then taking place from authoritarian rule. In *The Quality of Democracy,* O'Donnell argues that once we move beyond the political regime to look at democracy in other spheres, material conditions do become quite important, particularly when gauging the quality of any given democracy. "Being subject to physical violence, the recurrent fear of violence, hunger or malnutrition, serious preventable diseases, or severe inherited incapacities" (p. 60) all compromise the ability of individuals to exercise their political rights as citizens in a democracy. It is here that O'Donnell is able to borrow so fruitfully from a recent convergence in the work of human rights and human development experts, according to which the former are now emphasizing social and not just legal factors, and the latter are paying greater attention to human rights. Unlike the universalistic claims of most advocates of human rights and human development, however, he argues that the minimum sufficient set of rights or capabilities necessary to secure agency is "theoretically undecidable" (p. 178). Instead, this can only be decided by democracy itself.

In a less theoretical and more comparative mode of analysis, O'Donnell also identifies distinct sequences in the real-world acquisition of political and civil rights. Whereas civil rights were reasonably secure before the extension of political rights in the countries of the Northwest, in Latin America the sequence has been reversed. Well after the extension of formal political rights, the generalization of civil rights has yet to occur—consider, for example, O'Donnell's earlier work on authoritarian "brown" areas where the writ of the central state simply does not reach. Despite a less desirable historical sequence, however, he holds out the hope that political rights can be used to achieve other rights in the East and South, a strategy that feminists and minorities in the Northwest have used to great effect, and one that is simply unavailable in authoritarian political regimes. In a trenchant response in the third part of the book, Terry Karl offers a less optimistic view about the likelihood that political rights in new democracies will be sufficient to conquer other rights. According to Karl, the generation of a virtuous cycle between democracy, development, and the rule of law requires "systems of representation that actively encourage the political organization of the poor" (p. 192), particularly in those cases where democracy came about through elite pacts.

Although O'Donnell's theoretical contribution is what will perhaps bring most readers to this book, Vargas's description of the citizen audit is no less important and inspiring. Rather than applying a static set of criteria designed by experts, the audit used citizens' democratic aspirations as the departure point against which to measure actual democratic performance. Thus, the audit's designers convened a civic forum of 42 political, academic, and social leaders who, through a process of consensus, together devised the 33 "quality of democracy" measures that guided the audit. As Catherine Conaghan, Michael Coppedge, and Gerardo Munck argue in their contributions to the book, this inductive design raises important questions about how the audit could be replicated in other national and subnational units. These criticisms are well taken, and yet a more deductive approach emphasizing the problems that generally face Latin American democracies may well have failed to capture the particular experience of Costa Rica, widely seen as one of the region's most successful democracies, but one that nevertheless has its own problems (including abusive treatment of citizens by the state and obstacles to trade union organizing). Furthermore, the audit's broadly



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consultative and inductive approach—more than 4,800 individuals participated—certainly helps explain how the audit was able to function not only as a generator of new information about democratic performance but ultimately as a new mechanism of social accountability in Costa Rica.

**Paths to Democracy: Revolution and Totalitarianism.** By Rosemary H. T. O'Kane. New York: Routledge, 2004. 288p. $110.00 cloth, $33.95 paper.

— Graeme Gill, *The University of Sydney*

In this book, Rosemary O'Kane seeks to restore a political dimension to the comparative historical analysis of paths to democracy. Reacting against the focus upon social structure that has been so prominent in the analysis of democratic and nondemocratic regime trajectories, she attempts to provide an explanation of the paths to the contemporary situations of three countries, France, Germany, and Russia. She argues that these cases contrast with one another in ways that help to illuminate a range of hypotheses generally accepted about the relationship between democracy, on the one hand, and such variables as economic crisis, economic growth, industrialization, and urbanization, on the other. In broad terms, she sees the cases contrasting in the following ways: Russia contrasts with France and Germany in respect to democracy, Germany with France and Russia in respect to revolution, and France with Germany and the Soviet Union in respect to totalitarianism.

The book is introduced by an excellent discussion of the methodology of comparison. This is followed by a review of the meaning of democracy as discussed in contemporary empirical political theory, and this is then used to identify a range of hypotheses that "should not be applied to history in a rigorous way but be used simply to inform analysis, to offer guiding hypotheses" (p. 40). The following chapter discusses the part played by revolution in generating ideas that were then influential in shaping subsequent change. The remainder of the book comprises detailed studies of the development of the political systems in the three case study countries. This is a stimulating book, but it also has limitations.

One issue with the book concerns the nature of the so-called path to democracy. It is not clear what this means, apart from simply a description of the historical trajectory of particular countries. The discussion of the case studies occurs essentially in narrative form; it is largely in the form of descriptions of what happened, especially in the field of constitutional development. There is no dynamism in the accounts, nor is there any real explanation of why certain sorts of developments occurred. As a result, the notion of path possesses no explanatory significance at all. It may be the course of development the particular country has followed, but there is no sense that the path has any boundaries that were instrumental in keeping the country to a particular course. This is particularly clearly illustrated by the German case, where postwar German democracy was, in substantial part, a function of imposition by the victorious wartime powers. O'Kane acknowledges this, but without recognizing that it makes the notion of path problematic in anything but a passive, descriptive sense.

So if the notion of path has no in-built explanatory power, the question is, what can we take from this book? Essentially O'Kane seems to want to investigate what light the French, German, and Russian experiences throw on a number of hypotheses about democratization and what facilitates it. However, as she acknowledges, this number of case studies is insufficient either for grand theorization or for making substantive judgments about any direct relationship between the variables she notes and democratization more generally. This means that what we are left with is the accounts of individual national experiences against which we can compare the variables to see whether they are consistent. However, there are two problems with this approach. First, the variables are expressed in very general terms (eg., "the role of governments in waiting . . . the capacity of politicians to take personal responsibility . . . entrepreneurship," p. 41) with little investigation of what these might mean in a practical sense. Second, when the variables are combined with the descriptive tenor of the case studies, their importance and the role they played is made unclear. Even if it can be shown that a democratic outcome was associated with some of these variables, the causal relationship (if any) is not demonstrated.

The lack of an explanatory dynamic is not the only problem in the discussion of the national histories. The focus on constitutional and institutional aspects of development may be appropriate, given the emphasis of the book, but it is also very narrow. Furthermore, in some instances it is incomplete. For example, in the discussion of Nazi Germany and the Soviet Union under Stalin, no consideration is given to the position occupied by the dictator in each case, and yet this was crucial for the shaping of the regime. In both cases, heavy emphasis is given to the role of the security apparatus and forced labor, and yet in both countries, other institutional structures, such as the leader and the party, also played crucial governmental roles. In addition, there are whole historical debates bearing upon the issues discussed in this book that are not mentioned. For example, the debate over the *sonderweg* thesis has clear implications for an understanding of the German experience, while in Russia, the question of the relationship between the Lenin regime and that of Stalin is crucial to the interpretation O'Kane places on events. Furthermore, at times she gets some of the historical details wrong; for example, Vesenkha was not established in 1928 but in December 1917, and the Congress of People's Deputies was not strictly speaking bicameral but an assembly



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that met twice per annum and elected a standing body to fill in for it in between meetings.

Finally, the book really gives a conjunctural interpretation of the path to democracy, with the decisions of political actors playing an important part. Accordingly, it is curious that there is no mention of the democratization literature that has emerged over the past two decades, much of which has concentrated precisely on the role of elites in bringing about democratic change. Given the book's focus, this would seem to have been an obvious literature with which to engage.

So in sum, *Paths to Democracy* is an interesting book. Its discussion of the methodology of comparison is excellent, and the individual case studies are useful, but there is little by way of generalization that can enhance our understanding of democratization as a process.

**Studies in Public Opinion: Attitudes, Nonattitudes, Measurement Error, and Change.** Edited by Willem E. Saris and Paul M. Sniderman. Princeton: Princeton University Press, 2004. 384p. $65.00 cloth, $29.95 paper.

— Paul Whiteley, *University of Essex*

This edited volume contains a series of papers, which address a key issue in public opinion research. The issue could not be more general or more important. It is this question: Do members of the mass public actually have opinions about political issues? The question has been debated from the earliest work on public opinion, but was raised in a particularly stark form in a seminal paper by Philip Converse published in 1964. As is well known, he suggested that most people did not have meaningful opinions on most issues, due to the instability of their responses across different waves of a panel survey. The contributors to this volume are leading authorities in the field of political psychology and public opinion research, and the papers collectively make an important contribution to addressing this question. Paul Sniderman and John Bullock in the concluding chapter summarize the existing research agenda on this issue in the following terms: "The dominant themes of two generations of research have been that citizens tend to be muddle-headed (the lack of constraint theme), empty headed (the non-attitude theme) or both" (pp. 337–38).

There have been two broad lines of attack in examining this question, and both are well represented in the book. One is methodological, and it suggests that if survey instruments and methods of analysis are improved, then a lot of the "empty headedness" would disappear. However, the article by William Van der Veld and Willem Saris uses a linear structural relations modeling approach to control for measurement error in panel data, and concludes that response instability can still be quite high, even when measurement error is controlled. This challenges earlier methodological work, which suggested that response instability was largely a function of such measurement error. Similarly, a paper by Peter Neijens sets out the Choice Questionnaire methodology, which has been developed largely by Europeans with the aim of improving the quality of survey instruments. In this approach, respondents are given written information before their opinions are elicited in order to inject deliberation into the process of opinion formation.

The second line of attack is to try to provide a theoretical account of nonattitudes and response instability. The Receive-Accept-Sample (RAS) model associated with the work of John Zaller is one influential theoretical approach, essentially arguing that response instability is a function of ambivalence. The suggestion is that in responding to survey questions, individuals haphazardly sample the multiple attitudes or "considerations" they might have in their minds and construct opinions "on the hoof." Not surprisingly, this can produce response instability over time. In their paper, Milton Lodge and his associates also find that ambivalence is a key issue in influencing responses to survey questions because of the multidimensional nature of attitudes.

A third issue discussed is that of framing effects. As is well known, questions framed in different ways will produce attitude instability in respondents. This occurs not because they have no attitudes but because the information that people have is being processed differently in different contexts. Thus, framing effects are a logical consequence of multiple attitudes. Sniderman and Sean Theriault show that while these effects are large, they do not necessarily lead to biases in public opinion because the public is exposed to rival frames by different parties, which means that framing becomes part of the political contest.

*Studies in Public Opinion* addresses the agenda it sets itself very well, but it is not clear if this agenda is wide enough to answer the original question. At least three key issues are missing from the analysis. The first is the thorny problem, initially identified by Anthony Downs, that because it is rational for individuals to ignore things they cannot change, it is rational for them to be ignorant and have no stable opinions. In classical decision theory, actors acquire information for purposive reasons—they want to use information to acquire personal benefits. But in a representative democracy, only mass or aggregate opinions matter, and individual opinions are irrelevant for bringing about political change. Given this, why does anyone bother to spend his or her time thinking about political issues? This point provides a theoretical grounding for the original Converse critique. This volume needed to address the incentives for acquiring political information. Essentially, we need a theory of opinion incentives.

A second important point is that we know that people tell lies to survey researchers. In the British Election Study, we can validate if the respondents to our surveys actually voted or not, despite what they tell us in response to questions. We find consistently that upwards of about 5% of



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respondents claim that they voted, when in fact they did not. The methodological debates have focused almost exclusively on issues of reliability and have tended to ignore this important topic of validity. This is not surprising because the former is easier to get a handle on than the latter. There needs to be a research agenda that investigates the circumstances under which people will tell the truth as they see it, when they are responding to survey questions.

The third issue has received a lot of attention in the social psychological literature but has not been satisfactorily resolved. It concerns the relationship between attitudes and behavior. Under what circumstances will individuals behave in ways consistent with the opinions they express to survey researchers? Thus, for example, does the RAS model apply to voting? Notwithstanding the work of Martin Fishbein and others, we do not have a satisfactory theory of the relationship between attitudes and behavior, and so this is an important question that is neglected here.

Having made these points, I do think that this volume is a "must buy" for students of public opinion, since it makes a valuable contribution to an important topic.

## INTERNATIONAL RELATIONS

**Military Power: Explaining Victory and Defeat in Modern Battle.** By Stephen Biddle. Princeton: Princeton University Press, 2004. 312p. $37.50.

— Richard L. Kugler, *National Defense University*

Stephen Biddle has written a worthy book on the never-ending debate over why land wars are won and lost. It contributes to the academic literature, and his policy judgments deserve attention. He puts forth the theory that "force employment" (i.e., doctrine for using military forces in battle) plays a major role in determining outcomes, as big a role as the size of contending forces and the sophistication of their technologies. He further argues that modern employment doctrine, which enables skilled ground forces to negate the effects of lethal fires against them, was born in late World War I, when attackers finally overcame entrenched defenses. A skilled operations research analyst of tactical battles, Biddle offers a multimethod approach that combines logical reasoning, case studies, mathematical models, statistical analysis, and dynamic simulations. His book can be read by nonspecialists, but many of its technical discussions (e.g., Lanchester equations) require expert knowledge.

Biddle's main thesis is on target. As he points out, history and analysis support the judgment that force employment matters importantly in shaping victory and defeat in land warfare. Biddle also offers useful advice to U.S. defense planners when he urges them not to become so enamored with high-technology and air-delivered fires that they ignore the continuing importance of fielding strong ground forces with the capacity to perform effectively. For these reasons, his book likely will be well regarded by many readers.

Virtually any book on this controversial subject is likely to trigger rejoinders. Critics may say that Biddle sets up a straw man in order to cast his theory in favorable light. He argues that the main opponent of his theory is traditional "preponderance theory," which holds that force size and technology determine battlefield outcomes. Critics may point out that few preponderance theorists would dismiss employment strategy as unimportant. Likewise, critics may argue that the idea of mastering employment doctrine has been alive since Caesar. Frederick the Great and Napoleon used it to gain many victories against strong opponents. The German Wehrmacht nearly won World War II because of its battlefield skills. Historians normally devote considerable attention to battlefield strategies, often at the expense of sufficient attention to numbers and technologies. Critics may ask why Biddle claims his theory is new.

Critics may also accuse the author of lacking strategic perspective in citing two battles from World Wars I and II to exemplify his theory. His use of Operation Michael, a German attack against British and French trenches in spring 1917, seems odd. The attack gained only 20 miles at the weighty cost of 250,000 German casualties in nine days. Indeed, the entire German offensive of that spring, which featured several attacks, failed to win the war even though it gained some ground by using new doctrine. A better example may have been the subsequent allied counterattack in summer/fall 1918, which drove the Germans backward 150 miles and compelled them to surrender. Regardless of the battles examined, Germany was defeated in World War I because its exhausted army ran out of staying power in the face of preponderant allied forces, which benefited from a huge American buildup.

Likewise, Biddle's use of a failed British attack in World War II seems odd. Yes, Operation Goodwood of summer 1944, failed to break out of the Normandy beachhead because the numerically superior British made tactical errors and the outnumbered Germans showed tactical skill. But a few days later, the U.S. Army used preponderance and sound tactics to overwhelm German defenses at St. Lo, and afterward nearly encircled the entire German 7th Army. Biddle's theory would have been better served by analyzing the successful German blitzkrieg campaign against the French and British in spring 1940, which was largely won by better doctrine, not superior numbers. It was this battle, not the end of World War I, that created modern



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doctrine because it featured a breakthrough attack that quickly unraveled the defender's cohesion, thus making a costly attrition fight unnecessary. But had Biddle examined this battle, he also would have been compelled to analyze the final year of World War II, in which preponderant U.S. and British forces crushed the Wehrmacht, notwithstanding its tactical excellence. Critics may assert that the final results of both World War I and II validate preponderance theory, not Biddle's theory.

Another oddity is that Biddle largely ignores the lengthy debate over the NATO–Warsaw Pact military balance in Central Europe during the Cold War. It was this debate that gave rise to the planning tools, aimed at measuring preponderance, that the author strongly criticizes. The purpose of these tools was not to ignore employment strategy, but instead to measure whether the Warsaw Pact forces were so superior to NATO forces that conventional defense was impossible. They ultimately concluded that the force balance was sufficiently close to enable NATO to contemplate a forward defense. They opened the door not only to new NATO weapons but also to new employment strategies anchored in the reserve forces and fluid maneuvers favored by Biddle. Had he discussed this debate, he might have done a better job of putting the utility of these tools into proper perspective.

Biddle correctly observes that superior employment strategy figured heavily in the U.S. and allied battlefield victories over Iraq in 1991 and 2003. However, he seems wrong in asserting that current U.S. defense plans remain wedded to preponderance theory through technological supremacy. In fact, these plans show major attention to mastering modern employment doctrine. Biddle is correct in arguing that new analytical tools are needed to understand modern combat operations. Likewise, he properly urges that U.S. defense plans remain anchored in strong ground forces even as new networks, sensors, and munitions elevate the lethality of air forces. But his appraisal of the U.S. defense budget seems off-target. He says that the Department of Defense is underfunding readiness in order to procure new weapons. In fact, the opposite is true. Whereas the Pentagon's readiness budget is well funded, its procurement budget is only $75 billion: low by historical standards. Defense experts likely will dispute Biddle's call for less procurement spending, which could sacrifice valuable new weapons for all services. Likewise, experts may question his endorsement of the army's new lightweight weapons, which rely heavily upon air support and seemingly could undercut the sophisticated employment strategies favored by Biddle.

In summary, *Military Power* may be subjected to rebuttals of its specific features, but it accurately captures important lessons. Past wars have been won or lost because of preponderance, or doctrinal superiority, or a combination of them. Each war must be judged on its unique merits. Future war plans should reflect a proper combination of size, technology, and skill. Beyond question, this book represents a serious effort to apply sophisticated analysis to an important, complex defense issue. It presents many good arguments for experts to debate, and it allows inexperienced readers to learn a great deal about a vital issue that might otherwise seem arcane. It will make a good textbook for undergraduates and graduate students. It is well worth reading, owning, and remembering.

**No End in Sight: The Continuing Menace of Nuclear Proliferation.** By Nathan E. Busch. Lexington: The University Press of Kentucky, 2004. 512p. $40.00.

— Robert E. Harkavy, *The Pennsylvania State University*

This new work on the core issue of nuclear proliferation is somewhat unique in its focus and organization. Most of the works on this subject have a virtually standard organization involving a country-by-country breakdown of the most recent and most likely proliferators (India, Pakistan, Israel, Iraq, Iran, North Korea, etc.), according to their motives (why they want to go nuclear) and their capacity to do so, either by themselves or with the help of existing nuclear powers. The focus of these works is, therefore, on what often is called "horizontal proliferation," having to do with the addition of extra members to the "nuclear club." Regarding the Cold War superpowers, the United States and former USSR, there has been a largely separate literature devoted to "vertical proliferation," and associated arms control measures and possibilities, in the context of nuclear deterrence theories and concepts. The uniqueness of Nathan Busch's book is that he combines these two genres, treating various aspects of the nuclear programs of the long-existent large nuclear powers, as well as the emerging nuclear programs of Iran, Iraq, North Korea, and so on as an integrated subject.

Busch begins his detailed and lengthy analysis with a review of the contrasting views of what he calls the "optimists" and "pessimists," those who think nuclear proliferation is on balance a good thing, an instrument of peace and stability, and those who dread the further spread of nuclear weapons, a debate earlier formalized in a book that constituted a debate between Kenneth Waltz and Scott Sagan. Busch points out that several scholars other than Waltz, that is, Bruce Bueno de Mesquita, William Riker, Martin van Creveld, and John Mearsheimer, have made the "optimist" arguments. But he may exaggerate the weight of these arguments, as almost all other writers in the field are very much on the "pessimist" side of the debate; that is, the debate is highly asymmetric.

The guts of *No End in Sight* consists of a very detailed country-by-country review of the nuclear programs of the United States, Russia, China, India, and Pakistan, and under the rubric of "newly proliferating states," the "axis of evil" triangle of Iraq, Iran, and North Korea. Britain, France, and Israel are largely ignored, of which more below.



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For each of the countries covered, there is a full review of their existing nuclear arsenals, their nuclear production facilities, the mix and effectiveness of delivery systems, the role of extant arms control arrangements, command and control, and (a particular focus of this work) what the author refers to as MPC&A (fissile Material Protection, Control, and Accounting). Related to the latter, there is also a focus throughout the cases on accidental use (subsuming accidental detonation and accidental launch), unauthorized use, and inadvertent use, categories that elsewhere are not adequately separated out.

In the case of the United States, there is extensive discussion of command, control, communications, and intelligence, the implications of the several extant arms control arrangements (the Intermediate-Range Nuclear Forces Treaty, the Strategic Arms Reductions Treaties, etc.), the current U.S. nuclear order of battle, nuclear weapons storage and deployment, and under the heading of C3I, launch on warning (LOW), early warning systems, command and control systems, risks of accidental use and detonation, organizational and security issues, and so on. MPC&A is a constant focus, seeming to represent the author's primary research interest feeding into the book. A similar spread of subjects is covered for Russia. This book is a good place to go to get a look at how the two Cold War superpowers have reduced and reorganized their nuclear arsenals since 1990 in relation to their mutual arms control arrangements. Also very useful is the material on Russia's ballistic missile early warning radars, which shows how the Russians are still dependent on access to radars in nearby Ukraine, Belarus, and Azerbaijan, earlier also in Latvia. The data presentation on Russia's nuclear arsenals, its early warning radars and satellites, and its command and control system is interesting, excellent, and, for the academic literature, perhaps unique. For China, there is an equally thorough review of its nuclear facilities, delivery systems, and early warning systems. For each of the three leading nuclear powers, there is also discussion of past incidents involving nuclear accidents in the context of MPC&A.

The chapter on India and Pakistan is particularly useful, an area dubbed by the author "the most dangerous place on earth." He discusses the history of the several points in the last 20 years when these two countries came close to the brink of nuclear war: the Indian Operation Brasstacks in 1986–87, the 1990 Kashmir crisis, the Kargil crisis in 1999, and the renewed tensions after September 11, 2001, when the United States had to defuse a crisis while obtaining Pakistani support for its operations in Afghanistan. The author characterizes the nuclear relationship between India and Pakistan as one of "unstable deterrence," by comparison with the earlier U.S.-Soviet nuclear relationship. There is an excellent rundown of the structures of the two nations' nuclear facilities and delivery systems, somewhat reliant on the recent publications of the Carnegie Endowment. Finally, there is a comprehensive coverage of the nuclear programs of Iran and North Korea, both of which are shown to be moving toward the possession of nuclear weapons via both the U-235 (gas centrifuges) and Pu-239 (nuclear reactors plus plutonium reprocessing facilities) routes. For Iran, for instance, now perhaps the primary focus of U.S. proliferation concerns, the ominous connection between the Natanz centrifuge operation and the long-range missiles acquired from North Korea, which can reach to Israel, is highlighted. There is a concluding chapter with policy recommendations that, like the remainder of the book, is very much focused on issues of command and control, accidental use, and safeguard controls over nuclear materials in the existing nuclear powers.

In what is a generally fine and very useful book, a few areas might have deserved greater attention. Despite the lack of good information, the Israeli nuclear program might have been given greater coverage, if only because the nuclear programs of Iran and Pakistan are referenced to it as "Islamic bomb" programs constituting a counter-deterrent to that of Israel. And although these matters have only recently come to public prominence, much more attention might have been paid to the chain effects and international interactions involving the French, Israeli, Iraqi, Pakistani, Libyan, Iranian, and North Korean nuclear programs. The recent flood of press reports has detailed the skein that has run from the A. Q. Khan operation in stealing centrifuge technology from the Urenco plant in the Netherlands, and, in turn, the transfer of centrifuge technology to North Korea, Iran, and Libya. The French role in both the Israeli and Iraqi nuclear programs needs highlighting. In short, this book might have used a little more focus on deliberate country-to-country nuclear technology transfers in order to supplement the focus on internal controls of nuclear materials, deemed sincere if sometimes flawed by the authors and others. That said, this is a rich and interesting review of the nuclear spread menace as it is now currently unfolding.

**National Governance and the Global Climate Change Regime.** By Dana R. Fisher. Latham, MD: Rowman & Littlefield, 2004. 206p. $69.00 cloth, $27.95 paper.

— Robert F. Durant, *American University*

Debate continues among scholars in a variety of disciplines over the impact (positive, negative, or neutral) of transboundary environmental problems on the sovereignty of nation-states, especially in the developing world (see, for example, Karen Litfin, ed., *The Greening of Sovereignty in World Politics,* 1998). Expanding this debate to "economically advanced or postindustrial nations," sociologist Dana Fisher addresses three important questions in her book. First, what is the role of the nation-state relative to other actors (civil society, market,



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and scientific) in negotiating international agreements when transnational environmental issues are involved? Second, how best can the disparate reactions of governments to the development of these international environmental pressures be explained? Third, how does the interaction of international and national factors affect the substance of international agreements?

Using the policy debate since 1997 over the Kyoto Protocol as a "policy window," Fisher addresses these questions with a research design combining quantitative analysis and qualitative case study research in Japan, the Netherlands, and the United States. She begins the book by offering an analytical framework for studying international environmental policymaking that challenges two competing sets of theoretical perspectives in the sociology literature: "theories of the environmental state" and "theories of environmental sociology." The former offers an optimistic (she says European) perspective (grounded in postmaterialist, ecological modernization, and reflexive modernization theories) that portrays economic growth as advancing environmental quality. In contrast, the environmental sociology perspective is a decidedly more pessimistic (she says American) perspective: As economic growth occurs, the use of natural resources expands and environmental quality suffers apace.

Applying regression analysis to data from 29 (of 30) Organization for Economic Cooperation and Development nations to test the assumptions of these theories, Fisher begins by finding little support for the idea that success in reducing greenhouse gas emissions (GHGEs) is a function of top-down state-driven, bottom-up civic-driven, or economic prosperity–driven factors. Next, she offers and tests, with her three comparative case studies, a more robust analytical framework that she calls the "governmental environmental system" (GES). Variations in national responses to the Kyoto Protocol, she argues, are a function of reciprocal influences between domestic forces (specifically, the political interaction of the state, the market, the scientific community, and civil society) and international pressures (e.g., pressures by the European Union on other nations to adopt higher GHGE reduction targets). She concludes from her archival research and semistructured interviews with 80 key policy actors in Kyoto negotiations in Japan, the Netherlands, and the United States that the GES framework usefully accounts for differences in their approaches toward Kyoto, and that domestic debates among state, market, civil society, and scientific actors are significantly more important than is commonly recognized.

*National Governance and the Global Climate Change Regime* has several strengths. For example, the book offers a clear and concise discussion of the science that informs global change policy, concluding that the question is not whether humankind's activities are contributing to global warming, but how much, how quickly, and where. It also offers an excellent overview of the regulatory issues (e.g., carbon sinks and joint implementation), regimes created, and negotiating processes that animated international debates over the Kyoto Protocol (although the book ends before Russia signed the treaty putting it into effect). Moreover, by extending analysis of the environmental protection–national sovereignty nexus to economically advanced nations like Japan, the Netherlands, and the United States, the book both improves our understanding of the politics related to Kyoto in these three nations and affords a basis for comparing these dynamics with those in the developing world. The book also offers additional evidence challenging the "erosion of sovereignty" thesis, corroborating the reciprocal nature of national–international influence, and buttressing impressions that the power of domestic politics remains strong (at least in advanced or postindustrial societies). In the process, Fisher's case studies suggest the utility of thinking "beyond" the state to view its interactions with market, civic, and scientific sectors. Finally, and relatedly, the book further occasions skepticism about the role (positive or negative) of economic determinism in predicting environmental policy choices and impacts.

These contributions notwithstanding, the book also disappoints in several ways. Substantively, while the sociological perspectives offered may marginalize the role of domestic politics in international environmental decision making, environmental policy scholars will hardly be surprised at either their import or the specified interrelationships among actors in state, scientific, market, and civil society. Indeed, many of these scholars will infer a straw man argument in the book's claims that the role of domestic actors has not been widely recognized. In addition, a more precise and systematic comparison of the causal mechanisms across nations among these domestic forces, as well as between domestic and international pressures, would have better advanced theory building in this policy arena. Moreover, the book's failure to relate its findings to this broader comparative policy literature, as well as to ongoing debates over the role of the state in transboundary environmental issues, means that a major opportunity to address this important question is missed.

Methodologically, the book disappoints in its specification and operationalization of the quantitative model used to test the environmental sociology and environmental state perspectives. For example, the measures of ecological efficiency (e.g., motor vehicle travel per capita), environmental institutionalization (e.g., percentage of national park and protected areas to total land area), and environmental impact (e.g., the Sustainability Index of the Global Leaders of Tomorrow Environment Task Force and Ecological Footprint of Nations measure) are largely cross-sectional and are sometimes controversial (namely, the Sustainability Index). Moreover, the measures of top-down state action that are used to operationalize the state environmentalism thesis are largely indirect measures of state commitment that might have been affected by other



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factors (e.g., municipal waste in kilograms per person and energy supply per capita can be affected, as in the Netherlands, by economic and energy infrastructure factors). The study would have benefited from more direct operationalization of such variables as administrative regulatory capacity created or commitment to environmental regulation (e.g., size of environmental budgets per capita or as a percentage of gross domestic product).

Finally, some interpretive issues also present problems. For example, the ordinary least squares regression model predicts that a positive relationship will exist between a nation's prior efforts to deal with environmental problems (e.g., in reducing industrial waste) and its commitment to reduce carbon dioxide. As the book's discussion of the complex science of global climate change reveals, however, global climate change is what Edward Carmines and James Stimson ("The Two Faces of Issue Voting," *APSR* 74 [March 1980]: 78–91) call a "hard" policy issue that puzzles the public. Moreover, as the book's discussion of Japan's approach to Kyoto indicates, the immediacy of risk that has prompted citizens to seek regulatory protection from potential harms (e.g., the health risks of various toxic pollutants) is absent when global climate change is involved. In turn, the free-rider problem that is always acute in the international environmental policy arena increases when, as with global climate change, the timing, location, and magnitude of risks are problematic. Also, while the case analyses show how the interaction of domestic actors affected their approaches to Kyoto, the link to international pressures is less clearly documented. To be sure, EU pressure pushed both the Japanese and the Clinton administration into major concessions, but they did not have a similar impact on the Bush administration. Accounting for these dynamics would have improved the book substantially.

**Deterrence.** By Lawrence Freedman. Cambridge, MA: Polity Press, 2004. 160p. $59.95 cloth, $24.95 paper.

— Michael C. Desch, *Texas A&M University*

The concept of deterrence—the effort to make an adversary's costs and risks of going to war greater than the political incentives pushing in that direction—was the centerpiece of academic national security studies and the core policy concern of Western governments throughout much of the Cold War. Today, in the post-9/11 world, deterrence has taken a back seat to a different set of strategic concepts: preemption and prevention. Both involve destroying an enemy's capabilities before he can harm you, the former when the threat is imminent, the latter when it is potential (pp. 85–89). In his timely and useful book, Lawrence Freedman offers a compelling account of the decline of deterrence and creatively seeks to revitalize it as a subject of scholarly inquiry and as a viable policy in the post-9/11 world.

The precipitous decline of deterrence is something of a puzzle inasmuch as its track record from 1945 through 1991 was pretty good. The aim of the members of the North Atlantic Treaty Organization during the long struggle with the Warsaw Pact was simply to pose enough of a threat—using a combination of conventional and nuclear forces—to dissuade the Soviet Union and its allies from launching World War III. Despite fears on the Left that this posture might provoke accidental war and complaints from the Right that conventional deterrence by denial or nuclear deterrence by punishment was not sufficient to keep the Red hoards at bay, NATO's deterrent posture seems in retrospect to have been fully adequate.

Given this, it is surprising that deterrence would have emerged from the Cold War in such ill repute. Freedman's book helps us understand this puzzle. In addition to the persistent reservations about deterrence among many Cold War scholars and policymakers, Freedman highlights important post–Cold War developments that have further undermined deterrence. Essentially, the Bush administration and many analysts outside of government have concluded that new threats from nonstate actors or "rogue states" cannot be dealt with in the old deterrence framework, the former because they have "no return address" against which we can retaliate and the latter because they do not engage in a rational calculation of the costs and benefits of their actions.

These twin concerns came together in a "perfect storm" between August 2002 and March 2003 during the debate over the U.S. invasion of Iraq. The war party argued, despite compelling counterarguments from deterrence proponents like John Mearsheimer and Stephen Walt ("An Unnecessary War," *Foreign Policy* 134 [January/February 2003]: 51–60), that Saddam Hussein was not a leader who engaged in rational calculation (Kenneth Pollack, *The Threatening Storm: The Case for Invading Iraq,* 2002), and that the danger of his transferring weapons of mass destruction to al Qaeda made deterrence infeasible and preemption the only option.

It is now clear that these concerns were overblown. Morover, Freedman lays out the real diplomatic and military costs of this transition from deterrence to preemption and prevention, and makes it clear that he would like to find some way to make deterrence relevant again. But it is not clear that his efforts to reformulate deterrence will have the desired effect. He argues that it is necessary to supplement our traditional *interest-based* notion of deterrence (the more we can threaten things of value to an adversary, the greater our ability to deter him) with a *norms-based* conception (the more our adversary shares our same sense of the limits of appropriate behavior, the less likely he is to transgress them) (pp. 4–5). But there are conceptual and practical difficulties with this approach to revamping deterrence for the post–Cold War international environment.



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The theoretical problem with this approach is that it shifts attention away from the traditional focus of deterrence—affecting an adversary's military calculations—and moves us into the completely different realm of changing the political processes within other states. It is not clear how thinking about this as "deterrence" advances our understanding of the more general process of influencing other states' behavior.

As a practical matter, Freedman's approach would produce similar results to the Bush administration's policies. If a high level of shared understandings among states about the acceptable bounds of their behavior is essential to make deterrence work, then it is not too much of a stretch to conclude that the great powers ought to take steps to impose such common understandings. The most reliable way of doing so is for all states to have the same domestic political systems. Many scholars and policymakers have come to believe that one particular type of domestic regime—democracy—is uniquely suited to externalizing domestic norms of cooperation into the international system. Hence, the outcome of this approach may not be a return to a world of stable deterrence.

Indeed, the Bush administration clearly shares many of these reservations about a strictly interest-based conception of deterrence, and this surely explains why the administration felt it could not continue to rely upon deterrence to contain Saddam Hussein. Beyond that, the administration's commitment to the expansion of democracy in the Middle East and elsewhere around the world is also justified in terms of making the United States secure precisely through the creation of a world based not on mutual deterrence, but rather on shared norms of behavior from unilateral efforts to spread democracy. Ironically, in order to achieve this world of shared norms, the United States is willing to unilaterally wage preemptive and preventive wars to establish it.

In sum, Freedman has given us a helpful diagnosis of the demise of deterrence since the end of the Cold War and its replacement by preemption. Unfortunately, his efforts to resuscitate it by combining interests and norms in a post–Cold War formulation of deterrence may not lead us away from the world of preemption and prevention that he quite rightly deplores.

**The Limits of International Law.** By Jack L. Goldsmith and Eric A. Posner. New York: Oxford University Press, 2004. 272p. $29.95.

**The Politics of International Law.** Edited by Christian Reus-Smit. New York: Cambridge University Press, 2004. 344p. $75.00 cloth, $29.99 paper.

— Chandra Lekha Sriram, *University of St. Andrews* and *University of Maryland*

Is international law really law? If it is law, is it effective in constraining state behavior against interest? These two questions have vexed international lawyers and those international relations scholars interested in international law who until the last 15 years or so were relatively few in number. There has been a proliferation of scholarship on these questions, and on bridging the gap between international relations and international law (IL). (To note only one recent survey piece, see Anne-Marie Slaughter, Andrew Tulumello, and Stephan Wood, "International Law and International Relations Theory: A New Generation of Interdisciplinary scholarship," *American Journal of International Law* 92 [July 1998]: 367–397).

These two books offer dichotomous perspectives on the debate, and from unusual quarters. While international relations scholars have traditionally been more interested in the efficacy question, the volume edited by Christian Reus-Smit, a scholar of IR (although the volume contains multiple contributions by international lawyers) is more concerned with offering an account, through the lens of constructivism, of how international law functions. Meanwhile, the book by Jack Goldsmith and Eric Posner, two legal academics, is concerned with the efficacy question, using rational choice theory more familiar to political scientists. These books differ not just in their selection of motivating question and methodology but also in their assessment of international law. While the contributors to the Reus-Smit volume examine the function of law and the relation between law and norms in the international sphere, Goldsmith and Posner are convinced that international law itself has no direct effect on state behavior, but rather that it expresses the rational interests of states and that when it ceases to do so, it will not constrain them.

The impetus behind the two questions, and the source of the rift in the second half of the twentieth century between the related disciplines of IL and IR is well known to many scholars of both, and need not be rehearsed in much detail here. The questions and the rift have been driven by the dominance of realism in international relations scholarship, and, arguably, legal positivism in international legal scholarship. The former suggested that the absence of a "Leviathan" in the international sphere meant that international law could not be law as it could not constrain sovereign states (Reus-Smit, pp. 14–44). The latter suggested that international law lacked the requisites of law, either sovereign authority or the appropriate legal apparatus (legislature, executive, and judiciary). While a variety of arguments have been adduced in response to these challenges, for our purposes only the constructivist response of Reus-Smit and his contributors, and the further challenge from the rational choice theory of Goldsmith and Posner, are of concern.

Reus-Smit et al. place themselves squarely in a broad tradition in IR theory, constructivism (see, e.g., Alexander Wendt, *Social Theory of International Politics,* 1999). Constructivists reject the idea that politics is simply about



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power, or maximizing utility, and instead argue that not just structures but also norms and ideas can shape behavior, further that social identities shape interests and behavior, and finally that those identities are socially learned (Reus-Smit, pp. 21–22). Their collective argument, in direct challenge to realism, is that norms and law can shape state behavior, even against apparent interest, because that interest itself is not fixed but can change over time. They reject any suggestion that law and politics are distinct entities in the international sphere, and also the idea that law is just a function of politics, or is always constrained by it. Rather, they argue that there is a feedback effect, as law shapes actors' conceptions of their own interests. The constructivists' account of norm development, further, has a strong affinity with the account by lawyers of the development of customary international law (Richard Price, "Emerging Customary Norms and Anti-personnel Landmines," in Reus-Smit, pp. 109–13). This account requires the presence of two elements: state practice and *opinio juris,* the belief of a state that the conduct is required. This element of belief is viewed by many constructivists as akin to the effect of norms upon identity, interest, and action.

The authors do this by examining the dynamics of law and politics in a variety of issue areas, including the use of force, the International Criminal Court, and international financial institutions. As with any edited volume, the contributions are uneven, but the message of the contributions is clear—international law really *is* law, and it does constrain state behavior. A helpful illustration may be provided by the chapter on the use of force (Dino Kritsiotis, "When States Use Armed Force," pp. 45–79). The use of force is a classic example of high politics, an arena in which realists would least expect law to constrain power. This chapter, in examining the historical evolution of doctrines surrounding the use of force, describes law as a process: Rules are laid down limiting the use of force, but exceptions are also articulated. States seek to justify actions through reference to those exceptions, rather than simply rejecting the rule, suggesting that states do respect the law (Kritsiotis, pp. 63–78).

Taken together, the pieces in the volume offer a strong case for international law, and indeed the relationship between international relations and international law as process, rather than a set of fixed rules that are accepted or rejected by states. The strength of the volume is the shared theoretical perspective of all of the authors, constructivism. Yet that perspective is also ultimately limiting. As the introduction itself notes, scholars from other perspectives, such as liberal institutionalism, have also sought to establish a role for law in international politics. A volume with a plurality of theoretical perspectives might have ultimately offered a more fruitful set of arguments, particularly to do with the purported "legalization" of international life (*International Organization* 54 [no. 3, Summer 2000], special issue).

Goldsmith and Posner have a rather more skeptical take on international law, rejecting claims that it has any autonomous influence upon state behavior. Rather, drawing upon rational choice theory used by some IR scholars, they argue that international law is, at most, a codification or representation of state interests. States create international law and then "comply" with it, they argue, because it offers a rational solution to the prisoner's dilemma or other challenges of international interaction in the absence of full information, which can be modeled. International law simply offers a helpful mode for signaling, provision of information to prevent or punish cheating, and so on. The authors could have simply offered an institutionalist account of law but decline to do so, rejecting the common claims by such scholars that while law may originally serve coordination or information functions, it ultimately has its own constraining force (Goldsmith and Posner, p. 16). Rather, the authors would agree with realists that law will cease to constrain when it is not in state interests. They further reject that there are any noninstrumental reasons, such as norms or identity, that might lead states to comply with international law. They argue that four possible reasons exist for state behavior "associated with international law": It arises merely because states have a coincidence of interest, is created to facilitate coordination or to facilitate collaboration, or is created to assist the coercion of the strong by the weak (pp. 11–14).

The authors offer detailed analyses of two types of international law—custom and treaty—in a number of domains, including use of force, human rights, and international trade. They are particularly skeptical of the force of custom, suggesting that many rules said to have reached the status of customary international law, such as the protection of civilian fishing vessels, have not, because in fact state practice is not as uniform as it is said to be, and the evidence of opinio juris is limited or also divergent. If such limited custom is problematic, then broader custom, such as "treaties are to be respected" (*pacta sunt servanda*), is even less likely to have substantive effect, according to the authors (pp. 14–15, 23–43). Rather, custom, like the development of treaties, serves a convenient function for states, helping them to calculate their interests and the likelihood that others will behave in a similar fashion. Goldman and Posner adduce similar arguments for the development of treaty law (81–162). They finally challenge the status of international law on rather different grounds—having sought to show that it is simply the result of rational choice calculations, they then argue that it also cannot be justified on coherent moral grounds (pp. 165–224). These arguments would appear to further challenge the constructivist emphasis upon norms having autonomous persuasive effects.

A review quotation from Anne-Marie Slaughter on the dust jacket says that Goldman and Posner "throw down a gauntlet likely to infuriate many traditional international



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lawyers." This is undoubtedly the case, and the book will be the subject of much controversy. I will limit my remarks for reasons of space to two instances in which the authors seem to estimate the potentially concrete power of international law: the success of the European human rights regime and the rapid development of international human rights law, including that resulting in individual criminal accountability. The former is claimed to be an instance of federalism, rather than one of states complying with obligations against interest. Yet this is too simple a move, for confederations do not simply appear but may themselves be the result of law-conforming behavior and perhaps the influence of norms. The latter is said to be meaningless, for states incur little or no cost by signing human rights agreements, whether or not they ever intend to comply. But the dismissive view of the effect of human rights law fails to take account of costly developments, including those creating the ad hoc war crimes tribunals for Rwanda and the former Yugoslavia, and of the International Criminal Court, which appear to run against (some) state interests, and with which so many states have been engaged, even altering domestic legislation in order to better cooperate with the institutions.

Neither volume definitively settles the questions posed at the outset, and the engagement and debate between the disciplines of IR and IL is likely to continue for some time. Each of these volumes does, in its own way, offer an important contribution to that debate.

**The Lesser Evil: Political Ethics in an Age of Terrorism.** By Michael Ignatieff. Princeton: Princeton University Press, 2004. 160p. $29.95 cloth, $16.95 paper.

— Jeanne Morefield, *Whitman College*

While Michael Ignatieff was no doubt motivated to write this book by the civil and human rights abuses justified by the Patriot Act and the treatment of prisoners at Guantanamo Bay and Abu Ghraib, the framing question of it is decidedly more abstract: How can liberal democracies defend themselves against terrorism without violating their most sacred principles? The book is to be commended for the forthright way it confronts the uncomfortable moral dilemmas posed by democracy's encounter with terrorism, but Ignatieff is less successful in his attempt to articulate a coherent "lesser evil" approach to these encounters. More disconcerting, however, is the way the formal qualities of his argument potentially silence the kind of self-reflection and historical scrutiny that ought also to play a vital role in any democratic state's—much less a superpower's—response to terrorism.

Ignatieff sets up his argument by contrasting the two foundational assumptions of liberal democracy. The first is specific, premised on the right of a people to exist and to govern themselves. The second is universal, centered on a basic adherence to a set of immutable human (or citizen) rights. In a terrorist emergency, the gap between these principles opens, as the "consequentialist," who desires to preserve the political community by any means necessary, comes into conflict with civil libertarian beliefs that certain means like torture violate rights to such a degree that the political community is no longer recognizable. The book is at its best when it dwells in this basic tension, when the author is most agonized over the moral trouble created by the clash between these assumptions.

But *The Lesser Evil* is not meant to tarry with the conjectural. Rather, Ignatieff's goal is to fashion the kind of "political ethics" necessary to guide our approach to terrorism, a "third way" between civil libertarian and consequentialist arguments. He does this by carefully distinguishing between different categories of terrorism, forms of intervention, kinds of human rights, and military and political responses in order to provide us with "principles to guide public policy" (p. 146). The hope is that, when faced with the "factual uncertainty" (p. 19) that inevitably characterizes terrorist events, policymakers can draw upon these distinctions and learn to uphold state security in a manner that accepts some violations of human rights as inescapable but strives to keep these "lesser evils" to a minimum.

For the most part, however, there does not seem to be much relationship between Ignatieff's distinctions and the "guiding principles" he develops. At its most benign, this disconnect is merely confusing. For instance, despite his careful treatment of the relationship between democracy and torture and his discussion of the difference between torture and "lawful killing" during wartime (p. 137), it is ultimately unclear why torture limited to "sleep deprivation, permanent light or permanent darkness, disorienting noise, and isolation" is acceptable for the author and violations of "bodily integrity" are not (pp. 138, 47). More troubling, however, is the clear sense one gets that some of Ignatieff's distinctions serve no real diagnostic purpose but are merely smoke and mirrors behind which lurks his ideological agenda. Thus, while he expends enormous effort distinguishing between types of terrorism and insists that we understand these differences in order to develop policy responses, throughout the book he chooses to employ the Bush administration's term "war on *terror,*" rather than *terrorism.* As a basic human emotion, terror is ineradicable and, therefore, a war on terror requires no fixed date of cessation so long as those we determine to be "evildoers" (a word Ignatieff also uses once or twice without acknowledging its ties to White House speech writers) are loose in the world. In the end, the analytical clarity that he hoped to evoke with his typology of terrorism is vitiated by his use of the utterly mobile signifier "terror," which can be filled, in perpetuity, with the politically despised of the moment (recently, Mexican immigrants).

Perhaps more disturbingly, the author's insistence on the "need to learn from history" (p. 57) serves to occlude



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the essentially ahistorical premise of the book. Because we get detailed accounts of some history (e.g., of Japanese internment, the development of Russian nihilism), the book lures the reader into assuming that it is historically astute throughout. In fact, Ignatieff is virtually silent when it comes to twentieth-century American foreign policy. On only two occasions does he suggest that U.S. backing of undemocratic regimes in the Middle East might have some bearing on how the American state should now respond to terrorism (pp. 100, 156). But even then, his characterization of these relationships make it appear as though the United States were merely one hapless liberal democracy among many that had made some bad choices in friends. In fact, the United States is a superpower—what, in other writings such as his *Empire Lite* in 2003, Ignatieff is only too happy to call an "empire"—that actively sought to shape politics in the region by overthrowing a democratically elected government in Iran and arming dictators like Saddam Hussein. By subverting democracy elsewhere, the United States rendered its own foreign policy inconsistent with the kind of liberal democratic politics that Ignatieff values so dearly. Rather than dwelling in the implications of this tension, however, he chooses to ignore it and the results are discouraging. Where he could have combined his "lesser evil" approach to terrorism with a call for critical self-reflection by a democratic citizenry that has allowed an undemocratic foreign policy to be carried out in its name, he empties the American state of history and ends the book with platitudes. "Societies under the endless trial of self justification," Ignatieff concludes, "are apt to feel guilty about their success. But our success is not a fact to feel guilty about, and the failure of other societies is not our fault" (p. 168). In this instant, he transforms his own call to think critically about the relationship between terrorism and democracy into precisely the kind of ideological, even nationalist, rhetoric that the book's careful articulation of a "lesser evil" approach is meant to avoid.

**Resisting Rebellion: The History and Politics of Counterinsurgency.** By Anthony James Joes. Lexington: The University Press of Kentucky, 2004. 360p. $35.00.

— Richard Stubbs, *McMaster University*

At the end of this book, Anthony James Joes has written a two-page epilogue entitled "Conflict in Iraq," which notes the enormous task that the U.S. government has set itself in the long-term construction of a democratic order in the face of violent opposition and internal social conflict. And while the bulk of the book was obviously written prior to the war in Iraq, it can be read as a constructive critique of U.S. counterinsurgency policy in the months following the invasion. Joes draws on a wide range of cases from over two centuries of guerrilla warfare to illustrate what he sees as the key reasons for the success or failure of numerous counterinsurgency campaigns. His thoughtful and informative analysis provides a useful basis for a comparative perspective that has been largely absent from many of the assessments of the guerrilla war in Iraq and America's counterinsurgency policies.

Joes starts by exploring the origins of guerrilla wars. He examines a series of case studies from all corners of the world, noting the wide variety of factors that prompt people to take up arms against a more powerful enemy. He makes the point that whether guerrillas are motivated by religious oppression, nationalism prompted by a foreign invasion, or some other set of grievances, there are a number of factors that are crucial to sustaining a guerrilla campaign. These factors include maintaining morale and a sense of fighting for a just cause; an infrastructure that links the guerrilla fighters to the general population and provides then with intelligence, supplies and recruits; skillful leaders who have the respect of their followers; assistance from outside the country; and a secure base from which the guerrillas can launch their attacks. If these factors are important in sustaining guerrillas, then clearly governments conducting counterinsurgency campaigns must take them into consideration if they wish to prevail. Joes proceeds to analyze a number of these points and to show how successful governments have dealt with key issues.

The rest of *Resisting Rebellion* is devoted to a discussion of what Joes sees as proven ways in which counterinsurgency forces can gain the upper hand in a guerrilla war. He views four counterinsurgency campaigns as "especially instructive" (p. 232). The first two, both of which took place at the end of the nineteenth and beginning of the twentieth century, were the British defeat of the Boers in South Africa and the American victory over Emilio Aguinaldo in the Philippines; the second two, both of which occurred during the late 1940s and 1950s, were the success of the British against the Malayan Communist Party and President Ramon Magsaysay's U.S.-backed campaign against the Huk rebellion in the Philippines. From these experiences, and others, Joes singles out as especially vital to a winning counterinsurgency strategy a series of basic objectives: the need to isolate the conflict area and especially to prevent outside help reaching guerrilla forces; to establish security for civilians by committing enough resources, including troops, to maintain full control of a region threatened by guerrilla warfare; to treat all members of the general population, including guerrilla prisoners, in accord with the laws of the land and the highest "civilized standards"; to separate the guerrilla forces from their general supporters by addressing people's grievances; to develop a legitimate democratic government; and to establish an effective intelligence service. Joes comes back to these themes in an intriguing chapter in which he compares national approaches to counterinsurgency. And in a final chapter, which brings together the major threads of



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his argument, he sets out what he deems to be the vital elements of a successful counterinsurgency policy.

While there is considerable merit in much of the author's analysis, some weaknesses can be singled out. First, like many analyses of guerrilla warfare, there is little acknowledgment of the significance of the economic dimension to the rise and fall of insurgency movements. For example, economic factors are often crucial to the mobilization of support for guerrilla groups. Massive unemployment can frustrate even the best counterinsurgency campaigns, while full employment can sabotage the effectiveness of guerrilla movements. Certainly, past guerrilla wars have shown that a policy of resettling a segment of the population is usually not effective if the resettlement centers do not have an economic base. Moreover, diaspora communities, such as the Irish or Tamil communities in North American, have played a critical role in funding, and thereby sustaining, guerrilla organizations in their homelands. Second, more could have been said about the nonmilitary side of government and its role in counterinsurgency. To some extent, the impression is left that counterinsurgency is essentially a military activity. Obviously, the military have an important role to play, but past successful counterinsurgency campaigns show that if grievances are to be addressed and the general population neutralized or won over to the government, the civil side of the government needs to be actively involved. Finally, the fact that the discussion is rooted in a wide variety of historical examples, which in many ways is one of the book's strengths, tends to produce a somewhat disjointed discussion as the author moves quickly from one guerrilla war to another in order to illustrate the point he is making. There is also a tendency to repeat examples in different parts of the book.

But overall, this is an insightful, carefully considered, and instructive analysis. It is particularly useful in pointing out that the United States has successfully waged wars against insurgents in the past and that there is, therefore, no reason why it should not do so in Iraq. However, those in charge of mounting an American counterinsurgency campaign need to read up on their history. Joes provides an excellent guide as to where to look and how to interpret the historical examples.

**Ethnic Politics in Europe: The Power of Norms and Incentives.** By Judith G. Kelley. Princeton: Princeton University Press, 2004. 264p. $35.00.

— William Safran, *University of Colorado, Boulder*

The subject of this study by Judith Kelley is the international factor in ethnic minority politics—the role of external institutions and norms in the democratic development of several countries and the policies regarding their ethnic minorities. These institutions, more specifically the European Union, the Council of Europe (CE), and the European Security Conference, have secured commitments and undertakings from national governments regarding human rights in general and the protection of minority groups in particular. This is especially true of the CE, which has admitted a number of states on the basis of promises only.

The influence of these institutions is exerted via incentives, such as admission to membership in the EU, and pressures, such as the delay in 2000 in processing the application of Slovakia for such membership. Both may be based on "conditionalities" as articulated, for example, in the United Nations Declaration of Human Rights. Like many recent studies on the linkage between international and domestic politics, this book makes a strong case for the efficacy of third-party involvement in ethnic problems and is clearly informed by the rational choice approach.

Kelley posits several hypotheses regarding institutional engagement, and she argues 1) that the domestic policies of countries toward their ethnic minorities are influenced by the EU and the CE via membership conditionality and normative pressures; 2) that membership enticement has been particularly effective and has generated less opposition than have normative pressures; and 3) that policy outcomes are more compatible with international norms than with the preferences of the dominant domestic actors. She makes it clear that cost-benefit incentives in the cases examined here relate to politics rather than economics, which explains why there is no discussion of investments and only cursory mention of the World Trade Organization and the World Bank.

Kelley does not come to a categorical conclusion about the relative weights of "carrots" and "sticks." She is aware of the fact that the impact of incentives depends on the assumptions by the government in question about the capacities of the international actors and the seriousness of their promises and threats. She argues that "conditionality will only work if the cost-benefit calculations come out in favor of a policy change" (p. 40). In other words, incentives work only if the anticipated payoff is convincing—which in turn is a function of the credibility of the international organization offering the incentives. Moreover, these incentives work only if domestic support is sufficient and internal political resistance is weak.

The book title is somewhat misleading, because the focus is not on "Europe" but on selected ex-communist countries in East-Central Europe, specifically Latvia, Estonia, Slovakia, and Romania. These four countries reflect a certain symmetry: The first two contain significant Russophone minorities, and the second two, important Magyar communities, all face "beached" diasporas—minorities whose ethnonational brethren constitute majorities in neighboring countries. In all of them, factors have converged in a way not seen elsewhere: International organizations have been interested in appealing to them; membership in the EU has been offered to them; international norms have been, by and large, positively received;



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and incentives have worked reasonably well. Their experience with communist regimes has made them more receptive to democratic norms than has been true elsewhere, for example, most of the Middle East and Africa. It would be interesting to speculate to what extent the findings in this study apply to other former communist countries, such as Hungary and the Czech Republic, where discrimination against the Roma persists; Belarus, which continues to be Stalinist; and former Yugoslavia, where the impact of international organizations has been uneven and controversial. Even in the countries examined in detail, the political parties opposed to extended minority rights are strongly represented in parliament and are part of the governing majority. This may suggest that once a country has eaten the carrot, that is, has been accepted as a member of the EU and the CE, the incentive is no longer relevant and the norms are less strongly adhered to.

The book provides summaries of the postcommunist domestic politics of the countries dealt with, in particular their ethnic minority situations, and an analysis of the role played by language in determining voting rights and entitlement to welfare state benefits. It contains a number of tables, including a catalog of institutional mechanisms associated with normative pressures and a list of laws and policies adopted during a ten-year period regarding ethnic minorities and, presumably, responding to international pressures and norms, as well as the degree of opposition to them. The laws and legislative process on education, language, electoral participation, and citizenship are examined in detail. In doing this, Kelley is aware that there is a lag between legislative enactment and implementation, either because of deliberate foot dragging or because "the state does not have the capacity to enforce [a] policy for some years" (p. 57).

Kelley acknowledges the limitations of her study. To be sure, "ethnic issues have had high priority on the international agenda," but "international institutions could not and did not get involved in every ethnic issue, nor did they have the political will to engage equally in all countries" (p. 5). This statement applies a fortiori outside East-Central Europe, if one considers the international indifference to events in Chechnya, Tibet, Rwanda, and Georgia. In fact, the author introduces some uncertainty about the extent to which her findings can be generalized, and she admits that "few comparative studies systematically evaluate the effectiveness of [the international organizations'] efforts toward persuasion in terms of achieving policy goals" (p. 9).

*Ethnic Politics in Europe* is empirically based, well-documented, and competently crafted, and it devotes a good deal of space to laying out the analytic approach, research design, and theoretical underpinnings. More than a third of the book is devoted to the introduction, theory, and methodology. It contains a plethora of notes, a good bibliography, and a useful index. There is no doubt that it is a significant contribution to the literature on the management of ethnic conflict.

**Egalitarian Capitalism: Jobs, Incomes and Growth in Affluent Countries.** By Lane Kenworthy. New York: Russell Sage Foundation, 2004. 222p. $32.50.

— Peter V. Hall, *University of Waterloo*

Two decades of rising wage and household earnings inequality in the world's wealthiest nations make the guiding question of Lane Kenworthy's book both timely and important: "[M]ust we give up on the vision of a dynamic and productive yet relatively egalitarian form of capitalism?" (p. 1). To answer this question, Kenworthy presents a careful comparative analysis of income inequality in the countries of northwestern Europe, North America, and Australia during the 1980s and 1990s. He shows that there is not necessarily a trade-off between equality and income growth, although there may be a trade-off between equality and some categories of employment growth. Hence, he argues that with an updated, pro-employment version of the welfare state, we need not give into growing disparity in advanced capitalist societies. Whether this will be desired by all and achieved remains to be seen; nevertheless, the book is an important antidote to the "there is no alternative" type of thinking that pervades much contemporary discourse on economic globalization.

After brief and highly readable introductory and methods chapters, Kenworthy presents his findings in five tightly argued empirical chapters. In Chapter 3, he shows that while individual and household earnings inequalities have risen everywhere, in contrast to the United States and the United Kingdom, more egalitarian countries, such as Sweden, Finland, Denmark, and Norway, were able to overcome the negative effects of employment loss through government redistribution. Hence, there is nothing inevitable about rising household income inequality, but what does lower inequality imply for economic growth? In addition to national comparisons, in Chapter 4 the author also presents some results comparing U.S. states, and he finds support for his position that the relationship between inequality and growth is theoretically indeterminate. However, his evidence does suggest that there is a positive relationship between employment creation, especially in private-sector consumer service jobs, and inequality (Chapter 5). In Kenworthy's view, this need only be a problem if posttransfer household incomes remain unequally distributed. The experiences of individual countries indicate that several were able to address this problem (Chapter 6). Success stories are not limited to the Nordic countries or even continental Europe, but also include "anglo" countries such as Australia and Canada (Chapter 7).

Data issues are clearly central to debates about inequality, and one particular challenge facing this analysis is the seemingly unlimited number of explanatory variables



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compared to the very small number of cases. Income data from the Luxembourg Income Study, the study's primary data source, are applicable for only 15 national cases. To overcome this problem, Kenworthy differentiates most variables (e.g., change in employment or income is compared to change in inequality), and he also employs the neat device of running repeated regressions that use all the possible three-variable combinations of independent variables to explain a given dependent variable. This analytical device is indicative of a broader methodological standard that he applies to the evidence. Indeed, one of Kenworthy's major achievements with this book is that he is not blinded by the data. Sure, he expects statistical significance across his repeated regressions, but then he also demands that his findings conform to theoretical predictions and be consistent to a historically and contextually sensitive narrative. As a result, his findings are presented in an accessible and nuanced way, making this a wonderful model for students writing dissertations in economics, sociology, political science, and public policy. Indeed, *Egalitarian Capitalism* is appropriate for graduate-level courses in several social science disciplines.

That said, I was less convinced by some of the arguments in the policy-oriented concluding Chapter 8. Kenworthy's policy prescriptions boil down to the need for more employment-friendly policies and special attention to equal opportunity in job access. There is much in his list of policy prescriptions to agree with, such as well-targeted active labor market interventions and other supports designed to address the increasing instability of work, as well as improved public child care, schooling, and health care. While all these policies will reduce the cost of employing, somewhat more problematic is the author's proposal for directly reducing wages to spur employment growth in some contexts. This necessarily implies greater individual earnings inequality, which, he argues, can be solved by earnings subsidies or employment-conditional negative taxes, such as the U.S. Earned Income Tax Credit. But at what point does this pro-employment approach toward reducing inequality reach its limits?

First, lowering the cost of employing through transfer policies could have perverse productivity effects at the micro- or firm level by reducing the incentives to train, innovate, and improve workplace relations, thus lowering economic growth in the long run. Second, relying on the welfare state to fill the egalitarian gap resulting from lower wages arguably leaves something of a political deficit. While this problem is shared by all top-down welfare programs, the "low wages and welfare" strategy may be particularly undesirable if it disarms the very constituency for the package of tax policies, active labor market policies, and other measures that the strategy assumes. Third, it is by no means clear whether, and in what sense, this political choice even exists in most contexts. Although Kenworthy does note the effects of deeply ingrained institutions in some countries, such as "pattern" or corporatist wage setting, he rather too quickly dismisses the ideas of institutional complementarity and coherence. There is no reason to think that what might be tried somewhere in the American federal experiment will fit easily elsewhere. Not that there is anything wrong with trying, and this is Kenworthy's vital contribution.

**The Remnants of War.** By John Mueller. Ithaca, NY: Cornell University Press, 2004. 272p. $29.95.

— Stathis N. Kalyvas, *Yale University*

Rarely does a cover convey with such accuracy a book's central argument as in John Mueller's *The Remnants of War*. Shot by Ron Haviv, maybe the most iconic photographer of the Yugoslav wars of the 1990s, the cover picture shows a Serb soldier celebrating the fall of the town of Vukovar. The middle-aged soldier is drunk as he downs a bottle of liquor with the town's ruins in the background. In fact, he looks more like a homeless man in any European or American city than anything we would associate with the modern military profession—an observation that summarizes Mueller's four-tiered argument.

First, Mueller argues, the world is experiencing a secular decline of major interstate war. While this decline has a structural and institutional underpinning (namely, the dramatic rise of state capacity, domestically and internationally), it is driven primarily by "idea entrepreneurs" who have managed to produce a massive and deep change in popular values, comparable to the decline in popularity of dueling and slavery. Second, modern civil wars are mostly fought by predatory violence entrepreneurs and armies consisting of little more than packs of criminals and bullies seeking to exploit state weakness in the poor periphery of the developed world. Third, because these warlords are undisciplined cowards, they can be easily defeated by modern professional armies; however, military interventions are unlikely because of the domestic politics of advanced industrial democracies. Last, there may be hope for an internal, as opposed to an external, solution to this problem of "residual war." As governments of poor countries become more effective, they may be able to crack down on criminal warlords more effectively than they have been until now.

This is a sweeping, multifaceted, and complex argument that speaks to multiple research programs in political science, generates several policy recommendations, and addresses central issues of our time. I found the parts on the decline of major war, in particular, to be absolutely fascinating, and the effort to conceptualize violent conflict on a continuum going from small crime to terrorism to be very stimulating. In short, this is a nice example of a rich and erudite book that speaks to a larger public without sacrificing scholarly thoroughness.



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A book review of this size can do little justice to such a multifaceted argument, hence my focus on one dimension of the argument, the character of modern civil wars: This is the part most central to the overall argument and certainly the newest component of an intellectual project that Mueller has honed over a number of years and publications.

The argument about the essentially criminal aspect of civil wars echoes the well-known theories of "new wars" and dovetails with the equally known claim about "greed" as a causal factor behind civil war. The main difference between Mueller and the proponents of these arguments lies in interpretation. What is new for the proponents of the "new war" and "greed" theses is just residual for Mueller, the kind of violent conflict that becomes visible after major forms of war have disappeared: Contemporary civil wars are "essentially ancient forms of criminal war in which criminals and thugs . . . engage in warfare in much the same way they did in medieval and early modern Europe" (p. 86).

Mueller makes a bold case for his argument, culling anecdotes from several sources and introducing nuance when there is a need for it. For example, he recognizes that the distinction between disciplined and criminal warfare is ideal-typical and often fuzzy. He also distinguishes between three types of civil warfare: "mercenary" warfare fought by recently empowered and unpoliced thugs, rather than ordinary citizens; "brigand" warfare related to state failure; and disciplined civil warfare, which does not display the kind of pathology evidenced in the former two types. Typical examples include the wars of former Yugoslavia and Rwanda (mercenary warfare), Liberia and Colombia (brigand warfare), and Sri Lanka (disciplined warfare). As these examples make clear, this is a hard distinction to measure and sustain. The Rwandan case, in particular, is the toughest one to fit into this framework.

Ultimately, Mueller forgoes the systematic exploration of the processes that produce these different types of civil warfare in the first place. Had he not, he would have been forced to examine the analytical content of this distinction in more depth. At present, this distinction hinges on rather fuzzy evidence and a commitment to a single dimension of civil war, namely, the fighters' motivations—hence, the unabashedly military-centric character of his analysis of civil war. Political and social elites, as well as the masses, play a minor role in his account. Besides the obvious pitfalls of such neglect, this choice generates two flaws. First, although Mueller is right to target the uncritical uses of ethnicity in many descriptions of civil wars, his own account remains seriously undertheorized: Ethnicity (or any other cleavage, for that matter) is reduced to an ordering cleavage, a selection mechanism used by thugs for targeting their victims. It may well be that ethnicity is epiphenomenal to the whole story, but such an argument would require more solid theoretical and empirical foundations. This weakness leads to some inconsistencies in the narrative as mass preferences enter surreptitiously into the analysis, when "fanatics" suddenly appear among the various groups of criminals, or revenge is subsumed to criminal activity. Second, he analytically conflates evidence about the small size of perpetrator groups with evidence about criminal motivations. However, such conflation is unwarranted. The fact that civil wars entail the participation of small groups of individuals rather than masses is compatible with all sorts of motivations among both categories.

Looking at Haviv's entire portfolio, rather than just the picture of the drunk soldier in the cover, one is confronted with the multidimensionality of the wars in the former Yugoslavia. Consider some shots: A Serb man cries as Croatian forces capture his town; well-disciplined Yugoslav federal troops celebrate a victory; a Serb woman burns all her socialist books; Bosnian crowds demonstrate while holding Tito's picture. Whether these dimensions are essential or incidental to the war's causality remains an open question, to be investigated both theoretically and empirically. Mueller should be commended for having written an ambitious and stimulating book that poses these fundamental questions with clarity and boldness.

**Economic Governance in the Age of Globalization.**
By William Tabb. New York: Columbia University Press, 2004. 528p. $69.50 cloth, $29.50 paper.

— Robert Hunter Wade, *London School of Economics*

Martin Wolf, the influential economic correspondent of *Financial Times,* concludes his book *Why Globalization Works* (2004): "What the successful countries all share is a move towards the market economy, one in which private property rights, free enterprise and competition increasingly took the place of state ownership, planning and protection. They chose, however haltingly, the path of economic liberalization and international integration. This is the heart of the matter. All else is commentary" (pp. 143–44).

Wolf echoes the orthodoxy of our age, though he presents it as an embattled minority view in danger of being swamped by "the enemies mustering both outside and inside the gates" (p. 4), also known as the "anti-globalists." The large majority of professional and academic economists working in North America and Europe would broadly agree with him, as would a majority in the international relations subdiscipline of international political economy (IPE). The difference between them and Wolf is readability. Wolf writes in plain English. The academics use the methodology of formalization to polish the surface until icily dull, and practice an almost rancorous rejection of interpretive theory, springy narrative, or adventitious insight.



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William Tabb's book is different. It takes a jaundiced view of the claims of liberal globalists—claims that the worldwide shift toward liberal market economies since the 1980s has brought faster growth, less poverty, more income equality, and more stable societies. It goes on to take a jaundiced view of the governance of our present global state economic governance institutions (GSEGIs), seeing them as dominated by a tiny group of states that negotiate international rules and regimes among themselves and then present these "club" goods to other states to sign onto. The other states are then often faced with the "mugee's choice": Sign up in the interests of "preserving collegiality" or face dire penalties, such as exposure to the whims of the U.S. Congress in granting access to the U.S. market.

Tabb makes extended use of the analogy with "merchant law" of medieval and early modern times (lex mercatoria). This body of "soft law" provided for nonstate arbitration for transnational business disputes where private contract practices differed from one community or state to another; it was formulated and enforced by the cross-border business community itself. It thus operated outside the norms of each territorial community or state, and was not subject to accountability from that source.

Tabb argues that since World War II, international rules have increasingly supplanted national rules and standards in many economic domains of application (a new merchant law). Those international rules themselves are formed in a "club" process dominated by the United States—its government, its courts, its firms, its scientific bodies, its nongovernmental organizations. The United States has used its preeminent position to push for free trade and free capital mobility and for standards of international law derived from its own national law.

When markets had borders that corresponded roughly with the borders of political units, citizens could collectively regulate market competition to ensure that it advanced a notion of the common good formulated through a political process. With free trade and free capital mobility, however, politically determined limits on market competition are far more difficult to exert. Preferences for an egalitarian capitalism, a universal Sweden, never get expressed because the world lacks the institutions through which such a collective choice could be made. Instead, free trade and free capital mobility tilt the world willy-nilly in the direction of U.S.-style inegalitarian capitalism and noncorporatist democracy.

Tabb argues that the GSEGIs—notably the International Monetary Fund, the World Bank, and the World Trade Organization (which he examines at length)—are the spearhead of this universalizing of liberal economic rules; and he shows how they constitute a nascent international state for which previous notions of sovereignty, accountability, and equity miss the point. He has good case studies of the Multilateral Agreement on Investment, debt relief, and the halting counterhegemonic formation of an East Asian economic zone.

In terms of readability, the book is along toward the Wolf end of the spectrum (though Columbia University Press deserves no credit for its copyediting—too many typos and mispunctuations). It is up-to-date, with plenty of material on the foreign economic and political policies of the George W. Bush presidency, a subject on which Tabb delivers some of his punchiest lines. Each chapter is headed by choice epigraphs, a bonanza for examiners looking for "Discuss" questions. The book is also unusually well informed about the academic literature across economics, IR, IPE, and security studies, bringing insights from all or several of these disciplines to bear on the subject at hand. On the other hand, the book contains not a single table or graph. And it lacks a coherent theory of capital accumulation and class formation, which might have supplied an inner dynamic behind the processes of governance that it interestingly discusses. On a profundity scale, it certainly goes deeper than most of the surfacy IPE literature, but it could have gone further to the core.

*Economic Governance in the Age of Globalization* is to be recommended for courses in IPE and IR-with-an-economic-accent, for Masters and Ph.D. students, and for upper-level undergraduates of hard-working disposition. It makes a good counterbalance to more standard texts, especially for looking at U.S. actions through less rose-tinted spectacles, without descending to the polemics of Wolf's anti-globalists.

**Global Institutions and Social Knowledge: Generating Research at the Scripps Institution and the Inter-American Tropical Tuna Commission, 1900s–1990s.** By Virginia M. Walsh. Cambridge, MA: MIT Press, 2004. 208p. $50.00 cloth, $20.00 paper.

— David F. J. Campbell, *University of Klagenfurt, Austria*

What is the interrelating link between institutions and knowledge, primarily scientific knowledge? Conventional international relations literature focuses typically on the impact of knowledge on institutions. Virginia Walsh's interest, however, is to "turn the causal arrow" by focusing on the "institutional dimension" of knowledge creation: How do global political and economic, but also epistemic, institutions shape knowledge generation? (pp. 4–5, 38, 128, 130). Here, the scientific disciplines qualify as epistemic institutions (p. 34). From that point of departure, underscoring an institutional perspective, the emphasis concentrates on the "institutional mechanisms" through which knowledge is being formed. Groups leverage institutional mechanisms to "generate knowledge and fix beliefs" for the purposes of 1) establishing common understandings, 2) repairing uncertainties, and 3) directing inquiry by "fixing its direction" (pp. 3, 9, 38).



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Walsh explicitly borrows the term *belief fixation* from the philosopher Charles S. Peirce (*The Fixation of Belief,* 1877). She also draws the connection of defining knowledge as an "accepted belief" of individuals or groups (pp. 19–20, 41–42). In that understanding, knowledge is a "social knowledge" and observable, and consequently we can speak of "social beliefs" about "social facts" (pp. 8–9, 40–41, 47–49). Groups and individuals frequently are challenged to "make important decisions under conditions of uncertainty." Beliefs should reduce such uncertainties, thus supporting groups and individuals in their decision-making and problem-solving interest and emphasis (pp. 21, 44, 129, 137).

With regard to conceptual comprehensiveness, Walsh places the term *institution* between *organization* and *system.* Referring to the cited Oran R. Young (*Science Plan,* 1999), an organization represents by tendency the material entities, empirical configurations, and social practices of actors, while institution also addresses the "rules of the game" that underlie and structure the empirical patterns (p. 6). The author stresses that institutions "consist of clusters of social roles, to which rights and rules (or responsibilities) are attached" (p. 130). At the same time, she carries the broadest interpretation of an institution closely to the concept of a system, by claiming that the global political, economic, and epistemic institutions represent "complex systems" (pp. 38, 43). In a furthest understanding, there might be no difference between a social institution and a social system. Walsh explicitly acknowledges that in addition to an institutional dimension, there also exist organizational dimensions of knowledge generation (p. 137). Where the state, market, and scientist or the political/economic/epistemic institutions overlap, so-called hybrid institutions operate (pp. 35–38, 132).

Walsh sets up for discussion the following three mechanisms for fixing social beliefs: 1) The *positional fix:* A person, often a decision maker, uses his or her position, that is, social role with the attached rights and rules, for selecting beliefs or framing research (pp. 11, 38–39, 130). This may be seen as a statement supporting the claim that leadership qualities crucially impact organizations. 2) The *statutory fix:* Based on "ideas embedded in formal or informal rules" of institutions, beliefs (even if only temporarily) are being established (pp. 11–12, 39–40). Embedded ideas have the power to guide the selection and acceptance of beliefs and of research, and, in that respect, institutions can act as "amplifiers" of ideas, according to a cited quotation of Peter Haas and David McCabe (*Learning to Manage Global Environmental Risks,* 2001). Put in other words, acting persons can refer to the embedded ideas when decisions have to be made that influence knowledge generation (p. 130). Taken to an extreme, one could postulate that nothing like an idea-free or idea-neutral institution exists. 3) The *committee fix:* Regular formal or informal group meetings, within the framework of accepted "regularized practices," can shape beliefs (pp. 12, 40, 130–31). This resembles a typical practice for organizations, is being regularly applied by employed scientific or other expert staff, and refers to the conviction that through procedure, a consensus about content (knowledge content) may be established.

For the empirical testing of her conceptual propositions about "institutionalism" (p. 6), Walsh focuses on two structured comparative case studies: the Scripps Institute of Oceanography [www.sio.ucsd.edu], 1900–70, and the Inter-American Tropical Tuna Commission (IATTC), 1950s–90s [www.iattc.org]. Importantly, she contrasts the explanatory powers of institutionalism with "neorealism" and the "interest group approach" (pp. 10, 22–23, 131). "Poststructuralism" is shortly reviewed, but not integrated in the juxtaposition of different concepts, since poststructuralism can be regarded as incommensurable with institutionalism (pp. 17, 131). According to the author, institutionalism predicts better institutional decision making and behavior: first, when compared against neorealism in the case of the Scripps Institution, and second, when compared against the interest group approach in the case of IATTC (pp. 131–32).

It is interesting to evaluate the propositions of Walsh's institutional dimension of knowledge generation in the context of other current theories and concepts of knowledge:

1. *Social knowledge changes society:* The author argues that beliefs (i.e., social knowledge) are necessary for interest perception and for deciding on action (pp. 129, 137). Similarly, Stuart A. Umpleby defines social theories as theories that, "when acted upon, change social systems" ("Cybernetics of Conceptual Systems," *Cybernetics and Systems* 28 [1997]: 635–51).
2. *Accepted beliefs and paradigms:* It appears that Walsh's approach of introducing accepted beliefs as (social) knowledge falls closely to how Thomas S. Kuhn defines a "paradigm" (*The Structure of Scientific Revolutions,* 1962). Walsh elaborates on the mechanisms through which institutions fix beliefs, thus specifically acknowledging organizational ramifications. Kuhn emphasizes (also, but not only) the epistemic dimension, claiming that different paradigms can employ differing conceptual explanatory power. Walsh does not refer to Kuhn.
3. *Political/economic/epistemic institutions and Triple Helix:* Walsh preferably discusses institutional knowledge generation in the global context of political, economic, and epistemic institutions, and also brings so-called hybrid institutions into perspective. This design reveals similarities with the Triple Helix concept of Henry Etzkowitz and Loet Leydesdorff, who focus on the interaction of state, industry, and



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academia, paraphrased as "helices." They also underscore the importance of "tri-lateral networks and hybrid organizations" for innovation, which specifically place in the overlap area of the "university-industry-government relations" (e.g., see Etzkowitz and Leydesdorff, "The Dynamics of Innovation," *Research Policy* 29 [2000]: 109–23). Walsh cites Etzkowitz.

4. *Knowledge generation and Mode 1/Mode 2:* A new understanding of knowledge, which crucially influenced academic and decision-making communities in Europe, bases the properties of "Mode 2" of knowledge production (somewhat in contrast to "Mode 1," the conventional scientific knowledge production at universities) on "knowledge produced in the context of application"; "transdisciplinarity"; "heterogeneity and organizational diversity"; "social accountability and reflexivity"; and "quality control" (Michael Gibbons et al., *The New Production of Knowledge,* 1994). Mode 2 crucially supplements Mode 1, without replacing Mode 1. Walsh does not cite the Mode 1/Mode 2 authors, but, as in the case of Kuhn's paradigm approach, there might be opportunities for a mutual conceptual benefiting between Walsh's emphasis on institutions and the Mode 1/Mode 2 claims (and Kuhn's paradigms).

All these reviewed knowledge theories contemplate the epistemic considerations of knowledge generation. From an institutional perspective, "failures of collective epistemic action" can have "institutional sources," as stated in Walsh's conclusion (p. 135). Attempts to close knowledge gaps, therefore, may demand redesigning the institutional framework of a society or of the global international system.

**Men, Militarism, and UN Peacekeeping: A Gendered Analysis.** By Sandra Whitworth. Boulder, CO: Lynne Rienner, 2004. 225p. $49.95.

— Margaret P. Karns, *University of Dayton*

The rapid expansion in United Nations peacekeeping operations in the 1990s has inevitably led to official and scholarly scrutiny of their efficacy and shortcomings. Sandra Whitworth offers a critical eye and questions of gendered analysis by studying the subjects of the UN's peacekeeping operation in Cambodia, whose presence often meant greater insecurity as well as the (mis)conduct of soldiers from Canada—a country historically linked to major peacekeeping contributions—in Somalia. Using these two case studies, Whitworth highlights a basic contradiction inherent in peacekeeping that stems from its heavy reliance on soldiers whose training emphasizes masculine traits of violence, homophobia, racism, and aggression, yet whose tasks as peacekeepers require limiting violence to self-defense and providing a benign, altruistic presence. She uses gender analysis to examine these contradictions, to challenge peacekeeping's association with alternatives to military violence, to show how peacekeeping forces can increase local people's insecurity, rather than alleviating it, and to critique the use of soldiers for missions requiring unsoldierly skills.

*Men, Militarism, and UN Peacekeeping* initially explores peacekeeping's contradictions as well as past and present narratives. The former arise from the tendency to raise technical questions about "best practices" or issues of command and control, rather than political questions, such as "who benefits," who is excluded, and what effect operations have on the people in countries where peacekeepers are deployed. As a result, in her view, debates about the efficacy of peacekeeping take place largely among technical experts (predominantly men), excluding whole groups of interested people from the discussion (most especially those affected by missions, especially women). Furthermore, she notes that for many countries, post–Cold War peacekeeping provides a rationale for their militaries (and, one should note, a source of revenue). Absent, in her view, has been any critical questioning of who conducts peacekeeping and who needs it—the "us" versus "them" perspective—and an indigenous view of what peacekeeping is supposed to accomplish. She asks whether UN peacekeeping is the contemporary civilizing mission of the West, the latest social engineering project to impose Western values and institutions on the conflict-prone Third World.

The major sources of data and the core case studies are contained in chapters on the UN operation in Cambodia, viewed largely from the perspective of Cambodians, especially women, and on Canada as a peacekeeping contributor, specifically in Somalia. Although there are brief discussions of contingents from a few other countries, Whitworth does not attempt to provide a more systematic examination of what countries provided the large increases in peacekeeping troops for post–Cold War operations and where, other than in Cambodia and Somalia, there were problems of sexual assault, prostitution, increased HIV/AIDS, and racism, and to whose troops these were linked. Likewise, brief allusions to operations in Kosovo, the Democratic Republic of the Congo, and East Timor serve as examples of recent UN efforts to include gender advisers and gender units in peace missions, but they are not systematically examined.

The problems in Cambodia and Somalia are well known, as are recent problems among UN troops in the Congo. Also well known is the need for more peacekeepers in the 1990s and the reluctance of Western and especially Nordic countries to put their soldiers at risk in conflicts where there was no peace to be kept. These situations forced the UN Department of Peacekeeping Operations to recruit soldiers from countries without previous peacekeeping experience. Often these contingents had less training and



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discipline, and their governments were motivated by the financial benefits to be reaped from peacekeeping. This does not apply to the Canadians in Somalia, but it certainly applies to the Bulgarians in Cambodia. With respect to the former, racism was the problem, and for that, Whitworth's gender analysis is less helpful in elucidating its roots among Canada's soldiers. The gender approach does provide one critical lens through which to analyze the variety of problems in the UN operation, including the misconduct of the Bulgarians. One might wish, however, for more systematic gender analysis of the units that had the most problems and why, as well as more context for criticisms of other issues, such as the handling of refugees and the UN's effects on the local economy. And one should definitely ask what Cambodia would have been like without the UN operation, despite the latter's shortcomings. One should also note that in 1991, the UN lacked experience in mounting such a large, multifaceted operation in a short period.

Examining UN responses through gender mainstreaming, Whitworth notes that inserting gender concerns, including Security Council Resolution 1325 (2000), has increased emphasis on effectiveness, not on tough questions arising from the "radical political potential" of gender (pp. 120–21). In this, she reveals her desire for fundamental rethinking of UN peacekeeping as an institution. Likewise, in a richly documented chapter on "Militarized Masculinities," she examines the particular form of idealized masculinity that is inculcated into soldiers, both men and women, in modern state militaries, and she delves into post-traumatic stress disorder (PTSD) as an illustration of the fragility of the masculine construction of militarized male identity. The author concludes that soldiers who express feelings of fear and emotional pain associated with PTSD—or "traces of the feminine"—exhibit the very qualities required in missions to keep and create peace (p. 172).

Soldiers may not be the best peacekeepers, as Whitworth contends, but it is hard to imagine ready alternatives. Her analysis makes clear that women soldiers are not the answer since they, too, are inculcated with "militarized masculinity." Nor is it yet clear that gender-mainstreaming strategies are the remedy. The UN's experience in trying to recruit civil police for Kosovo illustrates the difficulties in mobilizing nonmilitary civilian capabilities for peacekeeping missions. Furthermore, the UN can only focus on human security (versus state security) if member states permit it to do so. It has moved in this direction over the last decade with increasing support for the "responsibility to protect" individuals through humanitarian intervention. Yet protecting human security almost inevitably requires enforcement action with the corresponding risks that accompany the presence of soldiers.

*Men, Militarism, and UN Peacekeeping* does not address these dilemmas directly, but the critical questions it raises enrich the debate over how best to deal with threats to international peace. Scholars interested in these issues, as well as those specifically interested in gender analysis of security-related issues, will find the book of considerable value. Those who want to understand the scope of the problems illumined by the author will, however, regret that the book provides snapshots of particular cases, not systematic analyses of post–Cold War UN peacekeeping operations.

**Media and the Path to Peace.** By Gadi Wolfsfeld. New York: Cambridge University Press, 2004. 284p. $70.00 cloth, $26.99 paper.

— Marwan M. Kraidy, *American University*

While the role of the mass media during conflict and war has been the subject of numerous studies in the field of mass communications, there is a paucity of research on mass media during peace processes. *Media and the Path to Peace* is an important step in remedying this lack. A study such as this confronts an unfortunate reality: The news media are attracted to war and conflict, while peace processes make for paltry news footage and thus are for the most part undercovered by the news media. Indeed, it is Gadi Wolfsfeld's thesis that news values and peace processes are inherently contradictory.

The book comprises seven chapters in addition to an introduction and conclusion. The first chapter establishes the theoretical framework and the second discusses the preliminary stages of the Oslo peace process between the Israelis and the Palestinians. Chapters 3 to 7 explore media roles in peace processes, with Chapter 3 discussing how the Israeli media affected the debate over the Oslo peace process, Chapter 4 teasing out how the Palestinians related to the Israeli media during Oslo, Chapter 5 looking at the news media in the Israel-Jordan peace process, and Chapter 6 using Northern Ireland as a comparative case study with Israel. Chapter 7 focuses on the collapse of Oslo and the conclusion provides policy recommendations.

The book's structure rests on six central arguments. First, because of the fundamental contradiction just mentioned, the media often have a destructive impact on peace processes. The four dominant news values of immediacy, drama, simplicity, and ethnocentrism are incompatible with drawn-out peace processes. The search for drama leads to sensationalism, while the values of immediacy and simplicity prevent systematic analysis grounded in historical facts. Ethnocentrism is especially detrimental to peace processes whose progress often depends on concessions that require public support, which depends on knowledge of and tolerance of the Other.

Second, the more support a peace process enjoys among the elite, the more likely it is that the media will play a positive role in the peace process. If the elite on each side is divided over the peace process, the news media will



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focus on that division and as a result will be distracted from focusing on the peace process itself.

Third, the media's role becomes more negative as crises associated with the peace process increase in frequency and severity. The author convincingly argues that "when things get bad, the news media often make them worse" (p. 30).

Fourth, the media influence a peace process following a politics-media-politics cycle, in which the political environment impacts the media environment, which in turn affects the political environment. This cycle is especially important during political waves, which Wolfsfeld defines as "sudden and significant changes in the political environment that are characterized by a substantial increase in the amount of public attention centered on a major political issue or event" (p. 32). The author demonstrates that the news media amplify political waves and give them structure and direction.

Fifth, the more sensationalistic the news media, the more negative their role in a peace process. For example, the author shows that the melodramatic style that characterizes sensationalistic media presentations has come to pervade the increasingly commercial media environment in Israel. Bottom line and peace concerns are mostly incompatible.

Sixth, the more the news media are shared by the different sides in a peace process, the more positive the role of the news media will be. This is clearly shown in Northern Ireland, where protagonists with a shared language and culture shared media, while linguistic and cultural differences between Arabs and Israelis prevented the sharing of media sources. This is a major reason explaining the positive role of the news media in the quest for peace in Northern Ireland. Shared media tend to mitigate ethnocentrism and foster self-criticism. After unionist militiamen murdered a Catholic workman, for example, the *Belfast Telegraph*—a unionist newspaper—referred to the killers as "terrorists." This trenchant criticism of one's own side is nearly never found in Israeli or Palestinian media.

The author of *Media and the Path to Peace* uses a combination of quantitative content analysis of news media and interviews with journalists, activists, and politicians. The analysis is fair overall, a laudable achievement in view of the highly politicized context of Middle East peace negotiations. Other strong points include an accessible style, a crisp theoretical framework, and attention to the intricate details of peace processes, including a clear chronological rendition of key events.

Missing, however, is a comprehensive analysis of the larger media sphere impinging on peace processes. Descriptions of news outlets, such as *Haaretz* in Israel, *al-Ra'i* in Jordan, and the *Belfast Telegraph* in Northern Ireland, are excessively brief and lack comparison with other media outlets in those countries. The rather general statement that "the Jordanian [media] model is more of a hybrid between a developed and developing state" (p. 140) is not elaborated. Expanding the range of media outlets beyond national newspapers and television would have made the analysis stronger. Notably, the exclusion of transnational media (*Al-Jazeera* gets a mere footnote on p. 110; The "CNN Effect" is acknowledged on p. 13 but not belabored; Hizbollah's *Al-Manar,* influential among Palestinians during crises, is not mentioned) leaves important questions about news media roles in peace processes unanswered.

Despite these issues, perhaps caused by space restrictions, this is a valuable book for scholars and policymakers interested in the role of media in peace processes and, to a lesser extent, in Middle Eastern politics and media. It is only to be hoped that more researchers in political science, mass communications, and Middle East studies will emulate Wolfsfeld and take the road less traveled of researching the less exciting but all the more important role of the mass media in peace processes.

**Crude Awakenings: Global Oil Security and American Foreign Policy.** By Stephen A. Yetiv. Ithaca, NY: Cornell University Press, 2004. 256p. $35.00.

— Øystein Noreng, *BI Norwegian School of Management*

Steve Yetiv's central argument is that oil stability has improved in recent years in the sense that the market is resilient, enjoying a self-reinforcing equilibrium by its ability to deter, mitigate, and contain threats to supplies. The author explicitly distinguishes oil supply stability from oil price stability, as oil prices have become more volatile while supply volumes have stabilized. Implicitly, he argues that price flexibility stabilizes supplies and that the world oil market has become more competitive and less rigid, so that relations between oil exporters and importers have become more pragmatic and less politicized. Attention is on the physical availability of oil: Disruptions are unlikely, not on oil prices or their impact on incomes, inflation rates, and trade balances, but historically, oil has always been available in the world market, at a price. The book would have benefited from a more systematic elaboration of the key argument, early on. He examines the oil market from the perspective of international relations, emphasising the projection of U.S. military power in the Middle East.

The major contribution of the book is to show the connection between concerns about oil supplies and the massive U.S. military buildup in the Persian Gulf (p. 59 ff.), and perhaps unintentionally that the Iraq War also was about oil (p. 209), besides Israel's security (p. 149). Explicitly recognizing 9/11 as an excuse to take on Iraq (p. 91), the author implicitly confirms that the foreign and energy policies of the United States include the offensive use of war.



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The author discusses the role of the (Arabian-Persian) Gulf in global oil supplies (p. 7), but not in world market price formation. Focus is on the risk of sudden supply disruption due to the political use of the oil weapon, not on the continuous impact of politics on the oil market. He belittles the importance of Saudi Arabia to oil market stability, but also points out, correctly (p. 40), that present conditions there differ from those of Iran in the 1970s, so that violent change and oil supply disruptions are unlikely. The author discusses the economic problems of Saudi Arabia (p. 33), but ignores the population pressure and the ensuing revenue needs that weigh on oil policy.

Yetiv dismisses the chances of a successful political use of the oil weapon today (p. 139). He maintains (p. 142) that the 1973–74 Arab oil embargo "unified western leaders in the pursuit of oil security" and "pushed industrialized countries to develop and exploit other sources of energy." This reviewer disagrees; the embargo split the Western countries, as Japan and Western Europe refused to follow the confrontational policies advocated by the United States, wanting to offset their loss of leadership by a countercartel to OPEC to push oil prices down. The outcome was the fairly toothless International Energy Agency, whose crisis plan was activated once only, in 1990. OPEC, securing higher oil prices, gave incentives to investment in other energy sources and in North Sea oil.

Analyzing the Gulf power balance, the author argues that according to economic and military indicators, the position of Saudi Arabia has strengthened at the expense of Iran and Iraq (p. 58), but overlooks Iran's demographic weight, progress in food self-sufficiency, and military potential. He presupposes that for a long time, Iraq will be under U.S. influence (p. 58). His key point is that with bases in Bahrain, Kuwait, Oman, and Qatar, in addition to Iraq, the United States has become the chief guarantor of oil stability (p. 76), and is well positioned to protect regional oil supplies by military means. His core argument is that U.S. military power, enhanced by the 1990–91 Gulf Crisis and the 2003 Iraq War, not oil supply economics, stabilizes oil supplies.

In the summer of 1990, before the invasion of Kuwait, Saddam Hussein voiced concerns that the United States would use its position to influence oil prices to its own benefit (p. 96). At the time, Iraq was practically insolvent, unable to service its huge war debts, and the alternative to bankruptcy was invading Kuwait. Against this backdrop, it is remarkable that the author does not pay more attention to economic threats to Gulf stability.

Yetiv understates the problems and mistakes of the U.S. occupation of Iraq (p. 208). Referring to the fall of Saddam (p. 76), he omits that it was achieved by foreign troops, not the Iraqi people. Comparing the U.S. occupation of Iraq with that of Germany and Japan after 1945 (p. 75), he overlooks the fact that Japan attacked and Germany declared war on the United States; Iraq did neither, and was the target of an unprovoked assault. The author praises U.S. nation building in Iraq (p. 76), but regime change was the third pretext for war, after claims of weapons of mass destruction and terrorist links were invalidated. To many Iraqis, the would-be U.S. liberators have become occupiers, compromising stabilization. Regional and oil supply stability would suffer from a protracted insurgency or an involuntary U.S. withdrawal.

*Crude Awakenings* is a scholarly book in the sense that it examines a defined issue from a distinct perspective, but the perspective is narrow and the author's notions of the oil market are simplistic, sometimes inaccurate. The largest and fastest growing market for Saudi crude is not the United States (p. 83), but Asia. To underpin his case of oil market resilience and cushions against supply disruptions, the author presents unrealistic forecasts about excess oil production capacity (pp. 159–61), for which neither policy changes nor capital investment is forthcoming as required. Consequently, Saudi Arabia remains of crucial importance for oil supplies. Assertions about oil privatization and Iraq leaving OPEC (p. 188) seem inspired by wishful thinking, not political realities.

Yetiv sees the U.S. military presence in the Gulf and Iraq as a factor of stability. Two years after the Iraq War, oil prices have doubled, less because of OPEC or the physical balance of supply and demand than because of a risk premium in markets that discount regional and oil supply instability.



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