

## BOOK REVIEWS

## Political Theory

**Our Lives before the Law: Constructing a Feminist Jurisprudence.** By Judith A. Baer. Princeton, NJ: Princeton University Press, 1999. 276p. \$55.00 cloth, \$18.95 paper.

Gayle Binion, *University of California, Santa Barbara*

It is a daunting assignment to review a book after it has garnered a major award bestowed by the organization that publishes this journal. Judith Baer's *Our Lives before the Law* not only is a very worthy recipient of the 2000 Victoria Schuck Award for the best book on women in politics but also is an erudite and wide-ranging critique of feminist thought with the goal of "forc[ing] feminists to confront mainstream discourse and mainstream discourse to confront feminism" (p. 175). This goal is in the service of Baer's desire to construct a new jurisprudence of sexual equality, one that avoids the pitfalls Baer perceives as inherent in the efforts of others to date.

The analysis ranges well beyond the boundaries of what is routinely conceived to be legal, indeed, well beyond the realm of the political, to draw upon all manner of feminist thinking. Of particular interest to Baer is the debate between those who focus on gender difference and those who focus on (male) gender dominance. Declaring that "this book will show that I am firmly on the side of the dominance jurists" (p. 11), Baer proceeds to interrogate the feminist theorizing that she believes has been unduly tied to the centrality of the body, indeed of genitalia, as sources not only of male/female difference but also of the relative disempowerment of women. She ponders why women are presumed to be psychologically, no less politically, structured around biology and the sexual and reproductive events in their lives. Robin West (e.g., "Jurisprudence and Gender," *University of Chicago Law Review* 55 [Winter 1988]: 1–72) and Nancy Chodorow (*The Reproduction of Mothering*, 1978) are here subjected to significant criticism for their lack of empirical evidence for the gender role analyses they offer and for their quasianalytic methods. Carol Gilligan (*In a Different Voice*, 1982) similarly receives an intellectual drubbing for her weak evidence of the different voice as well for having spawned voluminous works by others that valorize "care" as a feminine distinguishing attribute.

Baer defines all approaches to feminism focused on gender difference as "character" theories, which she rejects as irrelevant to the realities of women's lives; unequal power, not being different, is at the heart of the problem. As further evidence of what she sees as the foolishness of the difference enterprise, Baer makes the case that much of what is deemed "feminine" in legal analyses, such as emotionalism, is employed by the most retrograde of Supreme Court jurists. In sum, she expresses greater respect for the Catharine MacKinnon (e.g., *Feminism Unmodified*, 1987) situational school of thought, which views domination of men over women as both the source and consequence of law.

The book ends with a plea for reconstituting feminist jurisprudence in a variety of ways: give up the feminist monopoly on "care," challenge the excessive/unequal level of responsibility on women without the concomitant level of rights, address the liberal state's inability to connect "needs" with "rights" as compatible phenomena, and institute an imperative jurisprudence based on the expectation that society will meet human needs and ensure the survival of the species. From these premises expressed within the last few chapters, it is argued that one can imagine an equality of

rights and responsibilities between men and women. Baer does a fine job of building these principles into her critique of the law of reproduction, fetal protection, divorce, and corporate downsizing, at all points intertwining the conventional dictates of liberalism and indicating where within them feminism would demand modification.

In its analysis of difference and dominance as well as its deconstruction of the legal principles governing various areas of public policy, Baer's book makes a very valuable contribution to the literature on feminist jurisprudence and stimulates thinking about where the discourse has been, where it is, and where it should be going. But in some other ways the volume is less satisfying. Two points of concern or disagreement are noted below with reference to her critical assessment of the state of feminist jurisprudence and its identified weaknesses.

First, Baer criticizes feminist theory (including MacKinnon, with whom she generally agrees) for its attempts to develop grand theory that more often than not misses critical points. It is equally if not more plausible to perceive feminist jurisprudence as insufficiently grand, indeed, as partial, provisional, and inductive. Katharine Bartlett, in a pathbreaking attempt to outline the contours of feminist jurisprudence ("Feminist Legal Methods," *Harvard Law Review* 103 [February 1990]: 829–88), notes the self-consciousness of feminism toward issues of differences among women and the need to build theory from the realities of everyday life. This imperative has been at the heart of the work of a significant pool of feminist legal scholars, who insist on remaining deeply rooted in the empirical and eschew theory that is tied to abstract first principles. Indeed, Carole Smart's very compelling book, *Feminism and the Power of Law* (1989), concludes that it is law's demand for "grand theorizing" and its insistence on knowing where one's claim fits within predetermined theory that render the institution unreformably male and patriarchal. Thus, structurally, it may well be that the predominant modes of feminist legal thought cannot effectively compete with the abstract premises of liberalism, which dictate the primacy of such nonempirically based values as individual autonomy and institutional license.

Second, although Baer does an excellent job of demonstrating the problems she believes are associated intellectually and politically with difference theory, her critique may ignore the value of incremental steps toward sociopolitical change and may also focus wrongly on the difference model rather than on some of the uses to which it has been put. Martha Fineman, in an equally compelling analysis of feminist thought in the world of divorce and child custody (*The Illusion of Equality: The Rhetoric and Reality of Divorce Reform*, 1991), condemns "feminists" for insisting on gender neutrality in family law, which ignores the situational differences between men and women. As is to be expected in empirically rooted theorizing, in each case one must ask against which status quo in law is the criticized feminist activity, intellectual or litigative, occurring. In the case of marital law, gender neutrality arguably is a step forward in a system still bearing vestiges of unity of person and coverture. With respect to public policy more generally (re: Baer's work), the different voice proposition serves to address whether law is modeled on the life experiences of only men, and in no way is itself incompatible with dominance theory.

The problem for feminism is not that there is an interest in difference, it is how that difference would be understood and to what uses that stream of thought is put. I share much of Baer's concern that this enterprise is capable of undermining the quest for sexual equality, but the fact that she calls such theories character theories suggests that she sees the work as

essentialist. The best work in this area, however, very clearly rejects any notion of inbred difference and assumes that different life experiences (and certainly the range of life experience differs for women and men) have consequences for the way women view the world as well as the demands that they would make on the public policy system if that experience were empowered. Although Carol Gilligan cannot control how others understand her work, she has never claimed “natural” differences between the sexes. She has also never suggested that care be valorized as feminine, and she does not reject the idea that it be more equally redistributed as a responsibility.

**Prometheus Wired: The Hope for Democracy in the Age of Network Technology.** By Darin Barney. Chicago: University of Chicago Press, 2000. 340p. \$29.00.

Ted Becker, *Auburn University*

This is a welcome and provocative addition to the growing literature on the politics of the Internet. In addition to its rich intellectual texture and mother-lode of information about computer hard- and software, it is a quick read because the author has a sharp tongue and makes excellent points. It is a unique blend of political philosophy, political economic theory, and computer network technology in support of a political F-5 tornado warning.

*Prometheus Wired* is aimed directly at the heart of naiveté about the Internet and computer networking that characterizes much teaching and writing in fields such as communications, futuristics, journalism, and information science as well as media and politics courses in our own discipline. Barney's explicit goal is to debunk those who have been preaching about the innately prodemocratic, nonhierarchical, chaotic infrastructure of the Internet. He contends that the new computer networks are producing greater alienation among workers and greater mastery over citizens.

One strength of the book is Barney's clever, cogent, coherent, and compelling critique of the emerging global economy and how it deploys the “control utility of network technology” to produce “a hegemonic economic order” (p. 187) or the “universal homogeneous state” (p. 262). The result of this new information and communication technology, or what the Europeans call ICT, has not been to free and empower ordinary people but to tighten the screws and make their global economic and political rulers richer and less visible than ever before.

Barney points to all the usual indicators of what others call the new world order: diminishing union strength; proliferating electronic surveillance of workers and citizens; longer hours on the job; the isolating properties of telework and telecommuting; the downward pressure on wages and decline of employee benefits while profits and the value of stocks soar; greater polarity in wealth; the massive intrusion into personal privacy; and so on. “Insofar as they bolster the already formidable control of capital over the means of power, computer networks are an essentially conservative, not revolutionary, technology—conservative, that is, of the prevailing liberal and capitalist order” (p. 188).

As for democracy, Barney calls the Internet its Trojan horse. What appears to be a Promethean-like gift is in reality a most insidious weapon of destruction. In his view, if citizens do not enjoy much power in capitalist and quasicapitalist societies, then how can they accrue more when the networking technologies are even more alienating, demeaning, and manipulative of the blue- and white-collar working class? Another of his arguments is that “network technologies can

never be an adequate substitute for the *techné* of government, and will never fully satisfy the appetite human beings have for governing themselves well” (p. 267). Indeed, “if computer networks are to be involved in democracy at all, they are likely to be instruments of democracy at its worst” (p. 267).

Barney's treatise is a highly readable polemic that deconstructs the present system and, as is the wont of this analytic ilk, leaves the reader feeling that the future of democratic politics is somewhere between grim and hopeless. One could well finish this book feeling that all is probably lost, largely because of the “control utility” of computer networking, the ultimate capitalist tool. The brave new cyber world not only is coming to town but also is inside your PC, cell phone, wallet, and psyche.

Does *Prometheus Wired* provide conclusive evidence that those who laud the democratic potential of the Internet have been deluded? Not really. Despite the persuasiveness of Barney's argument, a good case can still be made for another scenario, that the Internet will yet prove to be a key factor in strengthening democracy in the immediate, proximate, and/or distant future. As a good advocate, Barney is aware of some data such analysts would proffer. He describes several cyber democracy experiments, although sparingly, and then cavalierly dismisses them; for example: “The limited role network technology *might* play in community enhancement overrides the generally uprooting quality of its essence” (p. 218, emphasis in original).

What could proponents of a counterthesis offer to support their position? First, they would probably claim that the tremendous capacity of computer information and networking technology to help develop both geographic and “virtual” communities is only a small part of the picture. There is already a massive trove of experimentation and literature on how other new technologies and techniques have empowered citizens in innovative ways, such as scientific deliberative polling (the deliberative poll, citizens juries, consensus panels; authentic electronic town meetings; the growth in and cyber networking of citizen initiatives in America and national referenda throughout Europe and other parts of the world). Computers sometimes play a leading role in this ensemble but are rarely the superstar and may never be.

A second major line of argument concerns the history of democracy globally and in America. There is something to be said for the view that democracy is part of humanity's genome and that as human beings have evolved, as our knowledge has grown, so has democracy. There is abundant evidence that the process of democratic transformation is not linear but surges and ebbs and may be nowhere near high tide at this time.

Proponents of this position are equally capable of peering into the future and asking: What happens to all this global cyber control when the universal homogeneous state falters or implodes because of some American, European, or global political, economic, biological, environmental, and/or technological disaster(s)? Mass movements preceding democratic surges take a long time to gestate but thrive under catastrophic conditions. There is merit to the position that increasingly alarming evidence of extreme global warming raises reasonable doubt about the viability and invincibility of this global economy, with all its “control utility mechanisms.”

Huxley, Orwell, and Barney have every right to fret over and forewarn about the variety of dictatorial futures human-kind faces due to current and emerging electronic and drug technologies of control, manipulation, and repression. Yet, no one really knows our destiny. The counterposition to Barney's has equal credibility: When political, economic, and social crises reach a turning point, at least the American

solution has been to transform the democratic process, not deform it into tyranny. All the pioneering work by political scientists and democratic activists around the world to engage and empower citizens through new and improved methods of informed deliberation—electronic and face to face—may well come into play when the time is right. Posterity alone will be the judge.

Finally, Barney and I interpret the myth of Prometheus differently. I view his gift as a net plus for humanity's growth and self-actualization. True, Zeus fumed, and Prometheus was severely punished. But his gift of fire helped lure the human life form out of darkness so as to transform all other earthly elements, if not the universe itself, not as self-proclaimed gods but as seekers and doers. In their own way, current and future networking technologies may yet prove to be as enlightening and liberating as fire was to primitive humanity. To believe otherwise will only benefit those Barney fears the most.

**Virtue and the Making of Modern Liberalism.** By Peter Berkowitz. Princeton, NJ: Princeton University Press, 1999. 235p. \$27.95.

Kenneth L. Grasso, *Southwest Texas State University*

"For quite a while," Peter Berkowitz notes, "leading academic liberals and their best-known critics formed an unwitting alliance, promulgating the view that liberal political theory" ignores the whole subject of virtue and cultivation (p. 170). If that view is correct, this neglect not only would spawn "fatal theoretical lacunae" (p. 4) but also would raise serious doubts about liberalism's capacity to sustain the "qualities of mind and character" (p. 172) required for "the operation and maintenance" of "free and democratic institutions" (p. 6).

In recent years, however, a new generation of liberals have challenged this widely held view. Thinkers such as William Galston and Stephen Macedo acknowledge that liberal regimes depend "upon a specific set of virtues," which "they do not automatically produce" (pp. 27–8). Their work points toward the "dependence" of liberal societies on "extraliberal and nongovernmental sources of virtue" (p. 28), such as "the family, religion and the array of associations in civil society" (p. 6). Simultaneously, they insist that "limited government is not the same as neutral government" (p. 173), and they affirm "that the liberal state, within bounds, ought to pursue liberal purposes" and, thus, "may, within limits, foster virtues" that serve these purposes (p. xii).

Although impressed by their work, Berkowitz believes that these thinkers have not "taken full advantage of the resources within the liberal tradition for illuminating the connections between virtue" and liberal politics. Likewise, they have not fully appreciated either "the disproportion between liberalism's need for virtue and the means" it "can muster to foster" it or "the vices that liberal principles can engender" (pp. 31–2).

It is these subjects that this volume seeks to address. Its "core" (p. xiii) is historical and consists of an examination of the thought of Hobbes, Locke, Kant, and Mill. Each, Berkowitz admits, adduced "strong practical and theoretical reasons for . . . circumscribing virtue's role" in political life (p. 3). Nevertheless, a reading of their work that attends to "text and context" (p. 171) demonstrates that they may disagree on the specific qualities needed and how these are to be cultivated, but all ultimately recognize that liberal regimes cannot flourish without citizens and statesmen "capable of exercising a range of basic virtues" (p. 32). At the same time, each faces an array of "practical and theoretical obstacles" in

affording "virtue the breathing room it needs to perform its function well" (p. 32).

Berkowitz realizes that this claim may seem implausible, given virtue's close association with an idea about which liberalism harbors profound reservations, namely, that human nature has "a single perfection" (p. 8) or "ultimate end" (p. 71). Virtue, however, can be understood instrumentally—and hence independently of this idea—as designating various "functional" excellences. It can be understood, in other words, as designating various "qualities of mind and character" (p. 172) necessary to the effective performance of the various roles (e.g., citizen) human beings may find themselves playing. There is thus no necessary incompatibility between liberal principles and the concept of virtue.

Yet, the work of these thinkers illuminates not merely the important but largely unacknowledged role virtue has played historically in liberal thought; it also illuminates the "genuine difficulties" of liberalism "giving virtue its due" (p. xii). To begin with, "liberal ideas about human nature, metaphysical first principles and the [human] good shift attention away from" the "moral and intellectual virtues" constitutive of human excellence, whereas liberal ideas about politics "concentrate attention on the restraint of government from legislating morals." Liberal principles thus "set in motion a conceptual dynamic that all too easily induces silence about virtue and encourages indifference to its cultivation" (pp. 171–2). Simultaneously, liberalism's recognition of its need for virtue has always coexisted with "a certain ambivalence in regard to virtue" (p. xii), an ambivalence that derives from virtue's association with ideas liberalism rejects and practices it deplores, and with the fear that the claims of virtue represent a threat to individual freedom and limited government.

Moreover, insofar as "liberalism depends on virtue that it does not readily summon and which it may . . . even stifle" (p. xiii), liberal societies experience grave difficulty in cultivating the qualities they need. Their commitment to limited government sharply circumscribes the ability of the state to protect and promote the virtues on which it depends. Simultaneously, taking for granted the extraliberal nongovernmental "institutions, practices, and beliefs" (p. 23) on which it has historically relied to foster the qualities liberal societies need, liberalism generally fails to appreciate how "the very actualization of liberal principles" can "erode" (p. 174) liberalism's "extraliberal and nongovernmental foundations" (p. 177).

In common with other regimes, liberal orders tend "to form citizens with an immoderate enthusiasm for its guiding principles" (p. 177). This enthusiasm acts to undermine the extraliberal institutions (e.g., the family) on which liberal societies have historically relied to transmit the qualities of mind and character on which they depend. Liberal orders also tend to produce "a dialectic of extravagance and neglect," in which "a favored [liberal] principle" is "carried to [such] an extreme" that other principles and considerations are ignored (p. 179). (Feminism, deliberative democracy, and postmodernism, Berkowitz contends, all exemplify this temptation.) This dialectic acts to obscure both the preconditions of such regimes and the "contending principles" and "practical necessities" (p. 183) that led "liberalism's founding fathers" (p. 33) to "limit the scope and application" of various liberal principles (p. 183).

Virtue thus poses a predicament for liberalism that has no easy or permanent solution. This does not mean, Berkowitz insists, that liberalism should be abandoned. Rather, we need to cultivate a "chastened" (p. 23) and "self-critical" (p. 29) liberalism, one that appreciates the need to maintain "a delicate balance" among the various principles affirmed by

liberalism and “the practical necessities” that require limiting their “scope and application” (p. 183). By taking more responsibility for fostering “the basic qualities” of mind and character “essential to the flourishing of liberal societies” (p. 184), such a view would recognize that “making liberalism work today requires either the renewal” of the “old sources” on which it has traditionally relied to foster the virtues it needs “or the creation of new ones” (p. xiii). It would also be able to distinguish between “indirect and relatively unobtrusive [governmental] measures for fostering these qualities” and “invasive laws and regulations that foist on citizens state-sanctioned conceptions of human perfection” (p. 191).

A thoughtful study, this volume casts light on both a major issue confronting contemporary liberal theory and a neglected aspect of the work of some of the seminal figures in the liberal tradition. By bringing into sharp focus one of the enduring problems of liberal thought, Berkowitz makes a significant contribution to contemporary liberal theory. One wishes, however, that he had pursued the reasons liberalism finds it difficult to give “virtue its due” in a more systematic manner. One particularly wishes he had pursued two subjects on which he only touches: the nature of liberalism as a distinctive intellectual tradition and why “the internal dynamics” of “liberal thought” operate to erode its “capacity” to address the whole subject of virtue (p. 19).

Berkowitz recognizes that “a complete understanding of virtue” involves “a defense of controversial opinions about human nature and the cosmos” (pp. 13, 171) and admits that “to understand virtue’s embattled position today” one must appreciate how “the philosophical ideas that partially constitute liberal, Enlightenment modernity” seem “to remove the ground from underneath” it (p. 14). Nevertheless, he seems content to treat liberalism simply as “a political doctrine” (p. 4) and never really pursues either the philosophical ideas that inform the liberal tradition or their bearing on the evolution of liberal political theory. The failure to do so prevents him from seriously considering the possibility that liberalism’s difficulty in “giving virtue its due” stems from these very ideas, the possibility that the understandings of man, the good, and the cosmos on which virtue rests are simply unsustainable in the light of liberalism’s metaphysical first principles. It prevents him, in short, from seriously entertaining the possibility that liberalism’s own metaphysical premises preclude both the theory of virtue that liberal political theory needs and the moral affirmations and politico-cultural commitments that are necessary to sustain free and democratic societies.

**The Environment: Between Theory and Practice.** By Avner de-Shalit. Oxford: Oxford University Press, 2000. 238p. \$60.00 cloth, \$19.95 paper.

John S. Dryzek, *Australian National University*

Although there is a lot of it about, environmental philosophy has so far had little influence on the world of environmental politics and policy, avers de-Shalit in this fine contribution to ecological political theory. In this assessment he is quite right. As someone who believes there should be a role in public discourse for the philosopher, de-Shalit seeks to provide a remedy for this deficiency. Why should the rest of us care? Because, he argues, the legitimacy of a policy depends on its rationale, which in turn is a matter of “the degree of moral persuasion and conviction it generates within the public arena” (p. 5). Who better to develop warrants for persuasion and conviction than the philosopher? In a democratic society of the sort de-Shalit favors, the philosopher is not just a

participant in public deliberations but “leads the discourse, in the sense that she is committed to certain standards” (p. 35).

To de-Shalit, the problem is that environmental philosophers have not measured up to the task. They are guilty on two counts. First, most of them are keen to establish a biocentric ethic that locates intrinsic value in nature, irrespective of human interests. So, when it comes to animal rights, for example, activists care about stopping cruelty, whereas philosophers worry about the moral status of animals. More generally, de-Shalit believes that the biocentric or ecocentric attitude is unlikely to get very far in a world in which most people are more concerned about human suffering, be it from environmental harms or the host of other ills that afflict us. Thus, animal rights will seem absurd so long as human rights are subject to gross violation (p. 20). This claim is not quite true. Biocentric legislation, notably the U.S. Endangered Species Act, protects species irrespective of any human interest in them.

The second count on which environmental philosophers are found guilty is their use of environmental issues to pursue a political agenda that is not at all about nature’s intrinsic value. Deep ecology, the most prominent biocentric doctrine, turns out not to be a moral theory for the environment but a program of psychological self-realization that uses environmental consciousness as an instrument to this end (p. 48). Ecofeminism is no better, as it seeks to use the environment in instrumental terms to further its essentially feminist agenda, which is all about relationships of humans with one another, not with the environment. De-Shalit is perhaps a little unfair to deep ecology. On a more charitable interpretation, the material that key texts in this area contain about the need for self-realization through deep ecological consciousness could itself be seen as merely instrumental to respect for nature’s intrinsic value.

De-Shalit is right, however, about the implicitly antipolitical character of much ecological philosophy; as a democrat, he wants its claims to be tested in a broader (human) public arena. Such testing goes straight to the heart of how environmental philosophy, or any moral philosophy with political intent, must proceed. De-Shalit advocates a method of “public reflective equilibrium,” whereby the philosopher tests his or her theories against the intuitions and reactions of actual participants in political dialogue. This is contrasted with the “private reflective equilibrium” of John Rawls and the “contextual reflective equilibrium” of Michael Walzer, neither of which involves any dialogue. Clearly, de-Shalit offers a philosophical method much more appropriate to a democratic society than that of these two alternatives.

De-Shalit’s method fits in nicely with his own environmental political theory. He argues for a democracy that is both participatory and deliberative as well as (for good measure) socialist. Liberalism is found wanting because environmental affairs call for “a politics of the common and the good, and consequently for interventionism” (p. 92). In the environmental area no less than elsewhere, liberalism is perhaps a bit more slippery than de-Shalit allows, although his critique of individualistic liberalism is absolutely on target. Community fares better, but not the historically given version beloved of most communitarians. Instead, it must be “community as collective rational reflection” (p. 108), which provides a nice field for the public reflective equilibrium sought by the philosopher. One might question whether this is really community, but it is democracy, and here de-Shalit is on strong ground. A deliberative and participatory democracy would, he believes, force governments both to disclose environmental risks and to incorporate a broader variety of values in collective decisions (provided, of course, that participants

have developed sufficient environmental consciousness). It also would prevent environmental critique from being converted into financial resource politics, to be addressed by directing government expenditure in the appropriate direction. In the end de-Shalit has great faith in what citizens (as opposed to consumers) will want and seek in environmental affairs.

A noteworthy omission in the book is any sustained attention to the idea of environmental justice, as both a political movement and a political theory. This omission is surprising in light of de-Shalit's comment that "we cannot turn a blind eye to injustice to humans when discussing environmental matters" (p. 215). Overall, however, de-Shalit offers a refreshing and well-argued political alternative to environmental philosophy as guidance for public affairs, one that he hopes will also be received by "both activists and members of the general public" (p. 214). The big question remains: Is anyone listening?

**Public Integrity.** By J. Patrick Dobel. Baltimore, MD, and London: Johns Hopkins University Press, 1999. 260p. \$38.00.

Andrew Stark, *University of Toronto*

Integrity is a shifty, furtive concept. Philosophers have had a hard time defining the idea because it raises a couple of recurrent perplexities. First, consider former Speaker Jim Wright's remark that "integrity is . . . the state or quality of being complete, undivided, [and] unbroken," or the *Oxford English Dictionary* connotation of an "unbroken state" of "material wholeness." The problem is that integrity, so understood, seems to leave no room for the possibility of individuals whose lives display any kind of self-critical revision, changes in course, or discontinuities over historical time, or for those who compartmentalize, differentiate, and assume conflicting roles across social space; in other words, for all of us. We need, as Amelie Rorty has written ("Integrity: Political, not Psychological," in Alan Montefiore and David Vines, eds., *Integrity in the Public and Private Domains*, 1999), a far better account as to how and where "integration and integrity . . . coincide".

Second, even assuming integrity is simply a matter of straightforward wholeness and integration, the stock criticism is that we would have to consider people who pursue a career of larceny as having integrity, as long as they do so in a whole-hearted and integrated way. Thus, integrity also must connote (to quote the *OED*) "soundness of moral principle; the character of uncorrupted virtue." As Speaker Wright once said, integrity is also "a matter of . . . moral behavior." But this is where the notion of public integrity becomes problematic. Public life is a realm of heightened pluralism—we differ deeply as to what constitutes "moral principle" or "moral behavior" in the ends or goals of policy—and of widespread pragmatism—actors whose means or instruments consist only of "uncorrupted virtue" risk getting nowhere. It appears as if we cannot bestow the title "person of integrity" on anyone who holds (major) public office. Perhaps it is because we are so deeply uncertain as to when someone qualifies for the term that integrity, unlike honesty, sincerity, or authenticity, has no adjectival form.

Although J. Patrick Dobel's book is not explicitly organized to do so, it sheds considerable light on each of these questions. Referring as needed to philosophy, fiction, the real world, and his own hypotheticals, Dobel performs a kind of balancing act. He never once glosses over the paradoxes, Catch-22s, grey areas, and hard cases that abound in his

topic, but he manages throughout to offer illumination and insight.

Consider the inconsistencies—shifts, reversals, broken commitments—an official's career can display over time while still remaining one of integrity. On the one hand, as Dobel notes, integrity seems to vanish if we believe that "bureaucratic interest [or] group conformity pressures" (p. 93), "giving in to interest groups" (p. 135), or other "conflicting social influences" (p. 26) have driven an official off the course of her commitments. On the other hand, integrity seems more likely to survive if "any [such] changes are reflective and self-conscious" (p. 118), if they flow from evolutions in a "person's character over time" (p. 184), or if the officials concerned have "train[ed] themselves over time to approach problems in different ways, to judge according to different standards, and to choose in new ways" (pp. 195–6). All this seems to suggest, unexceptionably enough, that inconsistencies in personal history do not jeopardize integrity to the extent that they are internally generated, through character, reflection, or self-consciousness, rather than externally imposed, through interests, pressures, or influences.

Yet, as Dobel shows, quite the opposite is the case when the inconsistencies are arrayed not over personal history, the way we operate within a given role over time, but "social geography" (p. 178), the way we move between different roles at any one time. Here, integrity survives precisely to the extent that we believe any such inconsistencies are externally forced, associated with the different roles themselves. We all understand that diverse roles mutually "entail discontinuity and tensions" (p. 179). But once those inconsistencies move from the domain of "socialized power into one's psychological world" (p. 172), disrupting "stable cognitive and emotional responses" and "internalized patterns of behavior" (p. 196)—in other words, once the discontinuities cease being a feature of the official's differentiated external world and become instead characteristic of a schizoid internal world—we begin to fear for the official's integrity, for her inner ability to connect.

Integrity, then, survives to the extent that an official's psychological gyroscope, the instrument that directs his swerving and careering over time, remains under internal, not external, control. And it persists as long as the social kaleidoscope, the array of shifting and colliding roles faced at any given time, remains an artifact of the official's external world, not part of the internal lenses through which he views that world. What of the problems that pluralism and pragmatism pose for official integrity? As Dobel notes in considering the issue of pluralism, an ethics of integrity cannot ignore "the broader issue of the right," but any moral theory that argues integrity "serves only good ends" does not "capture the complexities and ironies of life" (p. 217), since people hold "various conceptions of the public good" (p. 96). So, Dobel suggests, if we are to associate integrity with an official who supports a policy we believe is wrong, then we must at least be satisfied that his actions flow not from self-interest or self-deception, but from "deliberation and careful judgment" (p. 198) or "moral reasons [such as] internal dissent" (p. 113). Dobel evocatively contrasts Robert McNamara, who by upholding the Vietnam war to the outside world gained sufficient credibility with Lyndon Johnson to challenge its conduct internally, and Henry Kissinger, who cultivated an external reputation as someone troubled by the war, which required him to "assert his own toughness" internally so as not to lose Richard Nixon's confidence (p. 107).

What of pragmatism, the everyday possibility that officials may have to use the wrong means, never mind pursue the wrong ends? Here, Dobel argues, John Le Carré's character

George Smiley “represents the best hope of learning how to live with the moral ambiguity of flawed means” (p. 87). Smiley dislikes violence and even coercion, but he is not someone who, coddling his integrity, believes that his “uprightness” alone will “ensure his position” in any conflict (p. 202). In other words, he is not above getting others to abet a course he believes is right for reasons he himself would never abide, even by appealing to their weaknesses for alcohol, affection, or anonymity. More generally, Dobel writes, to “garner . . . consent among self-interested agents” (p. 197), an official must sometimes appeal to those interests, even though he does not share them.

Dobel suggests that if an official of integrity pursues what we think is a wrong course of action, she will at least have done so for what we accept as the right reasons, not as a result of self-interest but after reasoned consideration of principles conducted in deliberation with others. We expect an official of integrity to avoid wrongful means, but we will at least give her scope to accomplish her goal—to induce others to follow what she thinks is the right course of action—by sometimes relying on what she sees as the wrong reasons: by appealing to the sometimes squalid interests of others, instead of relying pristinely on the persuasive power of her own views and principles.

In Dobel’s book the outlines of public integrity come into view. We see the extent to which integrity remains compatible with inconsistencies over personal history and social geography, the ways in which it can abide both pluralism and pragmatism. Of course, questions remain. What does it mean, is it even possible, to ensure that one’s inconsistencies over time are generated internally, not externally, while those over social space remain externally structured, not internally assimilated? How, in hard cases, do we distinguish between the calculations of interest and the deliberation over principles that help define integrity in a world of pluralism and pragmatism? It is testimony to the success of this fine work that Dobel now directs the study of integrity toward these questions.

**Thomas Jefferson and the Politics of Nature.** Edited by Thomas S. Engeman. Notre Dame, IN: University of Notre Dame Press, 2000. 232p. \$17.00 paper.

Ralph Ketcham, *Maxwell School, Syracuse University*

This volume offers, as its blurb asserts, “substantive discussions of the key issues facing Jeffersonian scholars.” Beginning with Michael Zuckert’s by now familiar argument that Jefferson’s thought is best understood as resting in a Lockean natural rights framework, other able scholars more or less take issue with this analysis by appealing both to Jefferson’s own writings and to the works of major interpreters of the political thought of the founding era. The result is a serious reconsideration of Jefferson’s thought that takes up most of the key themes raised by Louis Hartz and Bernard Bailyn forty or more years ago over the place of the liberal tradition in American thought. Veteran expositors of Jefferson’s thought in a more civic republican and Christian way, Jean Yarbrough and Garrett Ward Sheldon, take issue with Zuckert’s Lockean, natural rights emphasis, upholding instead the influences of Aristotelian, Kamesian, and Christian thought on Jefferson. Though one cannot deny the strong Lockean strand in Jefferson’s thought, Yarbrough and Sheldon argue persuasively for the strong presence of the other dimensions as well. Zuckert’s effort in his “Response” to claim that this mixes without resolving conflicting philosophies, and thus, if Yarbrough and Sheldon are right, leaves Jefferson hopelessly

inconsistent, misses Jefferson’s brilliant blending of these outlooks, all obviously present in his writings, into what might be called a Jeffersonian republicanism.

Thoughtful articles by James Ceaser and Joyce Appleby take up important, more limited themes. Ceaser explains the danger, widely by-passed in Enlightenment thought, of projecting from nature as physical science to nature as authority in political and ethical matters. Thus, he details the unfortunate consequences of Jefferson’s flawed anthropology (supposedly “scientific”) of African racial inferiority in preventing him from moving against the institution of slavery, despite his earnest opposition to it on natural rights grounds. Joyce Appleby insists, as she has in other seminal works on founding era thought, that Jefferson’s agrarian bias was neither backward-looking nor anticapitalist. Rather, he advocated (and practiced) an agriculture that depended on the future growth of population (and hence markets) at home and abroad, and on a vigorous international trade in the Atlantic world, in which farmers would participate through growing commercial towns from Boston to Charleston. In particular, Appleby points out that the thirty-year high demand for American grains, from about 1788 to 1818, driven by both the Napoleonic wars and the inability of European farmers to increase production, was the foundation of good times for farmers (including Virginians who switched from tobacco to grains), and the material base for Jefferson’s agrarian ideology. Although Appleby understands the moral and political grounds as well, she too little emphasizes how those grounds were in fact a centerpiece of Jeffersonian thought: Those grain farmers would likely be independent, responsible citizens of the republic. Articles by Robert Dawidoff and Robert Fowler, on Jeffersonian “Rhetoric” and “Mythologies,” though elegantly written, too much simply quarrel with other scholars and too little get beyond tropes of literary criticism to reach Jefferson himself.

Zuckert’s lead essay on “Founder of the Natural Rights Republic,” though, sets the basic theme for the articles: Jefferson’s thought points consistently toward Lockean, rights-protecting self-government. Zuckert’s intention, furthermore, James Ceaser asserts, “is nothing less than to revive the doctrine of natural rights as a ground for modern American political life” (p. 165). This places Zuckert in the large camp of contemporary, liberal theorists who see the protection of the rights of autonomous citizens as the chief function of government. Another large group of contemporary theorists, though, designated civic republicans, communitarians, and so forth, harking to Aristotle and classical understandings of government, see this as not so much wrong as “thin,” or “minimal,” not in any way fulfilling the rich potential for government, responding to active, deliberative citizens, to further not life merely, but the good life. In Zuckert’s summary, “Lockean liberalism affirms natural individualism and self-interestedness, the artificiality of the political community, and the ultimate ordering of government to the securing of self-centered rights.” Classical republican theory, on the other hand, “builds on natural human sociability, the moral sense, a rejection of egoism, and an understanding of the human good as the natural fulfillment of human beings in political participation” (p. 197). Zuckert insists that these are incompatible differences, or conflicts, and that Jefferson’s thought “at bottom . . . is a version of Lockean liberalism” (p. 197). As Yarbrough and Sheldon especially point out, however, there is in Jefferson’s writings at least as much sympathy for the classical republican guidelines, as for the Lockean liberal ones. Though modern analysis of political ideas tends to find Locke and Aristotle in opposing camps, one starting with individual rights and the

other with a deliberative political community, somehow Jefferson did not get it. He combined the perspectives in the Declaration of Independence, his First Inaugural Address, and numerous other documents and letters. Near the end of his life he declared explicitly that in the Declaration of Independence, seeking to express “common sense” and “the harmonizing sentiments of the day,” he set forth ideas found “in the elementary books of public right, as Aristotle, Cicero, Locke, Sidney, etc.” (pp. 15, 73; quoting Jefferson to Henry Lee, May 8, 1825). More attention to Jefferson’s writings themselves, and less applying of presentist labels and less bickering among the authors, might have produced an even more interesting and enlightening book of essays.

**Hegel’s Philosophy of Freedom.** By Paul Franco. New Haven, CT: Yale University Press, 1999. 391p. \$35.00.

**Hegel’s Idea of Freedom.** By Alan Patten. Oxford: Oxford University Press, 1999. 216p. \$45.00.

Peter J. Steinberger, *Reed College*

In a sense, these two books, bearing almost identical titles, could not be more different. Patten’s work is a narrowly focused study of those passages in Hegel (primarily in the *Philosophy of Right*) that deal explicitly and pointedly with the idea of freedom. He proposes a “civic humanist” interpretation of Hegelian freedom. Such an interpretation is designed to make sense of what Patten calls the “*Sittlichkeit* thesis,” according to which ethical norms are composed of, or otherwise reducible to, duties and virtues embodied in the central institutions of modern social life. Franco’s work is much broader in scope. It offers a commentary on the entirety of the *Philosophy of Right* and argues, plausibly enough, that Hegel’s political philosophy is fundamentally a philosophy of freedom. It briefly situates that philosophy in the context of Hegel’s immediate predecessors (Rousseau, Kant, Fichte) and reviews Hegel’s own intellectual development, but its main goal is to show how Hegelian arguments about abstract right, morality, and ethical life constitute an account of what it means to be free.

In another sense, though, the two books are surprisingly similar. Each purports to offer an interpretation of Hegel’s political thought that takes seriously the larger philosophical or metaphysical claims in which that thought is embedded. Patten’s civic humanist view explicitly presupposes the idea that “Hegel is committed to giving a rational warrant for existing institutions and practices through philosophical reflection” (p. 41), while Franco agrees that Hegel’s political philosophy cannot be separated from his “speculative logic insofar as that latter articulates and justifies his views on philosophical method” (p. 140). Evidently, this is now the dominant trend in Hegel studies and is much to be applauded. Nothing could be clearer than that Hegel understood his political thought in general and the philosophy of right in particular to be part of a larger philosophical system. Without an understanding of the system, the political claims are unintelligible, in principle and in fact. Of course, all of this presents special problems for interpreters of Hegel, and the degree to which Patten and Franco deal successfully with such problems is an interesting question.

Patten distinguishes his civic humanist view from a “metaphysical view,” which he associates primarily with Charles Taylor, and also from a “historicist view,” which he associates mainly with Robert Pippin. He is painfully equivocal on the nature of the difference, however. He readily concedes that the three views are not necessarily incompatible and, indeed, may be thought of as “converging perspectives” (pp. 40–1).

In this context, the utility of identifying a distinct civic humanist view remains unclear. Presumably, Patten wants to say that his account does a better job of showing how Hegel identifies freedom with “self-actualization” and with the idea of “participation in community with others” (p. 38). But this seems to me less a departure than a gloss on much of what Taylor and Pippin (and many others) have already said; if anything, it runs the risk of identifying Hegel’s political thought with a much more conventional, and philosophically much different, tradition of civic republicanism.

In pursuing the idea of self-actualization, much of what Patten says makes good sense. For example, he shows that freedom, as Hegel understood it, is not so much a rejection of desire per se but a matter of acting from desires or feelings that are “reasonable or appropriate in the circumstances”—for example, a husband’s feeling of love for his wife (p. 63). One’s desires are or ought to be self-legislated, so to speak. The point is an important one, and Patten makes it well, although it has also been made elsewhere and at greater length, something Patten fails to acknowledge.

At the same time, his interpretation, even as it seeks to approach Hegel on his own terms, seems to back away from the full force of the larger Hegelian system. Hegel’s ethical thought (like Kant’s) presents a seeming paradox: If the free agent is not primarily motivated by authority or desire, then what other motivation could there be? Patten’s answer is that the agent “has reason to establish and maintain his own freedom and independence” (pp. 102–3). On the one hand, such an answer seems to be circular and question-begging. Why is the agent interested in maintaining freedom and independence? On the other hand, and even more importantly, it seems to ignore some of the boldest yet most characteristic claims of Hegel’s philosophy, namely, that rational inquiry necessarily produces a determinate content, and that the freedom of the individual qua rational agent is a matter of thinking and living in accordance with that content.

I find similar equivocations or hesitations in Franco’s book. Sometimes this is a matter of failing to explain fully what needs to be explained. In working through Hegel’s concept of the will, for example, Franco concludes that the idea of freedom is the idea of “being with oneself in an other” (p. 162). But exactly what does that mean? After five pages of background, Franco devotes barely a single page to the idea itself and offers little more than a series of quotations from Hegel’s text without substantial explication.

Elsewhere, the problems seem to me rather more technical. For example, Franco says that Hegel identifies ethical life with “an unreflective and habitual ethical disposition” (p. 228), and criticizes certain authors (including me) for over-emphasizing the reflective and self-conscious rationality of Hegelian citizens. But while he dutifully quotes an appropriate passage from Section 151 (Addition) of the *Philosophy of Right*, he pointedly fails to mention a crucial part of the very same section in which Hegel explicitly says that “habit [*Gewohnheit*] is part of ethical life just as it is also part of philosophical thought.” Presumably, philosophers take for granted, say, the law of the excluded middle or the other basic principles of logic, all of which are invoked more or less habitually. Does this mean that philosophers are unaware of or do not rationally understand those principles? If Hegel’s analogy of the philosopher means anything, surely it suggests that citizens are more rational—more capable of self-consciousness and self-reflection—than Franco would have us believe. (On this general question, I think Patten [p. 75] makes much the stronger case.)

More generally, I believe that Franco’s work reflects the same kinds of hesitations that one finds in Patten’s. This

involves, again, a certain unwillingness to accept fully the claim that rationality involves, for Hegel, a determinate and necessary content of some kind. Franco acknowledges that Hegel identifies freedom with rational necessity, but he denies that this necessity is a kind of “fact that first exists outside of human freedom and only later comes to lose its alien character by being understood” (p. 181). I don’t see a clear argument for such a denial. Franco insists that “the rational necessity of right . . . is itself derived from freedom, is produced by the logic of freedom, consists in the immanent development of freedom.” This seems to me quite right, but it also seems to me entirely consistent with the idea that freedom unfolds according to an internal, objective logic of some kind and that the process of unfolding results both in the fulfillment of human freedom and in the attainment of substantial truth.

I have mentioned here only a few of my criticisms—of Patten and Franco alike. I do not regard these criticisms, moreover, as mere quibbles; they speak to the heart of Hegel’s idea of freedom and reflect very serious disagreements. On the other hand, disagreements of this kind are only to be expected, and I might very well be wrong on all counts. The larger point is that these two books, in their different ways, are to be taken seriously as sober, informed, and intelligent efforts to make sense of their subject matter. I would not hesitate to recommend them, both as very useful introductions to Hegel’s political thought and as plausible contributions to a series of important and on-going scholarly conversations.

**Marxism, Revisionism, and Leninism: Explication, Assessment, and Commentary.** By Richard F. Hamilton. Westport, CT: Greenwood, 2000. 288p. \$59.95.

Alex Callinicos, *University of York*

A distinguished political sociologist, Richard F. Hamilton is perhaps best known for *Who Voted for Hitler?* (1982). More recently his attention has shifted to the broader methodological issues raised by the empirical claims of sociological macrotheories. This critical study of three versions of Marxism—the original statement by Marx and Engels and two rival reformulations, by Eduard Bernstein at the end of the nineteenth century and by Lenin during World War I—is to be seen in the light of this concern.

There are, even so, two ways of taking this book. One is to accept at face value Hamilton’s statement of his aim: “to provide a statement of the principal claims of three major theories and to indicate, through a review of relevant evidence, the empirical adequacy—the validity—of those claims. The task, put simply, is to take stock, to undertake an inventory of our theoretical holdings” (p. 9). He concludes that a clearing out is needed: We should “consign the Marxian theory to intellectual history courses” (p. 208).

The other way to view the book is to treat the objectivist language in which it is written as a rhetorical disguise for a much more partisan exercise, a systematic debunking of Marxism that generally selects for the least charitable interpretation. For a number of reasons, I think this view is the more appropriate.

The book starts with a detailed description of the liberal tradition to which Marx stands as both heir and critic. This is an interesting way to approach the subject and makes a refreshing contrast with the often tedious trot through Hegel and Feuerbach on which less imaginative commentators take us when situating Marx. Yet, the aim is more than mere scene setting. Hamilton stresses liberalism’s economic achieve-

ments and laments the failure of university teachers to provide the citizens of liberal societies with the intellectual resources required to defend this tradition. Strike one against Marxism, even before the game begins.

The *trahison des clercs* is also a theme of the final chapter, which puzzles over the continuing influence of Marxism when it has been so manifestly refuted. The reasons Hamilton offers are partly (to adopt his own classification) psychological (individuals’ psychic investment in theories) and social-psychological (intellectuals’ tendency toward conformism), but they also involve sociologists’ ignorance of economic history, which leads them still to accept such theories as Weber’s Protestant Ethic thesis or Lenin’s theory of imperialism, long since abandoned by the accredited experts in economic history departments. The *parti pris* that these reflections manifest, not only toward Marxism but also toward (it has to be said, a somewhat outdated view of) sociology, is unmistakable.

Between these two chapters Hamilton subjects the principal claims of Marx, Bernstein, and Lenin to serious analysis. Yet, he fails to observe one of the elementary principles of criticism, namely, to take on the strongest version of the opponent’s theory. There are two particularly clear instances of this. First, Hamilton makes the *Communist Manifesto* the basis of his statement of Marx’s theory, but this text was written before Marx had performed the bulk of the economic research whose eventual outcome was *Capital*. In the course of this research, Marx both considerably elaborated and, in certain crucial respects, substantially altered the theory of capitalist development sketched out in the *Manifesto*. For example, Marx came to reject the “iron law of wages” developed by Ricardo and Malthus, accepted by most nineteenth-century economists as well as Marx himself in the *Manifesto*, according to which wages tend toward the bare minimum of physical subsistence. Yet, Hamilton attributes this idea to Marx, presenting its rejection as if it were a defensive adjustment to recalcitrant facts rather than a consequence of Marx’s theoretical critique of Ricardo’s basic assumptions in the 1850s. I had thought that modern Marxist scholarship had made further discussion of the so-called immiseration thesis otiose. By relapsing into outdated anti-Marxist folklore, Hamilton does neither himself nor his readers any favors.

This lapse is symptomatic of Hamilton’s failure to address properly Marx’s economic theory. The focus on the *Manifesto* means that Hamilton does not mention the theory of the tendency of the rate of profit to fall, developed between the *Grundrisse* and *Capital*, which has further damaging consequences for his appreciation of Marx’s thought. He asserts that “Marx and Engels regularly portrayed the bourgeoisie as rational, informed, and knowledgeable” (p. 61), but Marx claims that a collectively suboptimal result—a falling general rate of profit—is produced by capitalists behaving in ways that are individually rational in seeking to maximize their own profits, which suggests a view of their behavior as myopic rather than perfectly informed.

The second instance is the easy enough work Hamilton makes of Lenin’s theory of imperialism, pointing out the various respects in which it is contradicted by historical research. Once again, this is a soft target. Lenin’s pamphlet *Imperialism* is subtitled “A Popular Outline” and draws heavily on more original research, not only Hobson’s *Imperialism* but also Hilferding’s *Finance Capital*. Amazingly, Hilferding does not even appear in Hamilton’s index. Entirely ignored is the substantial, theoretically sophisticated, and empirically wide-ranging body of Marxist writing on imperialism to which not only Hilferding but also Luxem-



burg, Bukharin, Bauer, Grossman, and Preobrazhensky contributed. To judge this entire discourse by one relatively crude text is not what one would expect from a scholar of Hamilton's standing.

I do not in the slightest suggest that Marxism is beyond criticism. On the contrary, capitalism is still with us, and even if one takes a less favorable view of this system than Hamilton does, its persistence raises pressing questions that those still committed to the tradition Marx founded must address. Hamilton is well equipped to put many of these questions, as is shown by the quality of those sections in his book that compare Marxian claims with the class structure of twentieth-century societies. It is a pity that he should lapse so frequently from the standards set by scholarly discussion of Marxist theory over the past generation.

**Revolutionary Values for a New Millennium: John Adams, Adam Smith, and Social Virtue.** By John E. Hill. Lanham, Maryland: Lexington Books, 2000. 213p. \$55.00.

Peter McNamara, *Utah State University*

No one will deny John E. Hill's claim that he has written an "unabashedly didactic" book (p. xi). This is not social science, or political theory, or history as it is usually understood by those disciplines. I do not mean that as a criticism, for there is great merit in writing as a concerned citizen-scholar. Hill puts his political cards on the table. He is a self-described "moderate liberal" (p. xi) who wants universal health insurance, public funding of elections, more restraints on the corporate sector, a more progressive tax system, more spending on education, and community service programs. He also wants liberals to rethink their attitude toward morality: They need to be more forthright about the importance of morality—social virtue—for the health of the Republic. In addition, Hill does not shy away from telling us that he does not like "individualistic excess" (p. ix), Alexander Hamilton, Ronald Reagan, or the religious Right.

*Revolutionary Values* attempts to provide an intellectual grounding for this very contemporary liberal agenda and to show the continuities between it and the goals and beliefs of the founders. Hill looks to John Adams as his primary source of insight into that generation and to Adam Smith as a way to buttress and, on the issue of the role of government in the economy, to supplement Adams. Hill achieves only partial success in what must be acknowledged as a bold intellectual strategy.

Large parts of the book are devoted to setting the record straight about Smith and Adams. The latter, Hill argues, has suffered from undeserved neglect. He regards Adams as a first-rate statesman and a political thinker of the highest order. He grants that Smith is still famous, but usually for the wrong reasons. Smith is erroneously regarded as an "advocate of pure laissez-faire" (p. 150) and, furthermore, his often overlooked moral philosophy provides a serviceable moral foundation for a liberal society. Hill acknowledges that he is not the first to make either of these general arguments. What is distinctive is his identification of a strong egalitarian streak in both men, who were highly suspicious of the rich and powerful. Adams sought to restrain their influence by placing them in a separate chamber of the legislature. Smith's attack on the mercantilist economic system was clearly, at one level, an attack on entrenched economic power.

But to make the leap from this egalitarian streak in Smith and Adams to the contemporary liberal agenda Hill advocates requires that certain key features of their thought be virtually ignored. Hill does not, for example, share the reservations of Adams (and the other founders) about democracy. Similarly, Hill all but obliterates Smith's reliance on a "system of natural liberty" to resolve income distribution

issues. That is, he neglects the extent to which Smith relied on an indirect rather than a legislative approach to solving problems. In other words, it is one thing to say that Smith is not Milton Friedman, but it is very much more arguable to suggest that there are no huge differences between Smith and John Kenneth Galbraith or Robert Reich. Also, it does not profit Hill to point to an indigenous American tradition of activist government that is coeval with the Revolution. To suggest, as he does, a simple continuity between this kind of activism, which was usually directed toward promoting economic development, and the kind of social agenda Hill favors is again to blur important distinctions.

Hill is on stronger ground when he addresses the social virtues. By these he means civility, tolerance, moderation, public spirit, justice, generosity, and the like, which formed the core of what might be called the revolutionary consensus. Adams—the family man, republican citizen, and statesman—was an exemplar of these virtues. This consensus was the product of a blending of three strands of American life: the liberal, the civic republican, and the religious. Hill points out that the social virtues found support in institutions of all kinds, public and private, secular and religious. One would be hard pressed to find anything objectionable in setting up these values as some sort of ideal, but there is a significant problem in Hill's optimism that this consensus can be more or less fully revived (and updated, e.g., to include equal rights for women). The difficulty is evident in Hill's frequent lament that postrevolutionary America quickly transformed into a dynamic commercial republic with an abundance of "individualistic excess," by which he means, chiefly, speculation, avarice, and luxury, accompanied by a decline in public spirit and republican simplicity. The question that immediately arises is whether the revolutionary consensus was the product of a particular and rare set of circumstances, notably a grave and unmistakable crisis. Without another such crisis, one wonders just how likely is a revival of those revolutionary values Hill justly celebrates.

**Constituting Feminist Subjects.** By Kathi Weeks. Ithaca, NY: Cornell University Press, 1998. 196p. \$39.95 cloth, \$13.95 paper.

Kimberley Curtis, *Duke University*

Feminist theorists have struggled to develop accounts of women's oppression that are historically specific enough to capture the variability in forms of male domination but do not neglect possible transhistorical and transcultural features. A related challenge has been to theorize a feminist subjectivity that can anchor an oppositional politics but avoid an overly unified notion of feminist subjectivity, while nevertheless attending to the patterned effects that women's systematic location in the social whole may have on their subjectivity.

These challenges were the subject of sometimes polemical but mostly fruitful debates throughout the 1980s and 1990s. They have been deeply shaped by two distinct developments. The first is the political struggles and theoretical writings of women of color, whose work on subjectivity and male domination has consistently pluralized women's subjectivity as well as historicized and contextualized male domination in relation to other axes of power. The second is postmodern critiques that charge modernist modes of theorizing are governed by either ahistorical metanarratives insufficiently attentive to variation and contingency or essentialist understandings of subjectivity in either a humanist/liberal/voluntarist or determinist/Marxist form.

It is in the context of this second development, which she casts as the modernist-postmodernist paradigm debate, that Kathi Weeks situates her carefully argued study. Charging

that the either/or nature of this paradigm debate, although initially useful, has become symptomatic of stagnation, especially in relation to feminist theory, Weeks attempts to “clear a space, propose some tools, and suggest a framework” (p. 159) for theorizing feminist subjects. She searches for concepts subtle and supple enough to support a conceptual framework that attends simultaneously to the constitutive power of structural forces and to the subversive potential of feminist collective subjects. Undergirding this enterprise is a clear commitment to social transformation, and it is to a reworking of feminist standpoint theory, a version of socialist feminism, that she turns.

To do this reworking, Weeks first wages battle with the paradigm debate’s reductions, polarizing animosities, and suppression of innovation. She rightly argues (and joins a growing chorus of contemporary voices) that both modern and postmodern theorizing have rich and heterogeneous traditions, and the “repertoire of reductions” (p. 63) each side uses to disable the other serves only at this point to cripple the task of theorizing.

The first two chapters of *Constituting Feminist Subjects* pursue the question of how the selective critiques of modernist theorizing by Nietzsche and Foucault could become transformed in North American discourse into “a radical challenge to modernism itself” (p. 57). Weeks is interested here primarily in two recoveries. First, she seeks to disrupt the seamless lineage the postmodern paradigm traces between Nietzsche and Foucault because she believes Nietzsche alone constructively works with the “post-Enlightenment” dilemma of the subject, namely, of being both determined and agentic. In so doing, it is Nietzsche rather than Foucault who has the most to offer feminist theorists at this historical juncture. The pearls lie, in Weeks’s view, in the doctrine of the eternal return, read in a Deleuzian vein as a selective ethical principle.

The second recovery that preoccupies Weeks is the heterogeneity of the modern tradition itself. The hegemony of the modernist-postmodernist frame has silenced modernity’s still relevant interparadigm debate with ancient thought and, of primary interest to Weeks, the debates among Marxists, between Marxism and Enlightenment liberalism, and between Marxism and socialist feminism.

These recoveries set the stage for the work of chapter 3, where the concern is to reinvigorate and legitimize our aspiration to totality. Weeks draws selectively from Marxist social theory—Lukacs, Althusser, and Negri—to argue for the necessity and existence of theoretical tools with which to grasp the social at the level of system, but to do so by conceiving it as “a totality of social forces that is open to the possibility of antagonistic subjectivity” (p. 93).

Not until the fourth (and last real substantive) chapter does Weeks, having cleared her ground and gathered some of her tools, turn to feminist standpoint theory. This is the most original chapter in the book, and it is a shame it takes quite so long to get there. Here, too, Weeks gathers concepts with which to relate system and subject in nonessentialist ways. The first resource is the project of totality understood as “a methodological mandate to relate and connect, to situate and contextualize, to conceive the social systematically as a complex process of relationships” (p. 5). The second is the feminist standpoint theorists upon whom Weeks draws (Nancy Hartsock, Hilary Rose, and Dorothy Smith), who focus on the gendered division of labor and understand women’s laboring practices as constitutive practices, constitutive of both what women know and who they are or can become. The third resource is the concept of standpoint itself, which Weeks defines as “a collective interpretation or reworking of a particular subject position rather than an immediate perspective automatically acquired by an individual” (p. 8). Put together, and properly elaborated, these resources form “a

political project based on the alternative ways of being, desiring, and knowing that can be developed from women’s laboring practices” (p. 8).

The creative move Weeks makes in her selective appropriation of existing feminist standpoint theory is her contention that we must depart from the dominant tendency to develop the epistemological consequences of the gendered division of labor. If we are to milk its resources in the service of constructing antagonistic feminist subjects, it is to the ontological consequences of women’s laboring practices that we must turn our critical attention. Moreover, a Nietzschean critical ontology of practices is of most use. Our questions must become: What alternative pleasures, desires, habits, wills, abilities, and practices do women enact that could, selectively valorized, provide the basis for an alternative, antagonistic standpoint from which to order/value our world differently? What are the values imminent in our practices that could be affirmed, not in the sense of mere repetition but of subversive repetition that allows us to take our being beyond itself without appealing to something outside of itself?

Weeks’ Nietzschean-inflected focus on the ontological aspects of women’s laboring practices as processes of constitution and enactment breathes new life into feminist standpoint theory. Moreover, she works carefully, and her nuanced effort to bring disparate, often antagonistic traditions of theorizing into fruitful dialogue is exemplary. Yet, because of the time she spends clearing the theoretical ground for the project, the actual space and effort she devotes to giving us a rich account of feminist subjectivity in practice is disappointingly small. What is especially absent (to her credit, Weeks points this out herself) is engagement with the work of women of color that persistently draws attention to the divisions of class, race, ethnicity, and cross-national boundaries. The book really is a prolegomena: a gathering of tools and a clearing of space to prepare the way for a rich theorizing that, we hope, Weeks will take up in her next project.

Weeks does a marvelous job of identifying the deforming aspects of the modernist-postmodernist debate as she unlocks the cases that imprison her pearls. In so doing, she confronts us with the clubs we often use and with the destruction they do. That is no small contribution.

**Constitutional Interpretation: Textual Meaning, Original Intent, and Judicial Review.** By Keith E. Whittington. Lawrence: University Press of Kansas, 1999. 320p. \$39.95.

Judith A. Baer, *Texas A&M University*

With friends like Edwin Meese and Robert Bork, “jurisprudence of original intent” (p. 3) needs no enemies. These polemicists have so corrupted originalism by associating it with reactionary ideology and partisan politics that, in Keith Whittington’s words, “the task now is to convince critics to take [it] seriously again” (p. xii). *Constitutional Interpretation* ably performs this task. Whittington’s rescue of originalist jurisprudence from its strangest bedfellows in itself is a major contribution to the study of constitutional law. But, although originalism has found a genuine friend, the book’s powerful argument against “dismissing originalism as an interpretive method” (p. 162) does not constitute an affirmative defense. Whittington’s efforts to make this case are informative and provocative, but they fail. This failure is traceable to serious defects in both the structure and content of the book.

Whittington has made organizational choices that militate against clarity. He jumps from analysis and critique of arguments against originalism (chaps. 3 and 4), to his argument for originalism (chap. 5), and finally to a defense of

originalism against still more critiques (chap. 6). This approach has the virtue of inviting the reader to join the author in a process of scholarly inquiry, and Whittington makes important points along the way. If his response to postmodern, hermeneutic, and literary “theories of language and interpretation” (pp. 12–3) does not quite rescue originalism from these new and powerful enemies, it fills a gap in constitutional jurisprudence. But the organization of the book has the defect of obscuring both theme and thesis. The author would have done better to make the positive case first and follow it with critique and rebuttal. Yet, this sequence would have made the argument clearer, not necessarily better.

Whittington reminds us that “originalists have not committed themselves to a conservative policy program and could, in principle, reach results that are actually antagonistic to that program” (pp. 167–8). Anyone familiar with Hugo Black’s dissent in *Adamson v. California* or Michael Curtis’s *No State Shall Abridge* (1986) will take the point. Whittington’s effort to rescue originalism from its other familiar bedfellow—judicial self-restraint—is less successful. “Uniform passivism in the face of violations of the interpretable Constitution,” he insists, “destabilize the meaning of the text and contradict the expressed intent of the sovereign people” (p. 168). Whittington avoids the common error of conflating democracy with majoritarianism: “The Court is not simply an antidemocratic feature of American politics but is an instrument of the people for preserving the highest promise of democracy” (p. 111). Yet, after making clear that his position is not dependent on a preference for restraint, he ends the book with this ringing statement: “The discipline of originalism promises to protect the Court from itself, and, in so doing, to protect us from the Court” (p. 219).

Why do we need this protection? Why not continue to muddle through as we always have, with our motley collection of interpretive strategies? An effective argument that choice among methods is necessary must specify decisions in which the judiciary exceeded its power and mount a persuasive argument that these rulings, taken together, represent a threat to the people. Every student of constitutional law has a personal list of specific examples of judicial usurpation. The usual suspects include *Lochner v. New York*, *Roe v. Wade*, *INS v. Chadha*, *City of Boerne v. Flores*, and, of course, the New Deal cases. But several of these decisions have been reversed or neutralized—indirectly, at least, through the democratic process they allegedly usurp—and not all these cases appear on everyone’s list. Judgment on any actual case depends as much on personal politics as on constitutional theory. The challenge for the constitutionalist is to separate theory from politics by linking diverse cases into a coherent constitutional jurisprudence. Alexander Bickel’s *The Least Dangerous Branch* (1962) is a model for such scholarship, whatever one

thinks of its thesis. But the index to *Constitutional Interpretation* lists exactly eight cases.

Without establishing the existence of a problem for originalist jurisprudence to solve, Whittington has difficulty making a case for the method. Nevertheless, he makes a significant original contribution to the study of constitutional law. Whittington “seeks to ground the authority of originalist jurisprudence, and the Constitution itself, in a theory of popular sovereignty” (p. 111). As does Bruce Ackerman (*We the People*, 1991), Whittington asserts that “the people emerge at particular historical moments to deliberate on constitutional issues and to provide binding expressions of their will, which are to serve as fundamental law in the future when the sovereign is absent” (p. 135). “Democratic dualism” (pp. 135–42) distinguishes between the sovereign will, expressed in the constitutional text, and the will of government agents, who are bound by that text. Judges must adhere to original meaning because the Constitution contains binding expressions of the popular will.

Alexander Hamilton and John Marshall were right after all. Whittington argues that the founding moment was more democratic than the authors of *Federalist 78* and *Marbury v. Madison* may have desired or realized: “Popular campaigning, pamphlet and newspaper commentary, and barroom discussions demonstrated the expansiveness of the deliberative process and the inclusiveness of the popular sovereign” (p. 152). Barrooms, yes, but what about sewing circles and quilting bees? The founding moment was inclusive and expansive only if one presumes that democracy is whatever exists in the United States at any given time. To confine judges to original intent in the name of popular sovereignty might frustrate rather than enhance democracy. And spurious inclusiveness is not the only problem with the “constitutional moment” thesis, which works only when the exercise of popular will results in constitutional (re)making. The founding period qualifies, of course, and so does Reconstruction. But what about the New Deal controversy? Where are judges to go for guidance about this constitutional moment? A more basic difficulty with Whittington’s thesis is its reliance on duality and dichotomy: The sovereign will is present or absent; the people or their agents act. Yet, both constitutional interpretation and agent-principal colloquy go on continually; popular sovereignty may be more constant and less episodic than this book presumes.

*Constitutional Interpretation* is an ambitious project that does not quite accomplish all the goals the author sets. Yet, the book is well worth reading, both for its rehabilitation of originalist jurisprudence and its linkage of constitutionalism and democracy. Whittington has produced a book that demands the serious attention of scholars in constitutional law and American government.

## American Politics

**Campaign Dynamics: The Race for Governor.** By Thomas M. Carsey. Ann Arbor: University of Michigan Press, 2000. 232p. \$49.50.

Nelson C. Dometrius, *Texas Tech University*

Carsey presents a sound piece of research planted solidly in two fields: voting behavior and state politics. The primary thrust is testing a model of campaign strategy and voter

reaction. Carsey takes advantage, as have many recently, of the steadily accumulating state campaign and exit polling data. Although not nearly as rich as the American National Election Survey (ANES) in content, these data provide reliable state samples and an abundance of cases in each state, which the ANES seldom does. We now often can use the states as true social science laboratories of democracy—expanding *N* without adding the confounding factors that plague cross-time comparisons of national elections.

Carsey’s theoretical base, lucidly presented, is in the ra-

tional choice and spatial voting literature. His complaint, starting from the *American Voter* (Angus Campbell, 1960) and moving forward, is that too much research treats key electoral elements as either fixed or exogenous: There are certain voter attitudes that even the most golden-tongued candidate is unlikely to shift. Candidates thus must try to locate the issue stances of the mystical median voter, proclaim them their own, and defend that position to the death. Yet, candidates cannot shift ground freely, for they are likely both to alienate their activist base and to be disbelieved by voters. Carsey, borrowing from William Riker ("Heresthetic and Rhetoric in the Spatial Model," in James M. Enelow and Melvin J. Hinich, eds., *Advances in the Spatial Theory of Voting*, 1990), proposes an alternative view, heresthetic change: A campaign dynamic alters the nature of the issue space. Campaigns can increase the salience of those issues on which the candidate is in a favorable position vis-à-vis the electorate and ignore those on which the candidate is at a disadvantage. To control the campaign agenda—the weight voters place on an issue—is to win.

Carsey proceeds to test this model in multiple ways. The Virginia and New Jersey 1993 gubernatorial campaigns are used as case studies in chapters 5 and 6. This is followed by quantitative examinations of abortion salience in 34 gubernatorial elections in 1990; presidential approval salience in 29 elections in 1990 and 1994; and the salience of various issues in 71 gubernatorial elections between 1982 and 1992. In the quantitative analyses, an interactive term is used to test for the heightened effect of an issue if it was stressed by one of the candidates. Carsey gathered an impressive set of data and has produced a work of considerable value to scholars of both voting behavior and state politics.

My qualms stem equally from the analyses performed and from recurrent overstatement of the inferences they justify. Carsey is honest about data and analytic shortcomings, but he tends to forget his own caveats in the chapter conclusions, a failure to which we all succumb on occasion. First, the term "heresthetic change" may be distinctive, but the concept is not novel. Witness numerous articles on priming or issue salience beyond those cited by Carsey, and especially a very similar approach of combining public opinion polls with insider information about John Kennedy's 1960 campaign strategy (Lawrence Jacobs and Robert Shapiro, "Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign," *APSR* 88 [September 1994]: 527–40). There are some mild theoretical distinctions between priming and heresthetic change but not empirically distinguishable ones. Carsey does add a clear theoretical embedding in rational choice as well as a model test with multiple campaigns, both of which are sufficiently important to justify the work without padding.

Second, the theoretical dismissal of persuasion and candidate position change is too harsh. Campaigns are less about total attitude change—candidate or voter switches from pro-life to pro-choice—and are more about contemporary policies that constitute baby steps, such as battling over partial birth abortions. Simplification may be a necessary analytic strategy, because mild persuasion, candidate change, and heresthetic change would be hard to disentangle empirically, but one should be cautious about reifying an analytic simplification into a theoretical verdict.

The case studies are a critical element, for the argument requires both intentionality and consequence. Campaigns must consciously seek to highlight issues on which the candidate is well positioned, and the voters must respond by seeing that issue as particularly salient to their choice. The case studies tackle the intentionality component but not

convincingly. A true case study would test the argument by gathering data on attempts at various campaign strategies to determine whether manipulating issue salience was a primary one. As presented, the cases are illustrations, selective incidents or comments consistent with the theory; they lack any criteria or comparison that would allow the reader to assess this explanation versus others. The cases are useful, especially the polling data that show voter changes across the campaigns, but they leave the theoretical argument as only plausible, not demonstrated.

Chapter 7, on the role abortion played in 1990 gubernatorial campaigns, is effective but could be stronger. Here and in chapter 9, Carsey identifies issue salience in each campaign through an exhaustive review of newspaper coverage for each election. He convincingly argues that an article or word count would misstate an issue's importance, but he substitutes his summary judgment instead (a salient or not salient dichotomy), which is also problematic. We have no intercoder reliability information (this part stems from his solo dissertation research) and doubtful replicability. Carsey's judgments are probably reasonable, but one wonders if an issue is really salient because a news story states that "Candidate Smith today continued to stress her views on taxes" (p. 51), even if that issue is seldom mentioned in the newspaper. The assumption is that such a news story reflects recurring themes highlighted in candidate commercials, literature, and debates. This is reasonable, but I would prefer some attempt at verification.

I am puzzled by Carsey's inclusion of interactive terms for gender, Catholicism, and abortion attitudes to assess their increased effect when abortion is a salient campaign issue. Gender and Catholicism are sources of abortion attitudes in a causal sequence, and to include all three variables in a single equation equals controlling for the very effect for which you are searching. Carsey recognizes this at the end of the chapter, but a simple respecification of the model, which would have clarified things, is not done.

Chapters 8 and 9 extend the analysis by exploring the salience of presidential approval in 1990 and 1994 gubernatorial campaigns, as well as the salience of numerous issues in 71 campaigns between 1982 and 1992. In each case, assumptions about issue salience in the campaigns (chapter 8) and to various groups (chapter 9) are far more tenuous than the earlier abortion analysis. They augment the argument, but the abortion analysis remains the crucial test.

In sum, Carsey has produced a valuable work. Certain reservations about various key elements are perhaps inevitable, given the size of the task Carsey undertakes. It is good and readable scholarship and should encourage others to take up the challenge and extend the analysis.

**Supreme Court Decision Making: New Institutional Approaches.** Edited by Cornell W. Clayton and Howard Gillman. Chicago: University of Chicago Press, 1999. 344p. \$55.00 cloth, \$19.00 paper.

Michael McCann, *University of Washington*

The editors of this timely volume announce at the outset that their aim is to provide a forum for recent scholarship that reacts critically to the previous generation of behavioralists who, since the 1950s, have analyzed the U.S. Supreme Court as little more than an aggregate of the relatively stable and identifiable policy preferences held by individual justices. Specifically, these essays pose a collective "response by a succeeding generation of Supreme Court scholars who are trained in political behavioralism but who have rediscovered

the value and importance of understanding institutional contexts” (p. 12).

Instead, the volume reveals a rather more complex intellectual dialogue among three distinct approaches to an understanding of judicial decision making that are all vying for prominence today. The leading voices in the collection espouse two quite different versions of the “new institutionalism” that have become broadly recognized throughout the discipline: rational choice institutionalism, also known by its more formal title of “a positive theory of institutions” (PTI), and historical or interpretive institutionalism. The third approach is the attitudinal model, which was inherited from the earlier behavioral era but is still very much alive for its critics and the spirited advocate (Jeffrey Segal) who writes one of the volume’s chapters.

The book is organized into three general parts that follow the editors’ introductory chapter. Part 1 provides a historical and comparative theoretical grounding. Separate chapters by Cornell Clayton and Howard Gillman make the case for expansive historical interpretive study of institutional norms and forces that shape judicial decision making. A chapter by Forrest Maltzman, James F. Spriggs II, and Paul J. Wahlbeck outlines the alternative rational choice version of new institutionalist analysis. All these chapters are highly interesting because they clearly draw the lines of theoretical division, which enables readers to discern and assess the similarities and differences among the three different approaches in unusually accessible terms. From these accounts, it is apparent that both versions of new institutionalism push beyond behavioral approaches to a broader, more complex understanding of the context in which justices act. For both approaches, institutional forces that are both internal and external to the court matter. At the same time, neither approach simply echoes traditional institutionalist, and especially legal formalist, convictions. The new institutionalists are openly postrealist and postbehavioral; they accept to some degree that justices act on preferences, that justices are political policymakers, that legal rules or norms do not “determine” what justices do, and that instrumental strategic factors matter for judicial decision makers.

But the two new institutionalist approaches also are at odds with each other in important ways. Rational choice, or strategic action, analysts are much closer to the older behavioral approach. Like attitudinalists, they emphasize that judicial actors are motivated by “sincere” personal policy preferences that tend to be stable, well ordered, and exogenous to the analysis. The notable contribution of PTI theorists is the complex view that balances appreciation for preference maximization with recognition that justices “are strategic actors who take into consideration the constraints they encounter as they attempt to introduce their policy preferences into law” (p. 46). As such, institutional rules, relations, and actors shape the context of judicial choices, but they do so primarily as external instrumental forces that constrain options for practical action.

By contrast, historical or interpretive approaches emphasize the constitutive power of institutional norms and practices that enable and authorize as well as constrain justices. In this view, institutional rules and norms are not external to actors; they are internalized and expressive of judicial actors’ very understandings, aspirations, and choices. In sum, institutions shape the very identities, interests, and imaginations of policymaking justices in complex ways. The development of such insights typically results in studies that are inherently more varied, indeterminate, and complex, and hence less positivist and parsimonious, than the formal efforts of rational choice institutionalists.

The diverse chapters in the other two parts of the book tend to be more empirical, less theoretical, and rather less contentious in style. Many of the essays avoid easy identification with either institutionalist camp identified in the early essays, but all of them very well illustrate the value of attention to institutional influences on justices. Part 2 addresses specifically the “internal” institutional influences of judicial norms, processes, and leadership on justices’ decision making. David O’Brien begins by charting the changing institutional norms regarding the writing of Supreme Court opinions, and he emphasizes the increasing replacement of singular “opinions for the Court” with disparate individual opinions in the last fifty years. Charles Sheldon identifies parallel trends toward “dissensus” in state supreme courts. Sue Davis’s essay explores the many facets of leadership by the chief justice beyond opinion assignment from the perspective of both rational choice and interpretive institutionalism; she emphasizes the incongruity rather than continuity of the approaches. The last two essays in the section—by Elizabeth Bussiere concerning Warren Court resistance to recognizing basic welfare rights, and by Ronald Kahn concerning Rehnquist Court constructions of constitutional law regarding abortion and religion—aim to demonstrate how legal doctrine and logics both structure and constrain judicial policymaking options.

Part 3 considers the influence on supreme court justices of “external” institutions and actors: Congress, the president, litigants, lawyers, interest groups, lower courts, and so on. Lawrence Baum’s excellent essay explores how the recruitment and selection processes along with other institutional factors end up “freeing” justices to undertake their distinctively legal modes of policymaking. The last four essays then signal a return of sorts to the paradigms introduced earlier. Lee Epstein and Jack Knight expand on their well-known rational choice analysis to demonstrate that increasing amicus curiae advocacy provides critical information to justices regarding how key political actors will respond to various policy decisions. Jeffrey Segal combines concerns of the previous two essays to “test” contending positivist models of decision making, which not surprisingly concludes in a vigorous defense of the attitudinal model against strategic approaches. As if to respond to Segal, Charles Epp marshals data and theory in service of the compelling argument that the Supreme Court’s policy agenda is the result of an interaction between the justice’s policy preferences and two institutional constraints—the institutionally transmitted responsibility to resolve policy issues that divide the lower courts and the organized “support structure for legal mobilization” by groups, patrons, and cause lawyers. Melinda Gann Hall and Paul Brace end the collection with a plea for developing rational choice analysis of institutional context through explicit comparative studies of state supreme courts as a way to “resolve some of the most perplexing problems in judicial politics scholarship” (p. 282).

Overall, this is an outstanding book. The featured authors are highly accomplished contributors in the field, most (but not all) of them in mid-career, which confirms the generational categorization. The essays are, not surprisingly, generally of very high quality. Indeed, the collection is unusually consistent in terms of sophistication, clarity, and significance. Moreover, most of the essays engage one another to some degree. A notable achievement is that many nicely clarify conceptual differences, politely acknowledge the contributions of rivals, and even make small accommodations in their own approaches. At the same time, neither the editors nor the contributors seek to promote any grand new syntheses. Indeed, a few authors state flatly that the different ap-

proaches are simply irreconcilable. This is probably wise, but it also signals the limits of engagement. What is striking about the debate represented here is the degree to which commitments to sustaining particular paradigms preclude taking seriously criticisms about paradigm limits as a motivation for fundamental conceptual innovation or reconstruction.

The present volume achieves its clearly stated goals quite effectively. It begins with quite traditional questions about how judges make decisions, but the featured contest among perspectives substantially widens the vista of inquiry, introduces a host of new variables, and opens the way for many new secondary questions. Perhaps the most important achievement of the new institutionalism is that its proponents have revitalized concerns about contextual complexity, political conflict, contested power, and normativity in the study of courts. Together with a companion volume by the same editors (*The Supreme Court and American Politics: New Institutional Interpretations*, 1999), which locates the Supreme Court even more broadly and provocatively within a divided American polity, Clayton and Gillman have at once successfully represented, elevated, and intensified contemporary scholarly dialogues about judicial politics.

**Regulating the National Pastime: Baseball and Antitrust.** By Jerold J. Duquette. Westport, CT: Praeger, 1999. 154p. \$59.95.

Arthur T. Johnson, *University of Maryland, Baltimore County*

Major league baseball, unlike other professional sports in the United States, has been exempt from antitrust laws for nearly a century. The reason lies with early state and federal court decisions, of which the most frequently cited is the Supreme Court's *Federal Baseball Club of Baltimore v. National League* opinion, authored by Justice Oliver Wendell Holmes in 1922. Baseball's legal status has been the subject of numerous law review articles and commentaries, historical narratives, and scholarly analyses. Nevertheless, Jerold Duquette claims that there has been no integrated and comprehensive examination of "baseball's unregulated monopoly."

Duquette employs an "historical institutionalist" method to explain the "persistence of the baseball anomaly." He wishes to explore not only the institutional and ideological but also the political and economic facts that have contributed to it. He uses the conceptual framework of Marc Allen Eisner (*Regulatory Politics in Transition*, 1993). Baseball's status is examined in terms of four periods identified by Eisner: the Progressive era, or the market regime, from 1880 to the late 1920s, when baseball's commercial character went unacknowledged; the New Deal era, or associationalist regime, from the 1920s to the early 1960s, when baseball's unregulated monopoly thrived; a new era of regulation, or the socialist regime, in the 1960s and 1970s, when baseball was severely challenged but still avoided regulation; and the efficiency regime of the 1980s and 1990s, when a shifting consensus on regulatory politics emerged and allowed baseball to remain the only professional sport exempt from antitrust law.

Duquette argues that major league baseball's leaders during the Progressive era "exploited both the cultural significance of the game and the institutional frailties of the federal government in their development of baseball law" (p. 22). During the New Deal, "the stability inside the game together with the instability outside of it both secured major league baseball's standing as the national pastime and insulated it from government interference," which resulted in baseball becoming a "cultural icon" (p. 43).

Baseball had lost icon status by the 1980s, challenged by shifting ideology nationally, a strong leader who led players' unionization efforts, the emerging popularity of other professional sports, nomadic franchises, and the growing influence of television on various aspects of sports. Despite growing fan cynicism in the 1980s, baseball exploited the dominant anti-government regulation philosophy to protect its status, and in the 1990s, despite much turmoil, it successfully coopted the major league players and won the support of the minor leagues (and host cities) as well as their political representatives, with the signing of the Curt Flood Act. This legislation, adopted in 1998, allows players to call upon the antitrust laws, but only after decertifying as a union, and maintains baseball's antitrust exemption on issues related to minor league players, franchise relocations, and the minor leagues.

Duquette concludes that although baseball interests are "succeeding in the political defense of their terrain, they are losing ground on the institutional and ideological fronts where the courts, the Congress, and the country are increasingly willing to reconsider" baseball's status (p. 127). That status, cultural and legal, is no longer what it used to be, and baseball's monopoly faces an uncertain future.

The book is well written and very readable. The author effectively and accurately provides a summary description of baseball's legal and political history, and he offers an insightful analysis of recent legislative events related to baseball. For example, he argues that Holmes's 1922 decision was consistent with Progressive jurisprudence regarding treatment of "incidental" interstate transportation, which is ignored or not recognized by most commentators. Duquette owes this observation, however, to G. Edward White (*Creating the National Pastime: Baseball Transforms Itself*, 1996). Also, Duquette may be the first academic to put into a political and legislative context the role of the minor leagues in organizing support for major league baseball's lobbying efforts on the antitrust issue in the late 1990s. He introduces to the academic community a political analysis, brief as it is, of the Curt Flood Act. Although the book is successful as a brief review of the history of public policy toward baseball, overall the analysis and conclusions are not surprising or different from what already has been written.

At a superficial level, Duquette presents a persuasive argument that "the unique place of the business of baseball in America—its regulatory anomaly—is a function of the game's historical development as encased in the interplay of institutions and ideas, as well as politics . . . and a fuller understanding of it is gained by focusing on . . . changing regulatory regimes" (p. 135). Yet, one is left wanting more than Duquette provides. At the macro level, the relationships he describes do exist, but does that close the argument? The maxim "correlation is not causality" applies. The argument is incomplete without a more rigorous analysis of events and issues.

Duquette identifies key issues and events, but little detail is given to demonstrate his primary thesis. This is because he builds his analysis primarily from secondary sources. Only a few interviews were conducted, and most were not with the primary figures in the more recent events. Numerous congressional hearings are referenced, but more basic research is needed to support the conclusions that Duquette draws. For example, he discusses the Sisk hearings in 1976 and ties the outcome (no action) to his analysis of the 1980s. He barely addresses what was considered at the time the prime reason for those hearings—the attempt to force major league baseball to place a team in the nation's capital—and he fails to explain in any detail why a successor committee to Congressman Sisk's Select Committee on Professional Sports was never convened. He fails to recognize the use of congress-

sional hearings as theater or bargaining tool. Despite his reliance on congressional hearings as a primary source of information, Duquette ignores other congressional sources. Also, he does not provide sufficient detail to help the reader understand how representatives of the professional sports industry influenced policymakers, whether as “vocal advocates of the progressives’ social, cultural and political agenda” or as lobbyists in the 1990s, even though much of his discussion is about that phenomenon.

Absent such details and any quantitative data, the author makes generalizations that are open to question, or at least in need of greater explanation. For example, he places the beginning of fan cynicism in the 1990s (p. 74), whereas many would trace it (as related to baseball) to as early as 1957 (the Dodgers and Giants move to California) or to the 1960s (CBS purchases the New York Yankees, and the Braves move to Atlanta). Duquette refers to the mobilization of fan discontent in the 1990s, when any evidence of an effective organized fan movement at any time is scarce, even Ralph Nader’s Fight to Advance the Nation’s Sports (FANS) in the 1970s, which survived one year.

Duquette refers to baseball as “big business” throughout the book but fails to provide any financial information relative to other industries. Although this is true today, was it true in the early twentieth century? In the 1950s or 1960s? For example, we are told that baseball “was the only industry of its size and scope that was not subjected to regulation by the federal government” during the Progressive era (p. 21). What were the total revenues of baseball compared to other business of the time? No data are offered to support the economic status of baseball as big business, which is an important question, given the subject of the book.

In sum, Duquette provides at one level a well-crafted argument that offers insight into the historical status of baseball. At a more demanding level of explanation, Duquette misses the opportunity to persuade with detailed analysis and empirical evidence.

**Money Matters: Consequences of Campaign Finance Reform in U.S. House Elections.** By Robert K. Goidel, Donald A. Gross, and Todd G. Shields. Lanham, MD: Rowman & Littlefield, 1999. 215p. \$62.00 cloth, \$19.95 paper.

Candice J. Nelson, *American University*

In an era when campaign finance reform is widely discussed by politicians, political scientists, and journalists, *Money Matters* provides a timely and thoughtful analysis of several reform proposals. The central focus is on the effect that spending limits, matching funds, full public funding, and partial public funding with spending limits would have on electoral competition, voter turnout, and voter involvement in elections to the House of Representatives.

A brief chapter introduces the approach of the book, and chapter 2 provides a comprehensive history of the relationship between money and elections from the earliest days of the country. It does an excellent job of showing how the linkages between wealth and campaigning were established and continued to be a thread throughout the nineteenth and twentieth centuries. The chapter also succinctly describes the characteristics of the current campaign finance system and sets the stage for the discussion that follows.

In chapter 3 the authors raise questions that challenge each of the five arguments often advanced by opponents of reform. For example, some say that spending limits are not needed, because not all that much money is spent on politics. This argument is usually advanced by comparing candidate

spending to commercial product advertising. The authors make a credible case as to why choosing a candidate and buying toothpaste differ, but they are less successful in articulating a standard by which to measure how much spending is too much or not enough. Similarly, regarding the argument that reform always fails, the authors point to the Australian ballot and civil service reform as two successes. Opponents would likely counter that such examples hardly point to the success of campaign finance reform specifically.

In chapters 4 through 7, the authors simulate the effect that varying levels of spending limits, matching funds, full public funding, and partial public funding combined with spending limits would have on competition, turnout, and voter information in House elections. By far the most interesting, and the chapter most likely to contribute to the debate surrounding spending limits and public funding, is chapter 4. The simulations assume spending limits, matching funds, and full public funding at levels from \$100,000 to \$1 million, and the combination of partial public funding and spending limits within the same range. The findings support the argument by opponents of reform that spending limits, without some public funding, would hurt challengers, although not to the degree one might expect. The simulations also suggest that some form of public funding, either through matching funds or full or partial public funding, would help candidates of the minority party, except at the very lowest levels of funding.

Chapters 5 through 7, which examine the effect of the four proposals on voter turnout and voter involvement in the election, find that campaign spending is much less important to turnout than is contact with political parties and campaigns. Spending limits might marginally reduce turnout, and partial or full public funding might marginally increase turnout, but the increases or decreases would be less than 2%. Confirming the earlier work of Steven J. Rosenstone and John Mark Hansen (*Mobilization, Participation, and Democracy in America*, 1993) the authors find that individual turnout is increased through contact with the party or the candidate, not by the amount of money spent. Yet, because the authors look at aggregate candidate spending, not where the dollars go, it is not clear whether certain campaign spending would increase turnout. Their findings suggest that spending on field and get-out-the-vote programs might stimulate turnout, since these programs involve direct contact. Due to filing requirements of the Federal Election Commission, it is not possible for scholars to break down campaign spending into categories, unless campaigns choose to submit detailed information to the commission.

Chapter 7 finds that voter contact with the party or the campaign is more important than spending in a voter’s familiarity with incumbents and challengers, placement of the candidates on an ideological spectrum, and concern with the outcome. The authors speculate that in some cases increased spending may in fact blur differences between candidates, but the data does not allow this hypothesis to be explored in any depth.

The last two chapters outline the most common loopholes in the Federal Election Campaign Act—soft money, issue advocacy, independent expenditures, and bundling—that have essentially eviscerated the contribution limits of the act and analyze three degrees of campaign finance reform: minimal, modest, and comprehensive. The authors clearly state the objectives they think reform should meet and evaluate them in terms of the three degrees. For both proponents and opponents of reform, this chapter holds no surprises.

The authors acknowledge the political difficulties in passing comprehensive campaign finance reform—full public

funding, bans on soft money and independent expenditures, and limits on issue advocacy similar to those in an earlier version of the McCain-Feingold bill—but they support such reform and predict that increasing abuses will eventually strengthen pressure for comprehensive change. The authors have a normative bias in favor of reform as defined by good government groups over the years, but this bias generally does not come through in their analysis. For anyone unfamiliar with the issue, this book provides an excellent framework from which to understand the discussion. Its strongest contribution is the analysis of public funding, spending limits, and electoral competition, which provides very specific evidence to support the argument that spending limits and public funding would help level the playing field in House elections. *Money Matters* advances our understanding of the issues surrounding the debate over campaign finance reform.

**Passages to the Presidency: From Campaigning to Governing.** By Charles O. Jones. Washington, DC: Brookings, 1998. 224p. \$39.95 cloth, \$16.95 paper.

Ryan J. Barilleaux, *Miami University (Ohio)*

The American political system has many features that set it apart from other governments of the world, but not all are equally apparent. One distinctive aspect is the length and importance of the transition period from one presidential administration to another. In most countries the passage of power occurs almost as soon as the election results are known (consider, e.g., the rapid assumption of power by President Kostunica after Slobodan Milosevič admitted defeat in the September 2000 Yugoslav election), but in the United States roughly ten weeks elapse between the election and inauguration. The American approach, as Charles Jones puts it in this outstanding book, is to transfer power at a “leisurely pace.”

That pace is nevertheless deceiving. For the new president and incoming personnel, life is anything but leisurely. Indeed, a common theme among those involved in presidential transitions is their sense of being gripped by urgency and haste: The political clock ticks relentlessly. In an environment dominated by the need to act quickly, there is ample opportunity for trouble.

The importance of the transition period has concerned practitioners and scholars for some time, although many close observers still do not appreciate how influential these ten weeks can be in launching a new administration on the right foot. Not many years ago, a veteran political journalist went so far as to assert that the transition should be shortened to days instead of weeks, as if a change in leadership necessarily improves with speed. Perspectives such as this do not appreciate the differences between the American system and those of other nations, where only a small number of top personnel change after the election of a new leader. In most other democracies, government departments and even much of the executive branch are staffed by civil servants, so few people have to be found to fill these positions. In contrast, our system is distinguished by the large numbers of posts that have to be filled by a new president, ranging from the Executive Office through several layers in numerous departments and agencies. In 1960–61, John Kennedy lamented that he had once thought he knew everyone he needed to know in Washington. Now that he had to fill so many high-level jobs, JFK remarked, it seemed that he did not know anyone. In the decades since then, the magnitude of the problem has only grown.

The presidential transition has been an increasing focus of

scholars in recent years, and their work has tended to follow two paths. One is the concerted effort to document the record of presidential transitions, at least during the past four decades. The purpose is twofold: Discover what happened and determine the activities or strategies that make transition more or less effective. The other path has a didactic purpose: Extract lessons for future presidents to follow in managing their own assumption of power.

Charles Jones follows the dual approach of other authors but makes a unique contribution. As one would expect from the scholar who almost single-handedly restored respect for the constitutionally separated system among students of the presidency, Jones illuminates the subject of presidential transitions in a way that few of his colleagues can match. In addition to the conventional topics—organization, personnel, agenda setting—he introduces other perspectives that are especially valuable for understanding how transitions fit into presidential politics specifically and the American system more generally.

Jones begins with a discussion of transitions and democracy, noting that peaceful transfers of power are at the heart of democratic government: “Representative democracy is about transitions. Citizens are chosen in free elections to become agents . . . every election is a time of transition, with first-time winners experiencing the challenge of assuming another’s stead and incumbents reading the results as a measure of their performance” (p. 4). Transitions are about something much larger than changing the names on the doors.

Jones interviewed an array of people involved in presidential transitions over the decades he examines. One focus is on “the biggest mistake” made during a transition, or what might be termed “if I knew then what I know now.” These mistakes are familiar to those versed in transition studies and often result from the sense of urgency: time and opportunities wasted, misjudged or mishandled appointments, failure to establish a good working relationship with Congress, poor organizational discipline. Jones’s summary comment on these mistakes points to the most essential task of transition: “being prepared to complete the shift from campaigning to governing” (p. 176). Presidents-elect and their aides must learn to think in a governing mode rather than a campaigning mode.

At the end of his study, Jones poses an intriguing idea: The transition typical of the post–World War II era may be fading into history. Ruminating on the failures and activities of the Clinton transition in 1992–93, Jones notes that it did not fit the mold of the “conventional transition.” Although Clinton and his people could have done many things more effectively, Jones suggests that a more campaign-oriented style of governing may be a permanent feature of the presidency for the foreseeable future. If that is so, then the Clinton case may be something other than a failed conventional transition. Jones finds much to fault in the Clinton transition, as do many of the participants, but he believes future transitions may contain several similar elements, such as use of outside political consultants, interest-group mobilization, and heavy reliance on polling. In the future, the Clinton case may itself be seen as the transition to a newer, campaign-oriented model.

Jones introduces a new concept—voice—that is a major advance in our understanding of how the contemporary presidency operates. As he explains it, “voice features constant monitoring of the interests and concerns of ordinary Americans, sympathetic exposure of these matters in a ‘family values’ setting, exhortation for a community solution . . . liberal use of executive orders and other presidential prerogatives . . . and little deference to jurisdictional bound-



aries between public and private or levels of government” (p. 193). Voice not only helps explain the Clinton presidency but also illuminates trends that presidential scholars have sensed but not examined directly.

Few scholars of American government other than Jones could pack as much into a book of this size, and do so in a highly readable way. He provides another major advance in our understanding of the nation’s highest office, and he raises issues that cry out for a sequel. The material in his conclusion, on a new model of transitions and the use of “voice,” deserve greater attention. Jones and other presidential scholars have their work cut out for them.

**Entangling Relations: American Foreign Policy in Its Century.** By David A. Lake. Princeton, NJ: Princeton University Press, 1999. 332p. \$60.00 cloth, \$17.95 paper.

Glenn Hastedt, *James Madison University*

David Lake provides a theoretical framework for understanding the security choices made by the United States in the twentieth century. He grounds his work in the metaphor that politics may be understood as firms producing security. The fundamental choices before states are unilateralism and cooperation. The former is equated with production within a single firm, and the latter can take several forms. Principal among these are alliances, in which polities act as if they were separate and independent firms entering into joint production agreements, and empire, which is similar to the integration that takes place in the modern multidivisional corporation. Alliances and empires form the end points of a continuum of security relationships. Alliances are at the anarchy end, as each polity retains full decision authority. Empire is at the hierarchical end, because one decision maker is dominant over others. Spheres of influence, protectorates, and informal empires are intermediate points.

The choice between unilateralism and cooperation is determined by three primary factors: joint production economies, the expected costs of opportunism, and governance costs. Couched in more political terms, this means that American foreign policy security choices are dependent upon the following considerations. How large are the gains from cooperation? Are partners reliable, and what are the risks of opportunism on their part? How costly are possible security relationships? According to Lake, alliances are most likely to be formed when the gains from cooperation are considerable, there is little risk of opportunism, and only slight costs are entailed in monitoring and enforcing the relationship. Empires are most likely when the gains from cooperation are great but so are the risks of opportunism. Hierarchy reduces these risks. Governance costs under empire are low and increase slowly with the move toward establishing hierarchical relationships.

Lake identifies three eras in twentieth-century American security policy. The interwar years (1919–39) were a period of aborted cooperation and reaffirmed unilateralism. The Cold War (1945–89) and the new world order (1990–present) are defined as periods of successful cooperation. Relatively anarchic cooperation marked many of America’s dyadic relationships in these periods, but examples are also given of protectorates and empire. As to the future, Lake identifies two contradictions that need to be managed successfully. First, American foreign policy facilitates cooperation among partners by reducing the risk of opportunism, but at the same time it sets limits on the gains from cooperation by providing a disproportionate share of the forces used in joint military operations. Second, through its leadership the

United States subordinates other states, which limits the burdens they are willing to shoulder and raises the governance costs of cooperation.

Lake indicates that he is not attempting to formulate a new theory of American foreign policy. Instead, rational contracting is an attempt to bridge theories rooted in neorealism, neoliberal institutionalism, and constructivism. Bridges are not easy to construct. Their ultimate success depends upon the strength of their foundation and the ease with which they allow one to move from one theoretical perspective to another. Rational contracting is most closely related to neoliberal institutionalism. An important question for future research is whether Lake’s analysis is rooted firmly enough in the other two perspectives to permit easy movement among all three, or is yet another island in international relations theory.

Two types of assessment are needed to determine the strength of its foundation. First, how broad is it? What types of research agendas can it support? Lake notes that a potentially important extension of his study is the examination of international economic policy. Such an extension intuitively is plausible because, as Lake observes, international economic relations range from relatively anarchic (each state retains full control over economic policy) to hierarchic (decision-making power is transferred to another polity). Left unaddressed by this formulation is the long-standing debate over free trade, protectionism, and economic sanctions. These questions are as much about the content of policy choices as they are about unilateral versus cooperative action, but they are central to the study of American foreign policy.

Second, can the methods used to construct this theoretical foundation be applied to other polities? In addition to assessing evidence on the actual choices made by policymakers, Lake examines the surrounding policy debates to see whether policymakers and the public were indeed interested in the types of concerns identified by his theory. This adds texture and richness to his presentation. Yet, with all research strategies that focus on societal conditions, domestic politics, or political culture, the challenge becomes one of replication in settings in which these data are not as readily available.

Bridges are needed in a discipline characterized by islands of research that often do not communicate well. *Entangling Alliances* holds great potential for serving as such a bridge. It is firmly rooted in theory and addresses concerns raised by those who stress both systemic and domestic causes of foreign policy decisions. In addition, the key concepts help us look at longstanding research questions in a new light, such as the reasons for alliance formation.

**The Presidency and Domestic Policy: Comparing Leadership Styles, FDR to Clinton.** By William W. Lammers and Michael A. Genovese. Washington, DC: CQ Press, 2000. 383p. \$28.95 paper.

Peri E. Arnold, *University of Notre Dame*

Can presidency research do more than probe idiosyncratic cases? Our knowledge of the American presidency and the status of presidency studies in political science both hang on the answer to that question. Indeed, the current research agenda in this field exhibits a confidence that its findings are amenable to generalization and deserve the discipline’s approbation.

The most prominent contemporary scholarship on the presidency stimulates further theorizing and invites general-

ization. The current concept of presidential behavior emphasizes factors of political context or personal style and character. For example, Stephen Skowronek (*The Politics Presidents Make*, 1993) argues that presidents' possibilities are defined, on the one hand, by their sequence within a regime cycle and, on the other hand, by the developmental state of American politics and institutions in their era. Alternatively, the line of theorizing begun by Neustadt in *Presidential Power* (1960) finds current expression in Fred Greenstein's (*The Presidential Difference*, 2000) examination of executive performance through variations in presidential styles.

Most books aimed at the undergraduate market convey little about the analytic issues in presidency research and do little to advance undergraduates' analytic capacities. *The Presidency and Domestic Policy* is a welcome exception. It was written primarily by William W. Lammers, and Michael A. Genovese took up the task of bringing it to print after his former teacher's death.

The book contains excellent profiles of modern presidents' influence on domestic policy. It also offers a theoretically informed explanation of the leadership performance of these presidents, from Franklin Roosevelt through Bill Clinton. The central argument is that leadership performance in domestic policy is an expression of "the styles and strategies presidents employ in their efforts to govern" (p. 3). They formulate, effect, and implement domestic policy through decision making, administrative work, public leadership, and relations with Congress. Therefore, the way they address tasks in these four dimensions should affect their efficacy in domestic policy. Yet, an assessment of relative efficacy requires an assessment of the domestic policy opportunities present within each context.

Opportunities for domestic policy innovation vary over time. These depend upon a number of contingencies, including winning margin, popularity while in office, electoral fortunes of the president's party in Congress, and budget deficits or surpluses. The authors do not consider broader historical perspectives in conceptualizing the opportunity context, and they might fruitfully have extended their thinking to include work such as Skowronek's or Lowi's (*The Personal President*, 1976).

Lammers and Genovese propose that the opportunity context can be ranked as high, moderate, or low. The first section treats the three high-opportunity presidents: Roosevelt, Johnson, and Reagan; the second covers the moderate-opportunity presidents: Truman, Eisenhower, and Kennedy; and the third examines the four low-opportunity presidents: Nixon, Carter, Bush, and Clinton.

The chapters on individual presidents synthesize existing scholarship and provide an overview of their styles, strategies, and effects on domestic policy in light of their opportunities. Although the profiles are sound and use the salient literatures well, they are constrained by their high level of generality. Theoretically informed case studies would give added weight to suggestions of causal relationships between elements of a president's style and the political consequences of his actions.

The concluding chapter compares the ten modern presidents' domestic policy leadership. Holding opportunity contexts constant by comparing presidents within each of the three categories, the authors observe that "some presidents have used their political opportunities effectively, while others have played their hands rather poorly" (p. 331). They consider Roosevelt the most efficacious domestic policy leader among high-opportunity presidents, Truman among the moderate-opportunity group, and Nixon among the low-

opportunity group. A notable finding is that presidential efficacy seems time bound. Mid-century presidents appear more likely to be efficacious domestic policy leaders than later presidents, whether their opportunity context is high, medium, or low. Lammers and Genovese ask: "Are present-day presidents hampered by new forces in demonstrating their skills and taking advantage of their opportunities?" They conclude: "Presidents serving since 1973 have indeed been affected by a series of limiting influences" (p. 351).

Unavoidably, the focus on domestic policy limits the analysis of presidential leadership. Indeed, the resonance between foreign and domestic policy since 1945 makes it difficult at times to distinguish precisely leadership in the two arenas. Consider, for example, Johnson's hesitancy to choose between "guns and butter" in the mid-1960s. Yet, the authors give the Vietnam War only passing reference in discussing Johnson's domestic policy leadership. To place foreign policy in the background results in a less than full analysis of these presidents' leadership skills or opportunity contexts.

On balance, this book is quite successful. I recommend it for undergraduate course use. More specifically, its pedagogical payoffs are twofold. First, it is an engaging, thoughtful, and well-written narrative of the modern presidents' initiatives and influence on domestic policy. Second, the conceptual focus on presidential performance and leadership style can strengthen the analytic focus of presidency courses.

**Pitiful Plaintiffs: Child Welfare Litigation and the Federal Courts.** By Susan Gluck Mezey. Pittsburgh: University of Pittsburgh Press, 2000. 209p. \$45.00 cloth, \$19.95 paper.

William T. Gormley, Jr., *Georgetown University*

In 1988 the American Civil Liberties Union filed a class action lawsuit against the Illinois Department of Children and Family Services (DCFS), on behalf of B. H., a 17-year-old youth in foster care, and nearly 20,000 other children forced to live outside their home because of abuse and neglect. Attorneys accused the DCFS, responsible for protecting and placing such children, with violations of Illinois statutes and the due process clause of the Fourteenth Amendment. B. H., for example, had been placed in ten different settings despite clear evidence that children require continuity of care for their emotional growth.

In *Pitiful Plaintiffs*, Susan Gluck Mezey focuses on the *B. H. v. Johnson* case in an effort to illuminate child welfare litigation: its origins, evolution, and consequences. The result is a rich and textured case study that sheds considerable light on how the process works. It also reaffirms the old adage that politics, like sausage, should not be viewed in the making by those with weak stomachs or faint hearts. Many Illinois children were uprooted from troubled homes, only to be placed in equally troublesome foster care settings. Although the judicial system responded to their plight, it took years for positive consequences to be discerned.

The *B. H. v. Johnson* case is a particularly good choice for an in-depth analysis. During the 1990s, Illinois had the third highest substitute care population in the United States. Problems with foster care in particular were rampant and acute. The consent decree that ultimately resulted from the B. H. litigation was unprecedented in magnitude among child welfare cases. Moreover, the DCFS was subject to as many as eight separate consent decrees, including the B. H. decree. Clearly, this case involved many interesting legal and political developments and high stakes: It represented nothing less than an effort to restructure one of the largest child welfare systems in the United States.

To better understand the B. H. case, Mezey interviewed a diverse group of 57 people, including attorneys for both sides, federal district court judges, and juvenile court judges. Many agreed to let at least some of their remarks be quoted, which enabled Mezey to be precise about the sources of numerous observations. In addition to the interviews, Mezey reviewed the relevant case law, not only Illinois cases but also key federal cases in other states and relevant decisions by the Supreme Court.

In constructing and organizing her narrative, Mezey blends approaches developed by Phillip Cooper, Robert Wood, and Wayne Welsh. The resulting analytic framework highlights five phases of litigation: stimulus, accountability, adjudication, implementation, and response. Although these stages are not altogether discrete, and the accountability stage might be more aptly named, the framework is serviceable enough. Like similar frameworks long used in the public policy field, this one enables the author to offer a chronological account enriched to some degree by political science concepts.

The most significant contribution of this book is that it highlights the extraordinary difficulty of reforming a child welfare system that can only improve if numerous public officials respond favorably to interest group pressure. The catch is that a favorable response in one institutional arena does not guarantee a favorable response elsewhere. Moreover, it is difficult to sustain the zeal of a reformist movement, due to personnel turnover in the executive branch and smoldering resentments within the legislative branch. For these reasons, federal district court judge John Grady may have been wise to promote a negotiated settlement rather than a bitter, protracted court trial. Even within the less adversarial setting of a consent decree, mutual recriminations made it difficult to achieve significant progress.

An attractive feature of the book is that it combines legal, political, and policy analysis. The legal analysis is excellent and lucid enough for advanced undergraduates to follow. The political analysis is wide-ranging and instructive, with good insights into each branch of government and occasional references to federal-state relations. The policy analysis is the weakest link of the triad, perhaps because of the paucity of good evidence on which of several factors best explains eventual improvements in the Illinois child welfare system. In chapter 7, Mezey lists several possible explanations, but neither she nor her sources do an effective job of disentangling them.

A weakness of the book is that Mezey's own viewpoint on key questions sometimes remains submerged. For example, in chapter 6, Mezey summarizes the views of the respondents on whether Judge Grady was too timid and whether the monitor he appointed was ineffectual. Here, Mezey is scrupulous about reporting the opinion of respondents but does not directly answer these questions herself. In chapter 8, Mezey offers a glimpse of her thinking on these issues but does not fully present her own point of view.

Another limitation of the book is that it suffers from the usual weaknesses of the case study genre. Mezey argues that it took interest group litigation to achieve systemic change in Illinois. But is litigation necessarily the best strategy in a state with a more sympathetic legislature or a more competent bureaucracy? On issues such as this, a comparative case study offers striking advantages over the single case.

Throughout the book, Mezey is critical of Donald Horowitz and others who assert that federal judges overextend themselves by playing an aggressive role in state institutional reform cases. She nicely demonstrates that Judge Grady did not exhibit unbridled judicial activism in the B. H. case.

Rather, he prodded the parties to reach a negotiated settlement and avoided the micromanagement that conservative critics decry.

It is less clear that the American Civil Liberties Union chose the right strategy in launching a massive legal assault on the DCFS. In a tantalizing reference to *R. C. v. Hornsby*, an Alabama foster care lawsuit with impressive outcomes, Mezey hints that a somewhat narrower focus in Illinois might have yielded swifter progress. Without an in-depth analysis of the Alabama case, however, we will never know for sure.

More broadly, the circumstances under which litigation on behalf of children ought to be undertaken also need to be better defined. For example, we are beginning to see lawsuits filed against day care centers, family day care homes, and resource and referral agencies that refer parents to child care providers. Because children cannot speak for themselves, it is tempting to turn to the courts to seek relief. But even if judges behave with circumspection, the judiciary may not be the best venue for a resolution of these disputes. Judges lack expertise in these matters, as Judge Grady openly admitted. Also, judicial intervention, at best, is a blunt instrument for reform. In the B. H. case, it made possible a significant reallocation of resources to the Illinois child welfare system, but it could not guarantee a change in the culture of the DCFS bureaucracy. Even supporters of the lawsuit concede that it fell short of its stated goals.

Although *Pitiful Plaintiffs* does not answer some important questions, it offers considerable insight into an important lawsuit and its aftermath. It also helps correct an imbalance in the judicial politics literature by focusing on the federal district courts, as opposed to the Supreme Court or the circuit courts of appeals. The book is well written and tells a compelling story. It is likely to be well received by students in courses on judicial politics, public law, interest groups, and possibly social policy. Although it will not end debates on the appropriate role of federal litigation in reforming state bureaucracies, it does advance our understanding of the underlying issues.

**Defending Government: Why Big Government Works.** By Max Neiman. Upper Saddle River, NJ: Prentice Hall, 2000. 260p. \$27.00 paper.

John C. Pierce, *University of Colorado at Colorado Springs*

Max Neiman provides a concise, well-written, and comprehensive critical analysis of "the conservative attack on the public sector, especially its explanation for and evaluation of the size and growth of the public sector in the United States" (p. viii). In doing so, however, he only partially fulfills what is promised in the subtitle, namely, explaining why big government works. Rather than explicitly assess the reasons for goal achievement in a variety of policy areas, as the title implied to me, Neiman focuses on why we have big government and on the various critiques of that size. To be sure, the book is appropriate for upper division and graduate courses in political science, public policy, or public administration. Indeed, the clear organization of the analysis, the lucid and economical writing, and the political salience of the substance make it accessible to good students. But Neiman clearly has a broader audience in mind, including the scholarly community, in both political science and related disciplines. For that audience Neiman also performs a significant service by the broad sweep of the analysis and the precise focus on key elements in the major arguments over big government, both intellectual and political.

Neiman begins by confronting a fundamental point of

contention: Has there been substantial growth in government size? He concedes that growth in absolute dollars “provides some astonishing figures” (p. 21), but he argues that an accurate assessment involves size relative to GNP. “In recent years there has been a decline and then a steadying of the percentage representing government taxing or spending as a proportion of GNP” (p. 24). Neiman then turns to alternative macrodeterminants of government growth, apart from the deeply embedded sociocultural memories produced by the “pain and deprivation” of the Depression (p. 47), which supported an activist government. Those explanations are mostly Marxist in content, and they are more about “why government” than about why it works.

Another set of explanations is found in the public choice literature. “In public choice theory, then, rational, self-interested economic roles prevail in both private markets and government sectors” (p. 69). The economic basis of these theories of government growth is attractive because of its wide applicability—to voters, to groups, to government bureaucracies themselves. Even so, according to Neiman, theorists are not uniform in their conclusions about rational choice and government size. Some suggest rational choice leads inevitably to more government as it competes for supporters in the public, and others (Neiman names Anthony Downs) believe it leads to restraint in government spending.

Neiman looks at the evidence and concludes that war-making and preparing for war “stand out as the premier causes of government growth” (p. 114). The reason is that the threats and dangers attendant to external conflict remove or minimize the consequence of other barriers to growth. Does that explanation fit with the major theories Neiman reviews? The answer is that war is a displacement factor, “supplanting existing notions of what is acceptable in the way of government involvement” (p. 93). Neiman then looks at some of the other dimensions of the dispute about size, such as whether big government is necessarily bad, leading to oppression; he concludes not, claiming that “democracy sometimes requires the public to use the tools of government to liberate individuals from the constraints of bigotry and unfairness” (p. 133).

What does Neiman not consider that he should have? Apart from the need to come to terms with the subtitle, three themes could have received much greater attention: technology, globalization, and social capital. These are not ignored completely but receive scant discussion, even though they are major policy arenas of dispute, both in public forums and scholarly journals. First, the implications of technology for government growth as well as the rapidly increasing penetration of personal lives and economic structures by that revolution deserve more than a paragraph or two (p. 46). Microsoft and Al Gore notwithstanding, the issues of e-commerce, market monopolies, surveillance, electronic democracy, volatile NASDAQ levels, and national security initiatives, among others, all beg for much greater coverage here, even if forced within the theoretical structures Neiman lays out and critiques so carefully.

Second, Neiman touches on the potential effect of globalization, but the review is skimpy (pp. 217–8). The possible implications of that powerful disturbance of economic and political structures for the role of government, both in the United States and elsewhere, should be examined in great detail, especially in the context of the other explanations for government growth covered in the book.

Third, few objects of analysis have received more attention in recent years than the notion of “social capital,” broadly seen as the social and political trust-based sets of relationships among individuals that provide them with the foundation to work collectively to achieve shared goals (James S.

Coleman, *Foundations of Social Theory*, 1990; Francis Fukuyama, *Trust: The Social Virtues and the Creation of Prosperity*, 1995; Robert Putnam, *Bowling Alone: The Collapse and Revival of American Community*, 2000). The concept attempts to link economic analysis to social analysis of the relationship between government and the individual. Neiman does not use that particular terminology but is aware of this connection: “The language and tactics of contemporary criticism of public policies and the scope of government [are] designed to underscore the lack of communal objectives and shared interests among Americans” (p. 211). But it also is important to consider the role of social capital in the control of big government, which may exceed the acceptable bounds of its legitimate activity. The trust base of collective activity can both lead to big government and constrain it. Trust is both source and sanction.

*Defending Government* is strongly recommended to those in search of a clear, concise, and intelligent review of arguments about the growth of government, but it self-consciously originates in a particular political perspective. Even so, scholars and students alike will benefit from a careful reading of this book, as well as from a careful consideration of its implications for a rapidly changing political, social, and economic environment.

**Good Advice: Information and Policy Making in the White House.** By Daniel E. Ponder. College Station: Texas A&M University Press, 2000. 244p. \$39.95.

Andrew Rudalevige, *Dickinson College*

That the amount and nature of the information reaching the president matters for the choices he makes is hardly disputed, but translating this insight into analysis has been slow work. This is true especially in comparison to other subfields (e.g., legislative studies), which in making use of the new institutionalism have stressed information by highlighting the roles institutions play in ameliorating the uncertainty rampant in political decision making. Daniel Ponder’s new book, then, is particularly welcome. *Good Advice* asks some critical questions: What did the president know, and how did he come to know it? Equally important, how did that matter?

According to Ponder, presidents do not face a dichotomy between responsiveness (politics) and competence (administration) but a continuum linking the two. They must choose a point along this axis to invest resources for policy formulation in a given field. Should they rely on the White House staff? On the various departments? The investment metaphor (pp. 30f) is used to suggest that presidents select among numerous sources and vast amounts of information, with an eye toward minimizing their risk and maximizing their payoff from any given policy choice.

In theory, presidents may call on any number of advisers. In practice, it is often argued, modern presidents have moved the locus of advice out of the wider bureaucracy and into the White House: they have “centralized” (a term coined in Terry Moe’s influential essay, “The Politicized Presidency,” in John Chubb and Paul Peterson, eds., *The New Direction in American Politics*, 1985). Ponder is sympathetic (perhaps even at times too generous) to this argument. To his great credit, however, he goes beyond a straightforward application of its logic and seeks to refine its theoretical grounding. Using the institutionalist language of rule-driven behavior (in the broad sense of individual action in a context of formal and informal incentives and constraints), he posits that the White House staff will indeed always play an active role in policy development. Yet, that same emphasis on rules compels the

conclusion that “although policy making is centralized in the White House, it is not of one kind” (p. 35). Because presidential resources, incentives, and constraints, are dynamic—and because policy proposals differ along many dimensions—“the character of centralization differs . . . in both degree and kind,” even within a single administration (p. 6).

The result is a theory of “staff shift” (p. 6). Ponder suggests that under some circumstances the president will follow a pure centralizing strategy, with the White House staff serving as policy “director.” In other cases the president may use the White House as “facilitator,” to coordinate but not dominate the work of the departments. In still other cases, policy formulation may be largely delegated to the departments, with the White House serving merely as “monitor” (pp. 35–8). A proposal will succeed or fail depending in part on whether the president has appropriately matched policy to advising process. Again, the question is not responsiveness or competence, but how the two can be most effectively melded.

Ponder explores staff shift by tracing six domestic policy proposals in Jimmy Carter’s administration. To maximize the variation in cases, he selects on both the independent and dependent variables, that is, both on the advisory process and on its degree of success. He chooses two examples each of policy direction, facilitation, and monitoring, one of which is deemed a failure and one a success. A successful advising process does not necessarily produce a “win” in Congress but, rather, a feasible proposal in terms of substance, politics, and presidential preference. Ponder deems Carter’s 1980 youth employment plan, 1979 energy plan, and civil service reform successful; the creation of a Department of Education, welfare reform, and national health insurance are denoted failures. He finds some interesting commonalities: For example, failed cases tend to assess inadequately the political landscape, and successful processes provide the president with an effective “honest broker” in substance if not in name.

The cases are well buttressed by archival material from the Carter Library and are handled deftly. The conscientious exposition of the Carter policymaking process convinces the reader that staff shift is an empirical reality. But this approach, of course, brings its own advantages and disadvantages. On the plus side, Ponder can trace changes in the president’s management environment in some detail, and he controls for the vagaries of individual personality that often prompt the “yes, but . . .” critique of institutional approaches to the presidency. On the negative side, Ponder cannot explicitly generalize this result to other presidencies. Was the Carter administration different from others? Certainly, it distrusted the bureaucracy less than the presidencies before and after it; Moe, in fact, views Carter as a “pause” in the politicized presidency. Ponder clearly disagrees, but a broader approach would be needed to cement this point. Furthermore, as he is quick to note (p. 56), can six hand-picked cases “prove” the broader patterns of staff choice or that the strategies Carter chose were linked to systematic shifts in the incentives and constraints he faced?

*Good Advice* should be seen as illustrative, not definitive. Its great contribution is in moving the theoretical debate forward, infusing presidency studies with information-grounded institutionalism. In this respect it could perhaps be clearer in two areas. First, readers not already familiar with the jargon of new institutionalism may find this part of the discussion (e.g., the distinction, if any, between institution and organization) somewhat opaque. Likewise, Ponder generally presents his assumptions and definitions with admirable clarity, but it is sometimes hard to flesh out the set of conditions under which he argues presidents might appropri-

ately choose one advising structure over another. A careful read bears insightful fruit (e.g., proposals that cross departmental jurisdictions but require a good deal of technical expertise are probably best handled by a mixed, “facilitator” approach); a clearer synthesis of expectations versus findings would make these stand out.

Overall, *Good Advice* is a valuable addition, well thought out and well researched, to what we know about how presidents obtain and use information and advice. It moves the study of the institutional presidency in a very welcome direction.

**Public-Private Policy Partnerships.** Edited by Pauline Vailancourt Rosenau. Cambridge, MA: MIT Press, 2000. 256p. \$45.00 cloth, \$19.95 paper.

Jonathan P. West, *University of Miami*

Recent calls for public sector reform advocate reinvention, deregulation, reengineering, outsourcing, and privatization to address deficiencies in the classic bureaucratic model and to improve government performance. Reform efforts that seek to capitalize on the advantages offered by the three sectors—public, private, and voluntary—include experiments in public-private policy partnerships. Experience with cooperative undertakings between the state and for-profit or third-sector service providers spans the last three decades. This book examines the pros and cons of these public-private policy partnerships, isolates the determinants of success or failure across policy spheres, and identifies the circumstances under which cross-sector partnering should be promoted or avoided.

The book consists of thirteen chapters written by accomplished policy analysts. In the introduction, Stephen Linder and Rosenau effectively present the analytical framework and guiding questions addressed by chapter authors. These essays were published in a special issue of *American Behavioral Scientist* (vol. 43, no. 1, 1999). The contributors offer richly diverse perspectives linked to their various policy focuses and disciplinary specializations. Chapters assess partnerships in health, education, welfare, prisons, criminal justice, the environment, technology, nuclear power, transportation, and public infrastructure. Contributors include specialists in political science, public policy, law, economics, public administration, and public health. The qualitative and quantitative analyses draw on U.S. and Canadian experiences. Collaborative arrangements in various policy arenas range from minimalist to authentic partnering; the former type is most common, but the latter is becoming more frequent. Rosenau’s concluding chapter, rather than advocate or criticize partnerships in the abstract, carefully sifts evidence and integrates material from the policy chapters to evaluate the effectiveness of partnerships.

Each of the well-written essays contains valuable insights, but some more directly address the book’s central purpose as outlined by the editor. If the book is read selectively rather than sequentially, I suggest reading the introduction and the chapter by Stephen Linder (“Coming to Terms with the Public-Private Partnership”) for a general discussion of the topic and the multiple meanings of partnerships, followed with the four chapters by Sheldon Kamieniecki, David Shafie, and Julie Silvers on the environment, Harry Levin on education, Michael Sparer on health, and Anne Schneider on prisons. Together, these four most directly and completely examine the various policy choices and forms of public-private collaboration, the evolving roles and tasks of the three

sectors, the criteria for evaluation, and the evidence of partnership successes and failures.

Some conclusions from these four chapters are general and not surprising. Market-based approaches to environmental protection can be efficient and effective, but may fall short in addressing environmental justice and public participation issues (pp. 123–4). Support or opposition for educational vouchers is “premised on ideology and values rather than evidence” (p. 141). Devolution in health policy is “more rhetoric than reality” (p. 157). Prison privatization is driven by value orientations: “a . . . conservative, antigovernment, law and order ideology” (p. 207). Rosenau provides a more useful and overarching set of conclusions in the final and most instructive chapter. She offers a balanced, lucid assessment of the strengths and limitations of cross-sector partnerships, summarizing and synthesizing crucial lessons learned from the ten policy chapters. Her criteria include cost and quality performance, equity, access, citizen participation, and democratic processes. She also considers the implications of partnerships for government regulation, accountability, conflicts of interest, cost shifting, managing risk and uncertainty, and the likelihood of success.

Evidence is mixed on key evaluative dimensions, notably cost efficiency, equity, and access. Kamieniecki, Shafie, and Silvers find cost efficiency gains are likely in an environmental partnership program, Levin finds private school voucher programs to be more cost effective, and Schneider identifies slight cost reductions in private prisons; but these gains may be offset by externalities and diminished performance on other criteria (e.g., quality, equity, democracy, accountability). In many instances public-private partnerships increase the costs to society due to additional public spending on monitoring, regulation, assessment, and transaction costs, and these factors seldom are carefully weighed when calculating anticipated partnership costs. Evidence about the efficacy of partnering remains cloudy in some policy areas (see Mark Rom’s chapter on welfare services for the poor) and is much more positive in other spheres (development of fuel-efficient vehicles, discussed in James Dunn’s analysis of transportation partnerships).

The chapters on environmental protection, health care, and education partnerships suggest that gains in equity through increased choice and competition may involve tradeoffs that adversely affect vulnerable populations. Rosenau correctly observes that in such cases social values and quality considerations should trump cost calculations and profit maximization.

Considerable diversity is found with respect to the influence of intersector partnerships on public participation. In some policy areas (environmental protection) public-private partnerships reduce citizen input in the policy process, but in others, as Nicholas Lovrich notes regarding criminal justice, public/not-for-profit partnerships may be overly receptive to citizen involvement. Walter Rosenbaum’s analysis of the lessons learned from experience with commercial nuclear power facilities provides examples of how institutions can be designed to provide a more appropriate balance with stakeholder access, including access to public interest concerns.

Despite considerable strengths, this book shares the deficiency of many edited volumes in that quality is somewhat uneven and a uniform chapter format is lacking. Furthermore, since the bulk of the book was published previously, it is unclear why the material was republished, other than to increase its accessibility. Also, more careful editing would have eliminated multiple, distracting references to “this article” or “this issue.”

Aside from these relatively minor criticisms, the book is

valuable because it provides fresh, up-to-date, and compelling evidence regarding public-private partnerships, the risks and rewards associated with them, the ingredients affecting their success, and recommendations for their refinement and reform. In this way, it makes a substantial contribution to the ongoing dialogue among policy scholars and practitioners about the prospects and pitfalls of collaboration among the three sectors and charts a direction for future initiatives.

#### **Language Policy and Identity Politics in the United States.**

By Ronald Schmidt, Sr. Philadelphia: Temple University Press, 2000. 250p. \$65.50 cloth, \$21.95 paper.

Anna Sampaio, *University of Colorado at Denver*

In the late twentieth century, language conflicts became not only a central battle in public policy but also an essential medium for political expression among traditionally marginalized groups. This is clearly reflected in a host of policy initiatives (from antiimmigrant propositions such as 187 in California, to English-only and antibilingual education movements in such states as Arizona, California, Colorado, and Florida) and the proliferation of studies linking language with Latino and Asian American politics (e.g., Antonia Darder and Rodolfo D. Torres, eds., *The Latino Studies Reader: Culture, Economy and Society*, 1998; and Louis DeSipio and Rodolfo de la Garza, *Making Americans, Remaking America: Immigration and Immigration Policy*, 1998).

Ronald Schmidt provides a timely guide in this historical overview, with a particular focus on the competing discourses among assimilationists and liberal pluralists. Through his “value-critical” approach, Schmidt artfully weaves political history and theory with statistical analyses and policy reviews while centering on three primary themes: (1) educational policy for language minority children, (2) access to civil and political rights and government services by non-English speakers, and (3) the establishment of English as the sole official language of the U.S.” (p. 11).

The book begins and ends in much the same place; that is, chapters 1 and 2 review language law and public policy in recent decades, and chapter 8 presents the author’s own articulation of a language policy that builds on the “English-plus” initiative, with enhanced immigrant settlement and economic restructuring measures. Along the way, Schmidt provides a thorough examination of dominant theories and discourses in contemporary policy debates (chaps. 2–4); a comparison of these competing theories on foundational questions of justice, equality, and the common good (chaps. 5 and 6); and a critique of each of these philosophical trends (chap. 7). The result (spelled out in chaps. 7 and 8) is a thoughtful and sophisticated defense of a language policy that “will promote justice for language minority group members and facilitate the common good” (p. 180).

Moreover, the author successfully frames this debate in the context of “identity politics” (here defined broadly in both material and symbolic terms), and he uses the policy issues to ask and answer larger questions about immigration and discrimination/racism in U.S. history in terms of citizenship and national unity. This is best articulated in chapter 7: “Here, as we have seen the primary issue is not language per se, but social identities and their relationship to justice and the common good . . . . Language, then, is an important ingredient in the political conflict, but the central issue motivating that conflict is the nature of the American people and their relationships with one another” (p. 224).

Schmidt achieves remarkable balance, both methodologically and ideologically, without compromising his commit-

ment to defending language minority communities. Regardless of political dispositions, one is drawn into this timely topic by Schmidt's careful discussion of policy initiatives, law, and statistical data in the first few chapters. These lay a clear foundation for more complicated philosophical tensions between assimilationists and pluralists. Moreover, rather than positioning these competing discourses in a simple dichotomy (with all language activists as advocating clearly for one side or another), Schmidt elucidates the range of similarities and differences between assimilationists and pluralists, particularly around questions of public versus private speech, equality, and national unity (see specifically chaps. 5 and 6). As such, this book offers thoughtful insights into foundational questions of political theory, and it is a useful tool for public policy analysts. In using various chapters of the book in my class on Latino politics, I found that students were moved to develop their own positions, beyond simple speculation or political gesticulation, and to grapple with the difficult weaknesses in even their most favored bilingual education programs. Moreover, the thoroughness of the statistical and historical information provided students with the necessary material to defend their opinions.

In addition to thoroughness and balance, Schmidt provides distinct clarity on complicated issues, not only through his accessible writing but also by focusing on the competing politics of language policy rather than endless cases of success/failure in bilingual education programs. Readers come away with a more comprehensive understanding of the larger state and national issues and a clear framework for situating other studies in this field. The author also brings a new perspective by comparing various components of U.S. language policy (e.g., how individuals communicate with the federal government and vice versa) with policy in other parts of the world. To the extent that the book has any shortcomings, one is the limited information provided in these comparisons and about the relationship between U.S. language policy and the changes to federal politics introduced by globalization. The author touches on these topics in chapters 2 and 8, but it is hoped the information will be expanded in future editions.

Ultimately, this research will serve as a compelling intellectual exercise for students, scholars, and policymakers alike. It is highly recommended to anyone interested in understanding more about contemporary language policy and its relationship to issues of immigration, identity politics (with a particular emphasis on Latino politics), and theories of national unity.

**Culture Wars and Local Politics.** Edited by Elaine B. Sharp. Lawrence: University Press of Kansas, 1999. 250p. \$35.00 cloth, \$16.95 paper.

Barbara Ferman, *Temple University*

Although political conflict is certainly no stranger to U.S. cities, the contributors to *Culture Wars* suggest that a new kind of conflict, heavily imbued with moral overtones, is surfacing with more frequency on the urban landscape. Battles over abortion, gay and lesbian rights, hate crimes, and the like, are taking their place along side the more traditional disputes associated with service delivery, economic development, and redistribution of resources. The morality-based nature of these new culture wars has, according to the contributors, created a new type of politics that is evidenced in the way issues are presented, debated, and resolved. These differences are a function of the passion associated with moral claims, the involvement of religious organizations, and

the use of nonconventional protest tactics that can be fairly aggressive. These differences between how culture wars play out and politics as usual may render existing theories of local politics insufficient.

With this as their vantage point, the contributors to this excellent and thought-provoking collection seek to understand how local governments respond to the challenges implicit in culture wars and the factors that shape and influence the varied responses. Borrowing from earlier research conducted by the editor (Elaine Sharp, "Culture Wars and City Politics: Local Government's Role in Social Conflict," *Urban Affairs Review* 31 [July 1996]: 738–58), the contributors apply a typology of possible government responses: evasion, responsiveness, hyperresponsiveness, entrepreneurial instigation, repression, and unintentional instigation. The two major variables examined are institutional structure (both formal and informal) and political culture. The latter is taken directly from Daniel Elazar's typology of individualistic, traditionalistic, and moralistic cultures (*The American Mosaic: The Impact of Space, Time and Culture on American Politics*, 1994).

The book has two major objectives. The first is to assess the accuracy and usefulness of the above typology of responses, and the second is to examine the roles of institutional structure and political culture in shaping governmental responses. The objectives are pursued through nine case studies that examine morality-based controversies in more than twenty-five cities. These controversies include gay rights legislation, sexual orientation in the schools, hate crimes, abortion conflicts, and needle exchange. As a reality check of sorts, a tenth case study examines how urban policymakers see their role and responsibility in these culture wars.

The case studies reveal the usefulness of the response typology employed (i.e., all were observed in the cases) but suggest the need to add an additional category, "nonresponsiveness." Evasion was the most likely response and was found within all political cultures and across different institutional settings. By contrast, hyperactive responsiveness and unintentional instigation were extremely rare. Variation in formal institutional structure did not have much effect on how cities approached their roles in the controversies. Political culture, by contrast, did play a critical part in shaping governmental responses. Responsiveness and entrepreneurial instigation were associated with the presence of an individualistic culture, whereas repression was more likely to surface in traditionalistic and moralistic cultures.

*Culture Wars* is an extremely ambitious effort on several fronts. First, comparative case study research that involves numerous contributors is difficult under the best of circumstances. The authors in this collection were operating in largely uncharted territory, since this is the first attempt to examine culture wars across various cities. Second, they were seeking to develop a new framework for examining this relatively recent feature of urban politics. Third, the specific issues in the case studies all fall under the general heading of "culture wars," but there was significant variation in the nature of the issues, their developmental history, and the constituencies they attracted. Despite these challenges, the book presents a coherent body of analysis, breaking new ground in urban theory and comparative case study examination. As such, it makes several major contributions to the literature.

First, this is the first systematic look at culture wars at the local level. As such, the cases and the analytical chapters demonstrate that these conflicts are not isolated fringe phenomena that occur sporadically but are recurring phenomena that require systematic investigation. In pioneering

such an approach, the book provides subsequent scholars with a firm foundation, promising methods, several interesting hypotheses, and strong analytical frameworks. This work will no doubt lead to many follow-up studies.

Second, the volume broadens urban analysis to include social movement theory. Given the preoccupation of social theory with collective action issues, group dynamics, and coalition formation, it has much to offer urban scholarship.

Third, the authors provide a necessary corrective to the traditional “dependent city” perspective. The cases reveal the reciprocal relationships between local activities—including those of local governments and their officials—and state and federal activities. The San Francisco case, in particular, in which local government policy forced corporate giants to change some of their practices vis-à-vis domestic partner benefits, brings this message home loud and clear.

Fourth, the book promotes the idea that culture wars constitute a fourth arena of city politics (the traditional three are developmental, allocational, and redistributive). There are enough similarities between the political and institutional dynamics surrounding culture wars and those surrounding the other three arenas to make the case that culture wars are definitely within the realm of local politics. At the same time, the differences are significant enough to warrant a separate category.

Finally, the reader is forced to think about, in a normative way, the role of moral issues in local politics. Although this was not a primary objective of the contributors, there is no escaping the fact that moral issues are becoming increasingly enmeshed in political discourse and activities. The continuing activities of protest groups, such as those chronicled in this fine collection, combined with the fallout from the Charitable Choice Act will ensure that politics and morality continue their uneasy dance. The cases in this book provide many angles from which to view the intersection of politics and morality and much rich detail in which to root the larger perspectives. This excellent book should appeal to anyone interested in the issues that comprise the new culture wars, as well as to students of conflict theory, social movements, urban politics, public management, and public administration.

**Civic Engagement in American Democracy.** Edited by Theda Skocpol and Morris P. Fiorina. Washington, DC: Brookings, 1999. 528p. \$52.95 cloth, \$19.95 paper.

M. Margaret Conway, *University of Florida*

Concern about the health of civic life in the United States has generated academic debate, journalist commentaries, several study commissions, and publications by the score. Is civil society in decline, or is it reinventing itself? That is the question addressed from a variety of perspectives by the sociologists and political scientists who contributed to this volume. The chapters, initially prepared for presentation at a conference in September 1997, examine issues at the center of the debate from several theoretical perspectives. The editors organize the contributions under three headings: the roots of civic engagement, civic life in a changing society, and the ironies of contemporary activism.

In an introductory essay, Skocpol and Fiorina review three theories that posit alternative causes for ongoing transformations in civic life: the social capital approach, which emphasizes socialization into the norms, networks, and cooperative actions seen as necessary for solving social problems; the rational choice approach, which focuses on incentives for individual action; and the historical-institutional approach,

which emphasizes changing organizational patterns, the resources available for collective action, and relationships between elites and the mass public.

Whereas Robert Putnam emphasizes the role of social trust in fostering democracy, Skocpol points out that the creation and evolution of democratic regimes also is fostered by conflict and distrust. Research in this volume makes clear that scholars who use different theoretical perspectives may reach differing conclusions about how voluntary associations affect civic life, social capital formation, and the operations of political institutions.

A core issue in the social capital debate is whether the associational life of American communities, so central to Putnam’s argument, has increased or decreased over time or changed in other ways that significantly influence social capital formation and its effects on civic engagement. Several contributors address that issue. Focusing on the community level, Peter Dobkin Hill analyzes trends in the population of organizations from 1850 to 1998 in New Haven, Connecticut, and the implications of organizational change for patterns of civic engagement. Chapters by Skocpol and by Elisabeth Clemen indicate that women’s voluntary associations have had significant effects on social welfare policy. As Skocpol points out, this contrasts to Putnam’s conclusion that these associations had few effects on policies during the Progressive era. Clemen provides an historical analysis of the role of women’s groups in the transformation of American politics between 1890 and 1920. She reveals their role in political mobilization and shows how their structure and internal procedures affected both external perceptions of the organizations and patterns of interactions among them. Multiple models of organizations—an “organizational repertoire”—enabled challengers of the established political order to employ nonpolitical models of organization.

Jeffrey Berry addresses post-World War II patterns of citizen advocacy groups through an examination of their participation in congressional hearings on domestic social and economic policy as well as media coverage of group activity. His research suggests that growing membership in groups based in Washington reflects a shift from local voluntary organizations to national groups that focus on policy solutions at the national level. Because such membership often entails little or no activity other than writing a check, the creation of social capital may be weakened.

Several chapters evaluate long-term changes in American society and their consequences for voluntary associations and civic engagement. Steven Brint and Charles Levy consider cultural and organizational changes among professionals. Susan Crawford and Peggy Levitt use the Parent Teachers Association for a case study, Marcella Ridlen Ray discusses the effects of changes in communications technology for group formation, and Robert Wuthrow explores the effects of religious involvement on patterns of civic engagement. Skocpol examines change over time in the universe of voluntary associations, with a focus on the withering of national membership federations and the development and growth of advocacy groups arising from the social movements of the 1960s. She attributes alterations to changes in the political opportunity structure, new methods and models for building and maintaining organizations, shifts in social class relationships, and evolving race relations and gender roles.

An alternative approach for examining social capital formation is provided by Wendy Rahn, John Brehm, and Neil Carlson. They use survey data to examine how social capital may be generated through participation in a national election.

Patterns of civic engagement are generally assessed as a



positive contribution to society, but negative consequences may flow from political participation. Fiorina points out possible negative consequences from activism by extremists. Kay Schlozman, Sidney Verba, and Henry Brady examine inequalities in civic participation and consider the biases that these bring to the political system.

This book is recommended reading for scholars interested in the social capital controversy. It will stimulate further research on change over time in patterns of civic life and the consequences for social capital formation and civic engagement. Also, it illustrates the advantages of approaching research problems from a variety of theoretical perspectives.

**It's Our Military, Too!: Women and the U.S. Military.** Edited by Judith Hicks Stiehm. Philadelphia, PA: Temple University Press, 1996. 309p. \$69.95 cloth, \$22.95 paper.

Francine D'Amico, *SUNY Cortland and Syracuse University*

This anthology opens with several compelling first-person narratives of life inside the U.S. military institution, followed by analyses by both military and civilian researchers. The academic contributors approach their topics from different disciplines, including political science, history, sociology, and literature/film. The volume seeks to bridge the gap between those inside the institution and those on the outside (p. ix). The effort to build this bridge began with a series of specialized conferences and workshops on gender and military culture spanning the 1990s. I participated in several of these and heard some of the contributors to this collection present their research. Other volumes that developed from the conferences are Mary Fainsod Katzenstein's *Faithful and Fearless* (1998), Mary Fainsod Katzenstein and Judith Reppy, *Beyond Zero Tolerance* (1999), and Francine D'Amico and Laurie Weinstein, *Gender Camouflage* (1999).

*It's Our Military, Too!* is aimed at readers new to the study of U.S. military culture and policy. All save one of the chapters are original publications. In the opening chapter, Army Lt. Rhonda Cornum criticizes the U.S. media for its obsessive focus on her brief experience as a POW during the Gulf War; she prefers to assess that experience in the context of her 13-year military career (pp. 3–23). Although this contribution provides a compelling hook, especially for the student reader, questions raised in succeeding chapters about women's efforts to "fit" into a resistant institutional culture should prompt the reader to reassess Cornum's uncritical essay on military service. Using pseudonyms to protect their careers, two active duty officers critique the military's hetero/sexist culture, Virginia Solms through the frame of her experiences as a lesbian Army officer, and Billie Mitchell through an analysis of cadet training at West Point.

The second part of the volume focuses on history and contemporary policy issues. In a meticulously detailed history of military nursing from 1775 to the Vietnam War, retired Army officer Connie L. Reeves argues that military nurses' struggle for acceptance was the vanguard for women's entry to other military occupations. Sociologist Brenda Moore (also a veteran) uncovers the history of African American WAAC/WACs who served overseas during World War II and analyzes contemporary military race/gender relations, expanding upon her earlier work (*To Serve My Country, To Serve My Race*, 1998). Disappointingly, many of the other contributors neglect the race/gender dynamic Moore so brilliantly exposes.

Advanced readers and policy researchers will no doubt find the chapter on gender and weapons design, by Department of Defense program analyst Nina Richman-Loo and Cornell

researcher Rachel Weber, most intriguing. They analyze the role technology plays in facilitating or limiting women's military service through a case study of the Navy/Air Force Joint Primary Aircraft Training System (JPATS) and its proposed redesign of the next generation of aircraft cockpits (p. 136). Beginning students will appreciate the concise and organized brief on changing military mission and personnel models by sociologist and Vietnam veteran M. C. Devilbiss, but advanced readers should also benefit from her insights.

Chapters in the final section of the anthology consider how society's ideas about gender are reflected in and sustained by military traditions and popular representations of war. Carol Burke explores the "pernicious cohesion" sustained by gendered folk traditions at Annapolis, where she taught from 1984 to 1991. Susan Jeffords analyzes the telling of "the war story" in American popular culture, and Miriam Cooke examines gender and military paradigms through a case study of the photographic framing of the Lebanese civil war. Along with Billie Mitchell's chapter on West Point, these are the theoretically richest contributions in the volume.

This text will be useful for a range of introductory college courses on contemporary social and political issues and perhaps also for high school social studies courses that address citizenship. Three dozen black-and-white photographs illustrating nearly every chapter are sure to engage student attention. Chapter notes and bibliographies and a cumulative index provide useful tools for academic readers.

Unfortunately, the editor provides no substantive introduction to identify the different projects and perspectives of the contributors or to offer theoretical threads to help make connections across the chapters. This omission may have been intentional, so as not to force the very different contributions to bend to a predetermined framework, but more might have been done to prepare the reader for what lies ahead. For example, what central questions shape or inform the collection? The brief preface poses none. Stiehm merely decries civilian inattentiveness/deference to the military and challenges the civilian reader to accept responsibility "for what it is and what it does" (p. ix). She elaborates on this obligation in the final chapter, "The Civilian Mind," but does not engage the theoretical literature on democratic citizenship.

Also missing is a discussion of how this text fits with other literature on military history, sociology, and policy or gender theory/politics. The literature has grown so vast and diverse that perhaps such a review would have been superficial at best to cover the breadth of disciplines represented. Still, locating the text within the narrower field of military gender studies would have been useful not only for student readers but also for instructors considering the volume for course adoption.

The anthology is something of a hybrid, a cross between a textbook and a citizen handbook. A chapter on rank structure and insignia provides a laundry list of "facts" about the military institution. This may be useful information for beginning readers, but these "facts" require greater organization, contextualization, and discussion for that audience. For example, how does the persistence of four service branches with different uniforms, insignia, organizational structures, and cultural traditions affect gender integration? Would a restructuring such as occurred recently in the Canadian military remove some of the obstacles to women's participation that contributors describe? Student readers will need additional guidance to understand the unspoken connections among chapters and across the three sections of the book.

**The Movers and the Shirkers: Representatives and Ideologues in the Senate.** By Eric M. Uslander. Ann Arbor: University of Michigan Press, 1999. 218p. \$44.50.

William Bianco, *Pennsylvania State University*

*The Movers and the Shirkers* is a critique and extension of a well-cited and important research program: attempts to measure the degree to which legislators shirk, or advance their own policy goals at the expense of those held by their constituents. Such analyses (e.g., Joseph P. Kalt and Mark Zupan, "Capture and Ideology in the Economic Theory of Politics," *American Economic Review* 74 [June 1984]: 279–300; John R. Lott, "Political Cheating," *Public Choice* 52 [1987]: 169–86) typically assume a principal-agent relationship between constituents and elected representatives, and they specify a regression analysis with roll-call behavior as a left-hand side variable and various measures of constituency interests and legislator ideology as right-hand side variables. Previous work (John E. Jackson and John W. Kingdon, "Ideology, Interest Groups, and Legislative Votes," *American Journal of Political Science* 36 [August 1992]: 805–23) shows that these analyses are bedeviled by measurement and estimation issues. Eric Uslander highlights a more fundamental flaw: By ignoring important and well-understood mechanisms that tie legislators to their constituents, these analyses assume what should be tested.

Uslander's argument will ring true to anyone familiar with the political science literature on representation. Rather than emphasize regular elections as the only mechanism that compels incumbents to behave as their constituents demand, Uslander focuses on recruitment—the decision to run for office. Simply put, potential candidates who would face large incentives to shirk, because their policy concerns are cross-wise to those held by constituents, generally decide against running because they assume, correctly, that they would have little chance of winning. The result is that a sizable fraction of incumbents are well matched to their constituencies, so shirking is not an option. In the main, doing right by their constituents furthers their own policy concerns.

Uslander also argues for a more nuanced definition of the constituency in models of shirking. As he notes, it would be no surprise to find that when disagreements exist, legislators are more responsive to the preferences of their reelection constituency over the interests of their entire district. Thus, apparent shirking—a mismatch between a legislator's behavior and constituent interests broadly defined—may reflect the essence of democracy: incumbents who try to hold the support of the people who elected them.

The core of the book is a reanalysis of the Kalt-Zupan data on voting in the U.S. Senate. Uslander faces a classic dilemma: Use suspect data in order to facilitate a comparison with well-cited findings, or use better data and abandon comparability. In the main, he opts for comparability, which is a defensible choice, although my preference would have been to emphasize Poole-Rosenthal NOMINATE scores as an alternate measure of legislator ideology. Yet, Uslander moves beyond contemporary analyses of shirking by estimating a multiple-equation model of roll-call behavior, and he also exploits some additional sources, including a CBS poll of Senate incumbents.

Uslander finds little systematic shirking. When it does occur, it is more directed at satisfying reelection constituencies than at furthering an incumbent's policy goals at the expense of constituents. Moreover, the policy preferences of Senate incumbents are strongly related to those of the people who elected them—their geographic and reelection constituencies. The point is not that Senate incumbents are well

controlled; rather, to paraphrase John Kingdon, they just reflect where they came from.

Uslander's results do not preclude shirking on proposals of little interest to constituents, or isolated, idiosyncratic shirking that is masked by aggregate measures. What the book confirms is that contemporary analyses of shirking, by largely ignoring recruitment, overlook a critical mechanism that binds legislators to their electorate.

An additional strength of this book is the author's self-consciousness about method. The analysis chapters emphasize how Uslander constructed measures and arrived at specifications, as well as the effect of alternate variables and models. In this sense, the book would be ideal for discussions in a graduate methods class. Readers may disagree with some of Uslander's assumptions, but these areas of disagreement are easy to find.

In sum, *The Movers and the Shirkers* is an important correction to contemporary studies of representation. Uslander moves the debate away from expectations based on simplistic principal-agent models and toward a more realistic specification of the ties that bind incumbents to constituencies.

**Choosing Equality: School Choice, the Constitution, and Civil Society.** By Joseph P. Viteritti. Washington, DC: Brookings, 1999. 284p. \$29.95.

Jerome J. Hanus, *American University*

The contours of the school choice debate are by now familiar to public policy students, but a lack of agreement about the appropriate weights to be given to the variables affecting the subject continues to splinter their ranks. On the surface, there appears to be a consensus that the latest scores on standardized tests will resolve the uncertainty as to which type of education, public or private, is most effective, but a dip into the literature quickly dispels any such hope. The only thing clear is that nonpublic schools, even with one financial hand tied behind them, do not perform any more poorly than public ones. Consequently, Viteritti wisely gives short shrift to the byzantine methodological distinctions made by researchers and, instead, focuses on the normative questions.

Unlike most writers who favor school choice (e.g., Milton Friedman, *Capitalism and Freedom*, 1962) because it will contribute to an increased dimension of individual freedom, Viteritti justifies it because its outcome will increase equality among classes and races. In a comment reminiscent of John Rawls (*A Theory of Justice*, 1971), Viteritti argues that "education policy must be designed to benefit the most disadvantaged members of society—those who are underserved by the current system" (p. 211). He does not sharply distinguish between equality of opportunity and equality of result because he expects the former to evolve straightforwardly into the latter. If lower income families in the inner cities can choose to attend a school other than their local public one, then they will have a greater probability of attaining the same quality education as middle-class children. This would help fulfill *Brown v. Board of Education's* desire that all races receive not just an education, but a decent one.

Viteritti focuses on three particular issues in the school choice debate: whether lower income parents are competent to choose among schools, whether aid to religious schools is constitutionally permissible, and whether school choice can contribute to greater equality in America. He quickly dispatches the first issue by recounting the experiences with school choice (both public and private) in Harlem, Milwaukee, Cleveland, and several other cities. In all of them,

despite onerous legal restrictions imposed by the state, substantial numbers of low-income parents did make a conscious decision to opt for a charter, magnet, or private school. Of course, most simply stayed in the local public school, either because they were satisfied with it or, perhaps, because they lacked the initiative to leave. In either case, experience has disproved the charge that low-income parents are incapable of distinguishing among schools or are unconcerned with the education of their children.

The constitutional question is more complex because it involves both federal and state constitutions and because the Supreme Court has unnecessarily complicated the issue. Viteritti rightly zeroes in on *Everson v. Board of Education* as the culprit that distorted the legal and social history of religion in the United States and introduced thirty years of textual incoherence into the law of church-state relations. By defining religion to include both “religion and non-religion,” the Court left itself open to the charge that when it required that government aid must support a secular (i.e., nonreligious) purpose it was promoting a secularist religion over sectarian religion. Beginning in the 1970s, the Court has taken a more accommodationist position, but it has done so more on public policy grounds rather than on constitutional principle, thus resting the law on the current political orientation of its members.

An important contribution to the church-state question is Viteritti’s analysis of the stringent state “Blaine amendments.” He does a public service in describing their anti-Catholic origins, direct and indirect, and points out that if the Court were logical, it would strike these down because they violate free exercise of religion by reason of their unconstitutional intent. To see the import of his reasoning, one need only compare the Court’s decisions to strike down laws intended to discriminate against racial minorities with those intended to discriminate against religious minorities.

The third issue in the school choice debate, and the one that most interests Viteritti, is the opportunity it offers low-income pupils to participate effectively in America’s dedication to providing a decent education for all. Observing America’s commitment to freedom of choice in so many facets of life, Viteritti finds it anomalous that the core government institution—education—is the least pluralistic and the least likely to acknowledge the importance of religion in people’s lives. This is even more the case with regard to racial minorities, because black churches have historically played such a crucial role in unifying their members and offering leadership for social advancement. As he observes, “the church is a special source of strength in poor communities” (p. 208). For these groups, an education devoid of a proper concern for their religious heritage and respect for their religious values and teachings will not provide these children with the sense of empowerment necessary for alleviating their political inequality. Thus, Viteritti draws together the elements of democratic freedom of choice, religious instruction, and social (and spiritual) capital to justify offering a plurality of educational options to the poor.

An argument for equal government funding for poor children to attend any kind of accredited school—public, charter, magnet, private, religious—has much to be said for it, and Viteritti deals effectively with the most frequently raised objections. Nevertheless, his philosophical commitment to equality does give rise to some misgiving. Education in America has ideally been assumed to be directed toward the individual student, not racial, social, or income groups. Critics of school choice often object to it because “the rich” might receive a government subsidy if they opt for a private school. Despite the fact that most private school pupils come

from middle- and lower-middle-class families, Viteritti is inclined to accept this form of class analysis. Instead of offering a justification for increasing freedom for all parents, he would restrict it to the currently poor and, presumably, would take it away from those who manage to work their way into higher income levels. Thus, Viteritti’s argument for school choice will still result in a certain amount of exclusiveness and bureaucratic entanglement, as the state must determine each year who is and who is not still poor enough to qualify for school choice. Because Milton Friedman avoids this rather distasteful prospect by including all parents in a policy of school choice, he provides a much more compelling intellectual foundation for creating a nation of thriving educational communities.

**The Politics of the Minimum Wage.** By Jerold Waltman. Urbana: University of Illinois Press, 2000. 172p. \$24.95.

Byron W. Daynes, *Brigham Young University*

Upon receiving Jerold Waltman’s book, the first thing I read was the dedication to the author’s “coworkers at A. & W. Root Beer, Ruston, Louisiana, 1963–67.” I like root beer, but I wondered whether a mistake had been made in sending me this book to review. It did not take long to realize no mistake had been made. This is a book that treats minimum wage as symbolic politics, but, more important, it entirely reexamines public policy.

Waltman begins by crediting Theodore J. Lowi with originating public policy research by identifying four basic policy types—distributive, regulatory, redistributive, and constituent policy—and alluding to the distinctive political process that attends each (Theodore J. Lowi, “American Business, Public Policy, Case Studies, and Political Theory,” *World Politics* 16 [July 1964]: 677–715, and “Four Systems of Policy, Politics, and Choice,” *Public Administration Review* 32 [July/August 1972]: 298–310). It soon becomes clear, however, that Waltman is disturbed by Lowi’s definition of public policy, which views coercion as the core identifying ingredient (Theodore J. Lowi, “Foreword: New Dimensions in Policy and Politics,” in Raymond Tatalovich and Byron W. Daynes, eds., *Social Regulatory Policy: Moral Controversies in American Politics*, 1988, p. x).

Waltman believes that a “more interesting theory can be generated” if public policy is defined as “any statute or administrative act adopted by the appropriate legal authority” (p. 4). This definition allows Waltman to separate policy into coercive and noncoercive categories. The first encompasses distributive, regulatory, and redistributive policies, and the second includes Lowi’s “constituent” policies, James L. Anderson’s “suasive” policies (“Governmental Suasion: Refocusing the Lowi Policy Typology,” *Policy Studies Journal* 25 [Summer 1997]: 266–82), and Waltman’s own “symbolic” policies (p. 4). Thus, coercion becomes the organizing variable Waltman uses to construct an entirely new definition of policy, and his revised typology is a primary contribution to the literature.

The remainder of the book explores all possible perspectives of minimum wage as symbolic politics: ideological roots, political history, public support, sociology, place in the policy process, and economic consequences. In chapter 1, Waltman looks at the roots of minimum wage in Progressivism and at the ideological cleavages it has caused. The political history examined in chapter 2, which begins with the New Deal and ends with 1996, shows convincingly how much more sympathetic Democratic administrations have been to the issue than have Republican administrations. Waltman argues that

the Supreme Court and Congress have vacillated over time in their support of minimum wage.

In chapters 3 and 4, the author reveals the importance of public support to the success of minimum wage politics, even though public opinion has been ambivalent over the years. Also, unlike some other issues, minimum wage appears constituency specific, of interest particularly among women, African Americans, Hispanics, and young people.

In the chapter on policymaking (chap. 5), Waltman introduces a new agenda-setting model, characterized by “instigators,” who introduce policy, and the more visible policy “enablers,” who facilitate the policy process. Since public support for minimum wage is so uncertain, the issue does not always make it to the formal policy agenda but remains viable enough to secure a place on the less visible political agenda (p. 107).

Waltman does not see minimum wage as having serious economic consequences on employment, inflation, business failures, or the reduction of poverty, but such concerns remain significant enough to influence debate.

In his concluding section, which ought to have appeared as a separate treatise, Waltman attempts to link the importance of work to citizenship in a discussion of what he calls “civil republicanism” (pp. 135–45). This was not a satisfying conclusion for an otherwise thought-provoking treatment of policy theory. Of course, this evaluation could be due to my own preferences for policy theory.

Waltman’s argument forces us to choose between his own policy perspective and Lowi’s framework. His treatment goes far beyond my own experience with the Lowi policy scheme. When Ray Tatalovich and I proposed that social regulatory policy should be a distinct policy type, Lowi labeled our attempt to add a fifth category “valiant” but indicated he preferred to subsume our efforts into his scheme, creating an enhanced version of his model that would still preserve the “simplicity and, more importantly, the logic of the analysis” (Tatalovich and Daynes, eds., *Social Regulatory Policy*, p. xii).

In contrast, Waltman alters the way of looking at policy, as he both rejects Lowi’s definition of policy and disassembles his fourfold scheme, even in its more elaborate format. I would like more extensive evidence as to why the rejection of Lowi and the acceptance of Waltman would enhance my understanding of policy. Although Waltman’s argument is fairly tightly drawn, it is ultimately less convincing than Lowi’s assertions in support of his framework because Waltman does not fully explain how an economic regulatory policy such as minimum wage can be totally divested of all its coercive elements, despite the symbolism that defines responses to it (p. 1). But even if one does not subscribe to Waltman’s ambitious policy revision, the questions he asked are important.

**Checkbook Democracy: How Money Corrupts Political Campaigns.** By Darrell M. West. Boston, MA: Northeastern University Press, 2000. 220p. \$47.50 cloth, \$16.95 paper.

David A. Breaux, *Mississippi State University*

Darrell West argues that elections, rather than stimulate debate over ideas and policies, have degenerated into contests in which candidates try to raise more and more money. The cost of campaigns has skyrocketed, the level of cynicism and apathy among voters has increased, and the perceived accountability of elected officials has been placed at risk.

*Checkbook Democracy* begins with a discussion of how the abuses of Watergate helped spawn the Federal Election Campaign Act of 1974. Its key features were strong disclosure

laws designed to end secrecy in fundraising; caps on contributions at \$1,000 per individual and \$5,000 per political action committee; voluntary spending limits for presidential races in exchange for public subsidies; and the creation of a new federal agency, the Federal Election Commission (FEC), which was charged with enforcing the new law. As a result of these provisions, the ability of wealthy individuals to fund individual candidates was sharply curtailed. In the subsequent chapters, West provides several case studies that illustrate how the quest for more and more money has led candidates, political parties, and interest group organizations to take advantage of loopholes in campaign finance laws.

Chapter 2 examines the independent expenditures that funded the Willie Horton advertisement during the 1988 presidential contest. Although the FEC collected extensive evidence that documented the Bush campaign’s role, the FEC lawyers were not able to persuade the courts. This set the tone for the flood of independent expenditures in subsequent elections. Chapter 3 focuses on the rise of issue advocacy and details how the Christian Action Network spent \$2 million on advertisements to defeat Bill Clinton, whom they saw as undermining traditional family values by advocating “radical homosexual rights.”

Chapter 4 looks at what West calls the “schizophrenia” of current campaign finance rules. The focus is on the case of Simon Fireman. He and his company, Aqua-Leisure, were fined \$6 million for making contributions above the \$1,000 limit to Republican presidential candidate Bob Dole. At the same time, individuals, unions, and corporations donated hundreds of thousands of dollars to political parties, but because these funds were not given directly to candidates, they were exempt from campaign finance restrictions.

Chapter 5 examines the role of donations from foreign nations to the Democratic National Committee in 1996. This was an embarrassing scandal for the Democratic Party and raised serious issues of national security. Chapter 6, which illustrates how powerful groups can help candidates evade campaign finance rules, looks at the role of the Teamsters Union in Clinton’s 1996 reelection campaign.

Chapter 7 examines how tobacco companies helped defeat policy proposals that had the overwhelming support of the American public by waging a \$43 million lobbying campaign and making large donations to the Republican National Committee. Chapter 8 explores how candidates rely on nonprofit, tax-exempt organizations to help them evade campaign finance law. These organizations are not subject to disclosure requirements or contribution limits and can accept money from anyone who desires to contribute.

The concluding chapter notes the various forces working against serious campaign finance reform. Thirteen reforms are suggested (including on-line weekly disclosure reports, a ban on soft money, an increase in contribution limits, and restructuring of the FEC) that would bring about open, fair, and equitable elections.

*Checkbook Democracy* is a lively read because of anecdotal information on specific instances of campaign finance abuses, but the effect of money on campaigns and elections is an extremely complex issue, and West does not provide a rigorous analysis. His claim that the increase in voter cynicism and apathy is a product of campaign finance abuses is not supported by evidence and therefore is mere speculation. Although West puts forth some intriguing prescriptions for campaign finance reform, his suggestions are not only untested but also politically unviable.

This is an easy-to-read book concerning specific abuses of the campaign finance system and how these undermine the electoral process. For more than anecdotal information,

however, one needs to look elsewhere, such as David Magleby, ed., *Outside Money: Soft Money and Issue Advocacy in the 1998 Congressional Elections* (1999). The contributors to *Outside Money* employ a common method that allows them to present and analyze hard data on actual spending by parties and groups.

**The Market Approach to Education: An Analysis of America's First Voucher Program.** By John F. Witte. Princeton, NJ: Princeton University Press, 2000. 221p. \$29.95.

Michael Mintrom, *Michigan State University*

John F. Witte has been prominent in the academic discourse surrounding market-like reforms of public education. In the two-volume *Choice and Control in American Education* (1990), Witte and his coeditor, William S. Clune, assembled an impressive collection of analytical essays and literature reviews that highlighted relevant theoretical concerns and showcased the insights from practice available at the time. In September 1990, Witte was appointed the state's evaluator of the pioneering Milwaukee, Wisconsin, voucher program. From 1991 through 1995, he and his collaborators produced annual reports analyzing key aspects of the program. The salience of school choice as a policy idea and the political interests at stake inevitably meant that all eyes were on Milwaukee, and the question in everyone's mind was "does it work"?

Although Witte found much to be commended about the program, he uncovered little evidence that voucher students in private schools were doing better with respect to test score gains than a control group in the city's public schools. Others disputed those findings. In particular, in a reanalysis of Witte's data, Paul E. Peterson and colleagues suggested that, when compared to a theoretically more appropriate control group, voucher students were making significant gains. The Peterson study went to unusual lengths to discredit Witte's work, and what might have been a productive scholarly exchange turned sour. Witte published many scholarly articles on school choice issues throughout the 1990s, typically grounding his arguments in evidence he had gathered in Milwaukee.

*The Market Approach to Education* is the capstone of Witte's effort to make sense of Milwaukee's voucher program, not only its internal dynamics and local outcomes but also, and more important, its broader political significance. The result is impressive. This is undoubtedly the best analysis of the details of school choice and its social implications since Jeffrey R. Henig's *Rethinking School Choice: Limits of the Market Metaphor* (1994). Both books are essential reading for those interested in this issue. Witte's book is fascinating for two reasons. First, as one would expect, it provides many vital insights into the major debate taking place over the future of public education in this country. Second, it raises some serious questions about the relationship among political science, policy analysis, and political advocacy.

Chapters 1 and 2 provide an introduction to the study and to the enduring controversy over educational choice. Witte returns to the broader politics of school choice in chapters 7 and 8, and he covers a range of key issues. Chapter 3 provides background on Milwaukee's public schools and creation of the voucher program. In chapter 4, Witte discusses participation in the voucher program, which was deliberately targeted at low-income families; the choice students came from poor, predominantly minority, and mostly single-parent female-headed households. Compared with other low-income families in Milwaukee, those of choice students were typically

smaller, and the parents (especially mothers) were more educated, were more involved in schooling before coming into the program, and had higher educational expectations for their children. Witte argues that participation rules matter greatly in determining who enters voucher programs. Furthermore, based on analysis of admissions to voucher schools and how they compare with admissions in other (public and private) choice programs in Milwaukee, Witte concludes: "Absent explicit restrictions, schools will be as selective as permitted" in determining which students they will admit (p. 81). The implication is that, when asking "who chooses?" under school choice, we should pay attention to the schools as well as the families. If unchecked, the actions of both can exacerbate social stratification.

In chapter 5, Witte discusses the handful of private schools involved in the voucher program, noting both their successes and their failures (several schools closed during the period of the study). Witte emphasizes the freedom of private schools to devise their own programs and the variety that results, although he also notes that many of the practices in these schools seem to be similar to those found in traditional public schools.

The issue of student performance outcomes is addressed in chapter 6. Witte points out that this is only part of what should interest us when thinking about the outcomes of voucher programs. He also acknowledges—as would most analysts—that student test scores are a frustratingly narrow measure of achievement. Nonetheless, these are the performance outcomes most easily quantified and hence most readily amenable to analysis using standard forms of social scientific hypothesis testing. Witte presents his original approach to determining the comparative test score performance gains of students in choice schools and traditional schools; he also replicates the analyses of others who have considered the Milwaukee data. As in the past, Witte's analysis indicates that the choice students showed no significant, systematic improvement in performance compared with nonchoice students, no matter how the control group is chosen. The analysis is thorough and persuasive. It is also fair to conclude that the analysis of test score data associated with the Milwaukee program and other voucher programs has benefited from the Witte and Peterson controversy (even though its tone was unfortunate). There is much in this chapter of interest to anyone concerned with selection bias problems and how these might be handled when modeling complex social processes.

In several places Witte expresses concern that reasoned discussion about the future of American education is being crowded out by "choice theater." "What is implied is that the images, symbols, characters, and dramatic flourishes are just as or more important than legislative and administrative details, votes, or budgetary considerations. They certainly are more important than policy evaluations, especially when such evaluations provide complex and mixed findings" (p. 173). Witte believes that few evaluations of school choice programs will be commissioned by policymakers in the future. (Incredibly, all data collection for the Milwaukee program ceased in 1995.) For politicians, the operative question is: Why fund evaluations of your pet programs when the findings will be used against them? For Witte, who comes from the positivist tradition in political science, this apparent negation of objective policy analysis is troubling. It is also enormously troubling for what it implies about the value (or lack of it) that some contemporary politicians accord to informed political debate.

For political scientists interested in policy questions (and, in one way or another, that means most of us), Witte's

broader discussion of school choice politics invites reflection on our own practices. On the one hand, scholars must seek to produce work that seriously advances the discipline's knowledge of politics in general. On the other hand, scholars often have knowledge and skills that can be usefully employed to cast new light on contemporary social phenomena and, perhaps, contribute to practical problem solving. In practice, these two roles are often inextricable, especially for those who seek to illuminate aspects of politics in general through their study of contemporary concerns. With respect to the rise of school choice, the work of political scientists has not been inconsequential for shaping the terms of the debate. The best of this work also has made important contributions to disciplinary knowledge. But still we must ask: Whose interests are advanced when we choose our research topics and publish our findings? And when do we become complicit with the "theater"? Increasingly, questions of this sort are being raised within the discipline, and they deserve to be asked more often.

In *The Market Approach to Education*, Witte draws our attention to the ways that voucher programs (whether pub-

licly or privately funded) have been established with the immediate purpose of providing an escape for poor students from woefully inadequate urban public schools. Yet, he notes that the broader intention of many voucher advocates is to secure subsidies for middle-class families who desire to send their children to private schools or who do so now at considerable cost. If this is the goal of voucher advocates, then those seeking a lifeline from poor urban schools can kiss goodbye to the promised land.

Witte also goes to some length to demonstrate how private foundations and business interests have worked in the background to support a variety of school choice efforts, setting agendas through their quiet power. To the extent that scholars play into these private agendas while masking themselves as objective social scientists, they become complicit with the "theater." Does this promote the discipline? Witte's efforts to reflect on such questions are laudable. They make this book highly relevant and recommended reading for all political scientists, not just those who care about education policy and the details of voucher programs.

## Comparative Politics

**Prosecuting War Crimes and Genocide: The Twentieth Century Experience.** By Howard Ball. Lawrence: University of Kansas Press, 1999. 288p. \$35.00.

Richard Falk, *Princeton University*

There is little doubt that an abiding feature of international relations in the current period is the struggle to extend the rule of law to crimes of state. The 1998 detention in Britain of former Chilean dictator Augusto Pinochet, in response to a request for extradition issued by a Spanish judge, gave prominence to this quest. This development was further reinforced by the campaign to establish a permanent international criminal court, which eventuated in a treaty signed in Rome by about 120 countries two years ago and is on its way to securing the 60 ratifications needed to bring it into force. Organized international society is far from the end of this journey; powerful governments, including our own, are not ready to submit their citizens or leaders to international procedures of accountability.

It is against such a background that Howard Ball's carefully researched *Prosecuting War Crimes and Genocide* needs to be considered. Ball does an excellent job of narrating the main steps taken over the last several decades, especially since the end of World War II, in relation to wartime criminality, genocide, and severe violations of human rights. Separate chapters discuss the Nuremberg experience and the trials held in Tokyo to assess the responsibility of Japanese military and civilian leaders for atrocities committed during their period of rule. There are also separate chapters on each of the three major genocidal experiences of the last part of the twentieth century: Cambodia, the Balkans, and Rwanda. The book concludes with chapters on the Rome treaty as a belated fulfillment of the Nuremberg legacy and on an assessment of the obstacles that remain, especially the unwillingness of the United States to commit itself to the accountability that it did so much to bring about.

Ball gives an excellent and generally reliable overview of this complex and controversial story. His concluding words indicate his belief that a positive trend is under way: "The

world community's quest for an effective enforcement response to these horrendous crimes will be realized sooner than later and . . . the twenty-first century will be the initial millennium where justice for victims of human rights abuses, genocide, torture, war crimes, and crimes against humanity becomes the norm rather than the exception" (p. 240). Ball poses the underlying issue of policy as a collision between "realpolitik" and "solemn cries for utopian justice" (p. 6). He makes no secret of his own predisposition: "The culture of impunity must end" (p. 6). The impunity generally enjoyed by perpetrators of such terrible acts deprives victims and survivors of "justice" (p. 10) and serves as the most dramatic indication of the extent to which the realities of international relations and domestic governance remain games of power.

Most tellingly on this issue for Ball was the cynical response of the United States to the Cambodian ordeal perpetrated by the Pol Pot regime in the late 1970s. Ball notes that the Carter administration, with special prodding from National Security Advisor Zbigniew Brzezinski, gave strong diplomatic and indirect economic (and even military) support to the Khmer Rouge. The U.S. government gave priority to opposing the Vietnamese invasion that toppled the genocidal regime and to playing "the China card" against the Soviet Union. Such a blatant embrace of geopolitics by a liberal American president who had gained world notoriety for his support of a human rights diplomacy should serve as a wakeup call for those who claim that the United States exercises moral leadership in world affairs. Belatedly, the Clinton administration, despite Pol Pot's recent death, has called for the creation of a special international criminal tribunal mandated to bring to justice those remnants of the Khmer Rouge that survive. So far, the Cambodian government has resisted the call, suggesting its own suspect willingness to proceed. Ball tells this complex story without much editorializing, although he does rely upon the Cambodian case to make his central argument that realpolitik contributes greatly to impunity for notorious perpetrators of crimes of state.

The weaknesses of Ball's book relate to its lack of analytic depth. Because the difficult theoretical issues are addressed rather superficially, the book seems more suitable for the

general reader or undergraduate than for an advanced student of this subject. The narration does provide a coherent account of the international efforts to overcome impunity through prosecution and punishment, but it does not explain very well why the glass is even half full, or more tellingly, why it is only half full. Ball does not recount at all the saga of heroic individuals of conscience who kept the Nuremberg idea alive during the long period of intergovernmental silence of almost fifty years. This silence started almost immediately after the Nuremberg experience and lasted until the tribunal dealing with the crimes associated with the breakup of Yugoslavia was established in 1993. He does not mention the important commentary on accountability that occurred during the Vietnam War when Telford Taylor, a prosecutor at Nuremberg, wrote a widely discussed book, *Nuremberg and Vietnam: An American Tragedy* (1970). In other words, the role of civil society in the struggle against impunity is virtually ignored. As a result, there is an inadequate presentation of the successful collaboration between a coalition of 200 or more NGOs and a series of moderate governments that eventuated in the Rome treaty, despite the objections by such powerhouses as the United States and China. If an effective international criminal court ever sees daylight, it will be largely the result of the continuing transnational pressure mounted by civil society organizations and dedicated citizens; although their role is briefly acknowledged by Ball (pp. 196–8), its political relevance is not sufficiently assessed.

There is also little insight into both the willingness to embark on the path of international accountability in the first place and the subsequent reluctance to proceed farther. On the one side, there were the peculiar circumstances that existed in 1945, including the guilt of the victorious liberal democracies that they had turned such a blind eye to Nazi policies so long as these were directed internally. Furthermore, there was a widely shared sense that the outcome of World War II was a victory in “a just war,” which made it appropriate to punish the unjust side so as to reinforce the political importance of defeating fascism. There was, as well, the pedagogic impulse to tell the full story of Nazi and Japanese wrongdoing both as a way to acknowledge past victims and to build support for a more humane politics in the future. On the other side, from the outset, was the problem of “dirty hands,” the extent to which the Allies had themselves waged the war in a manner difficult to reconcile with the laws of war, especially the strategic bombing campaigns against German and Japanese cities and the use of atomic bombs against Hiroshima and Nagasaki. In the Cold War era, the bipolar structure of the world order and the recourse by both sides to interventionary wars created a mutual vulnerability to charges of Nuremberg-like criminality. Quietly, without public discourse, major states abandoned the pursuit of accountability for crimes of state.

Ball also does not give much insight as to why the moves against impunity were revived in the 1990s, and he does not give the Latin American experience of transition to democracy the attention it deserves. It was the fall of military dictators in leading countries in Latin America more than a decade ago that revived the sense that crimes committed by a government need to be acknowledged, even if domestic conditions do not permit the full imposition of accountability on the perpetrators. The UN creation of tribunals to address the crimes emanating from the breakup of Yugoslavia and the genocide in Rwanda in the 1990s reflected a special set of circumstances that centered on the ambivalence of the United States and its allies, especially their own unwillingness to take timely risks to protect the victims. Refocusing attention on criminality seemed without risk and usefully diver-

sionary, but at the same time responsive to increasing public and media pressures to enforce international law against its most willful violators. At the same time, the reluctance to pursue high-profile indicted war criminals, such as Karadzic and Mladic in Bosnia, discloses again the degree to which political opportunism (in this case the fear of retaliatory casualties among the peacekeepers) leads to ill-deserved impunity.

All in all, Ball has contributed a useful, if limited, book to the growing literature on this subject. It does not have the scholarly originality or conceptual elegance of either Martha Minow's fine *Between Vengeance and Forgiveness: Facing History after Genocide and Mass Violence* (1998) or Gary Jonathan Bass's challenging *To Stay the Hand of Vengeance: The Politics of War Crimes Tribunals* (2000). At the same time, *Prosecuting War Crimes and Genocide* not only tells the main story in a generally helpful and accurate way but also engages itself in the “good fight” for criminal accountability as an integral dimension of global humane governance.

**Warriors in Politics: Hindu Nationalism, Violence, and the Shiv Sena in India.** By Sikata Banerjee. Boulder, CO: Westview Press, 2000. 207p. \$62.00.

**Ethnicity and Populist Mobilization: Political Parties, Citizens, and Democracy in South India.** By Narendra Subramanian. Delhi: Oxford University Press, 1999. 371p. \$59.95.

Sanjib Baruah, *Bard College*

These two books are about two powerful regional political forces in India—the Shiv Sena of Maharashtra (with a focus on the city of Mumbai) and the Dravidianist parties of Tamil Nadu. Many readers of this journal may know these places by their older names: Mumbai is Bombay, and the state of Tamil Nadu and its capital city were once known as Madras.

Both books, not coincidentally, have much to say about the rise of Hindu nationalism in India, which is perhaps the most dramatic change in the Indian political landscape in recent years. That, indeed, is the central theme of Banerjee's book, which investigates the Hindu-Muslim riots in Mumbai in 1993. Banerjee argues that the politics of Hindu nationalism provides the context for the riots. In Mumbai, the major political force articulating a Hindu nationalist agenda is the Shiv Sena (literally, the warriors of Shivaji, a legendary Maharastrian Hindu hero).

In Tamil Nadu, by contrast, Hindu nationalist political forces have not fared well. According to Subramanian, it is largely because the Dravidianist parties occupy that ideological and political space. In his words, parties that appeal to caste and language in Tamil Nadu have “inhibited the growth of other visions of community” (p. 32). The idea of Dravidianism comes from the fact that languages in southern India are of the Dravidian family, whereas most in northern India are of the Indo-European family. The ideologues of Dravidianism consider non-Brahmin Tamil speakers—indeed, speakers of all southern India languages—to be descendants of a Dravidian race.

Subramanian's theoretical framing helps bring out the broader implications of both books. The official nationalism of many postcolonial societies, he writes, emphasizes secularism and seeks to make the state a neutral arbiter among competing religious and ethnic groups. The goal is to create inclusive political communities, but the problem is that by keeping the cultural component “thin”—because emphasizing particular cultural symbols would seem exclusionary to

some—secular nationalism has also proven to be a rather fragile foundation for nation-building projects.

In India that project, it now seems, did not strike deep roots in many regions. Secularist pan-Indian political parties are losing ground to parties that employ “culturally thicker appeals to community” (p. 323). But at what cost? The two books together throw light on the good, bad, and ugly sides of the politics of ethnic and religious political mobilization in India.

Subramanian makes creative use of the notion of populism to elaborate the complex story of the ethnic mobilization by Dravidianist parties. In many situations ethnic appeals work because they are mixed with populist notions of “a common people,” as opposed to a privileged elite (p. 8). This can heighten ethnic antagonisms in some situations, as occurred in Sri Lanka and could have occurred in Tamil Nadu, but it also can temper ethnic hostilities. In Tamil Nadu, he argues, the populist turn of Dravidianist parties contained the exclusionary and violence-unleashing potential of Dravidianist discourse. Rather than police ethnic boundaries, Dravidianist parties celebrated mass culture and created entitlements for less privileged groups.

The ascent of Dravidianist parties in Tamil Nadu has been accompanied by a remarkable afflorescence of associational life. Even though these associations were formed under the auspices of the Dravidianist political leadership, there was significant room for pluralism and for competing political projects within the Dravidianist framework. Important differences arose over time that led to splits in the party and to leadership change, which accompanied shifts in the party’s platforms and mobilizational styles. Subramanian’s interesting analytical distinctions between assertive populism and paternalistic populism, for instance, roughly coincide with what are now two distinct parties—the Dravida Munnetra Kazhagam (DMK) and the All India Anna Dravida Munnetra Kazhagam (AIDMK).

The “All India” was added to the AIDMK’s name in 1976 to emphasize its acceptance of pan-Indianism. Dravidianism by then had come a long way. In the early stages it advocated secession, a platform it abandoned in the 1960s. This is remarkable in view of the havoc that secessionist politics have caused in Kashmir, Punjab, and the northeastern states. Subramanian gives the credit to the Dravidianist parties, not to the supposed capacity of the pan-Indian political system to accommodate diversity and ethnic dissent. Although populist Dravidianism is in many ways one of Indian democracy’s success stories, Subramanian does not ignore its occasional authoritarian propensities. The AIDMK, for instance, has attacked trade unions, has been intolerant of oppositional newspapers, and has showed more than a fleeting fondness for repressive laws.

Subramanian combines interesting theoretical insights, drawn from a wide range of readings, with painstaking empirical research. He interviewed hundreds of local leaders in five carefully selected electoral constituencies of the state assembly. The book is almost a dazzling piece of scholarship. Certainly, it is the most important work to date on the politics of Dravidianism and is an original contribution to the study of Indian politics.

One misses in Banerjee’s book a sense of what goes on inside the Shiv Sena organization. For instance, there is reference to one crucial ideological turn, but one does not fully understand why it happened. From being a party of Maharashtra regionalism in the late 1960s and 1970s, the Shiv Sena became a party of Hindu nationalism. Banerjee refers to the earlier period, when the Sena clashed with Hindu migrants from southern India and Uttar Pradesh over

access to jobs. Since 1984, she suggests, “because of an alliance with the more nationally based BJP” (the Bharatiya Janata Party), the Sena has “broadened its Maharashtra Hindu vision to include a pan-Hindu focus” (p. 41). Most readers would like to know more, however.

The strength of Banerjee’s book is the focus on the structural factors that contributed to the Mumbai riots of 1993. Most notable is the role the author assigns to the failures of the Bombay textile workers strike in 1983. Historically, the hundreds of textile mills and the ancillary industries—machinery, dyes, chemicals, and the marketing and transportation of textiles—had provided employment to a very large part of Mumbai’s population. The strike hastened the closure of many mills and permanently displaced millions of workers in these industries.

This economic devastation, according to Banerjee, provided the backdrop for the Shiv Sena’s aggressive politics of Hindu nationalism. The 1983 strike shifted textile production to the powerloom sector in small towns. Powerlooms use less complex technology and are owned by small-scale entrepreneurs who employ very few workers. Thousands of workers who were making a living wage in large industries—albeit only enough to live in the slums of one of the world’s most expensive cities—now had to make do with less pay in the less predictable world of the informal sector. Furthermore, there were somewhat differential effects on Hindus and Muslims. Many workers in the textile mills were Hindus, but the powerlooms in small towns mostly employed Muslims. Many Hindus, Banerjee suggests, turned to the Shiv Sena. She points out that slums like Jogeshwari, which saw the worst violence in 1993, were home to a very large number of displaced mill workers.

The relationship Banerjee posits between the decline of Mumbai’s textile industry and the Hindu-Muslim riots is complex. The decline of this key industry, along with a stagnant labor market and the manifest erosion in the Congress Party’s capacity to govern, “conspired to create a dawning awareness among many voters that a viable political alternative was necessary” (p. 111). The Shiv Sena filled that void. Despite this complex formulation, to explain riots one has to take another crucial step. As her theoretical point of departure, Banerjee searches for an alternative to the either-or choice between primordialist and rational choice explanations. To her credit she emphasizes context: the “economic and political crisis that creates a sense of anxiety” (p. 5). But it seems to me that one must take into account the logic of crowd behavior—“the heightened psychic states and convulsive behavioral impulses”—to which anthropologist Stanley J. Tambiah has drawn attention (*Leveling Crowds: Ethnonationalist Conflicts and Collective Violence in South Asia*, 1996, p. 266).

Given Banerjee’s lucid criticism of binary categories, it is surprising that in drawing out the implications of her study for Indian politics she gives such primacy to the distinction between ethnic and cultural nationalism. She qualifies these categories significantly, to be sure, but she describes India as standing at the crossroads of a “struggle between cultural and civic nationalism.” The outcome will determine whether “constitutional guarantees of civil liberties and protection of minorities enshrined in its Constitution” will survive (pp. 175–6).

Which way will India go? The optimists would turn to Subramanian’s thesis that “culturally rooted, yet tolerant, visions of community” not only can emerge in Indian politics but also can acquire hegemony. At India’s present political juncture it is hard to disagree with him that such forces are



more likely to curb intolerance than a “culturally vacuous” notion of Indian citizenship (p. 326).

**Incentives and Institutions: The Transition to a Market Economy in Russia.** By Serguey Braguinsky and Grigory Yavlinsky. Princeton, NJ: Princeton University Press, 2000. 282p. \$39.50.

Stephen Wegren, *Southern Methodist University*

After a decade of market reforms, it is obvious to most observers that something has gone wrong in Russia. Some economic indicators suggest modest successes in instituting a market economy, but most of the evidence is clear that the economic and social crisis has far exceeded what might be expected in terms of a “transformation recession.” Industrial and agricultural output is down substantially, capital renewal is not keeping pace with the retirement of old machinery, the nation is deindustrializing, real living standards have plummeted, and investment capital is fleeing the country. The authors of this book conclude that “the Russian privatization program was nothing but a grandiose failure” (p. 6).

In the political realm, new institutions have been used to benefit a few vested interests. Elections are competitive, but electoral rules are routinely violated to benefit incumbents. Russia did not choose democracy but stumbled into it. The notion of a benevolent government is far from Russian reality. The authors assert that “if Russian authorities could afford to continue the totalitarian rule, they would most definitely do so” (p. 204). Overall, the political institutions being constructed in Russia are unlikely to lead to a Western-style political system based upon democratic political values.

The purpose of this book is to examine the causes of these outcomes. The answer, according to the authors, is that “the structure of incentives built into the current transition environment is leading to the consolidation of a system that is almost as remote from a free market economy and democratic state as the previous communist system was” (p. 10). Arguing that post-Soviet institutions were built on the foundation of Soviet institutions (not from scratch), the authors examine how old institutions were used by carry-over elites. They also examine how new institutions were manipulated and created dysfunctional incentive structures.

An important aspect of the argument is the attention drawn to incentives. Western lending agencies and advisors concentrated heavily on creating (duplicating) Western-type institutions in Russia with the rather naive expectation that the construction of incentives would or could, by themselves, lead to desired policy outcomes. This view was naive because it ignored both the cultural context in which these institutions operated and the incentive structures derived from them. If outcomes did not meet expectations, the putative reason was “reform was not implemented” or “the state is too weak.” Few policymakers or specialists understood that the real reason lay in the way actors responded to the incentive structures they confronted in the new institutional configuration.

Any reader, whether or not specialist or policymaker, will benefit from this book, but three principal shortcomings deserve mention. First, it is argued that the “wrong” incentives emerged from Western-style institutions, which implies that institutions caused these incentives, which in turn led to behaviors that undermined the logic and intent of reform. But why? Why did institutions that were “successful” in other previously authoritarian countries—such as Germany and Japan—lead to different outcomes in Russia? The authors

suggest the reason is cultural, but the cultural aspect is neither explicit nor rigorously examined.

Second, if the nature of reform institutions is the independent variable, causing reform outcomes that will lead neither to democracy nor capitalism, then what can be done? What could have been done? What were the options? What alternative models existed? The authors are very critical of the institutions introduced to Russia and are persuasive in demonstrating the consequences, but they do not present alternatives that might have avoided the fate Russia has endured for the past ten years. If their argument is accepted, not only was the reform path misguided, but also the prognosis for Russia’s future is fundamentally pessimistic.

Third, the authors maintain that their analysis is relevant for other transitional nations in the former Soviet bloc, yet they indicate that the reform outcomes are culturally specific to Russia. The way in which Russians respond to incentive structures is not being duplicated throughout the Eastern bloc, and in several nations reform outcomes are entirely different. For example, the Czech Republic, Hungary, and even Poland have not shared Russia’s economic fate. The Baltic states, although beset with energy problems that affect their economy, have not experienced the same political deformations in their advance toward democracy. Thus, the analysis seems to be Russia-specific, not broadly applicable to other nations.

Overall, the strengths greatly outweigh the shortcomings. The book is very insightful and persuasively argued. It benefits from the rich detail and well-illustrated examples used to substantiate larger points. This would be expected, since one of the authors is a notable political figure, leader of a political party, and a candidate for president in 1996 and 2000. Without doubt, this book should not simply be read, but should be pondered and studied by policymakers and specialists.

**Ending the LDP Hegemony: Party Cooperation in Japan.** By Ray Christensen. Honolulu: University of Hawaii Press, 2000. 228p. \$52.00 cloth, \$27.95 paper.

Kenji Hayao, *Boston College*

The Japanese party system has been in flux in recent years. In 1993, two groups defected from the Liberal Democratic Party (LDP) and joined with the opposition to form a broadly based coalition government. A year later, the LDP regained power by creating a coalition government with its ideological opponent, the Japan Socialist Party (JSP). Both events shocked virtually everyone at the time. The LDP had been in power for so long—almost 40 years—that it seemed almost inconceivable that it could lose power. For just as long, the JSP had been the main opposition. By the 2000 election, a dozen parties had come and gone, the JSP’s strength dropped to a very small fraction of what it was a decade earlier, and the LDP had to turn to various coalition partners to maintain its control of government. All this is quite puzzling to even close watchers of Japanese politics, because party politics, especially the role of opposition parties, has been a relatively understudied area. For those who want to make sense of how these events came to pass, Ray Christensen’s *Ending the LDP Hegemony* will be very helpful.

According to conventional wisdom, opposition leaders were either too incompetent, too complacent, or too ideologically rigid to be much of a threat to LDP dominance. Christensen shows that the two transforming events of the early 1990s had roots in countless earlier attempts by moderate opposition leaders to expand their numbers in the

National Assembly and take control of the government. The book outlines three major strategies pursued by these leaders in their quest for power.

First, opposition leaders worked to unify the centrist forces. In 1970, the Democratic Socialist Party (DSP), the Clean Government Party (CGP), and the JSP announced a merger plan. Although this did not reach fruition, they began to coordinate electoral campaigns in order to run the optimal number of candidates in each district and to divide the opposition vote evenly, so as to minimize the number of “wasted” votes. Christensen’s careful study shows that, surprisingly, the opposition did better than the LDP in coordinating their efforts.

Second, opposition leaders repeatedly tried to entice various LDP faction leaders to join them. As early as 1966, the DSP’s Sasaki made plans with Nakasone Yasuhiro “to dissolve the Diet and create a new centrist coalition” (p. 137). In 1974, opposition leaders collaborated with Miki Takeo. In 1983, they worked to support Nikaidō Susumu for prime minister against the incumbent Nakasone. In 1990, the JSP’s Ishibashi and the LDP’s kingpin, Kanemaru Shin, discussed plans for Ozawa Ichirō and his followers to defect from the LDP, three years before the split actually occurred.

Third, the leaders of the centrist forces explored ways to become a junior partner with the LDP in a coalition government. As Christensen points out, the LDP had very good relations with all the major opposition parties, with the notable exception of the Japan Communist Party. In 1979–80, for instance, the DSP and CGP both worked with Prime Minister Ohira on legislative and budget bills with the hope of becoming coalition partners in the near future.

These efforts brought some success. The electoral alliances helped whittle down the LDP majority to razor-thin margins in the 1970s, and the LDP lost control of the upper house in 1989. The opposition parties often negotiated with the LDP to gain its support in local elections and force compromises on legislative bills. Nevertheless, at least until 1993, the opposition was unable to bring down the LDP or force it into forming a coalition. Even when the LDP lost control in 1993, it soon divided the opposition again and took it back.

Christensen argues that campaign laws and the party system put the opposition at a strategic disadvantage. Campaign laws (e.g., bans on candidates from advertising in the media) hindered the parties from reaching voters not tied to parties or politicians through organizational networks, such as labor unions, agricultural cooperatives, business groups, or *kō enkai* (personal support groups). Thus, parties had difficulty expanding beyond their base of support. Instead, the opposition was forced to depend mostly on building alliances with one another or enticing defectors from the LDP. In addition, because the opposition was fragmented, it faced “strategic dilemmas of cooperation” (p. 7). As Christensen points out, the LDP maintained remarkably close ties to all the opposition parties, with the exception of the communists, so it often exploited their divisions to entice one of them to break from a united anti-LDP front.

The book would have been even stronger had it pursued the analysis a bit farther. Given the importance of electoral systems in electoral cooperation, more could have been done to analyze the effect of the electoral system on parties and the party system. For instance, Christensen notes that the fragmented opposition has put these parties at a strategic disadvantage, but he does not mention to what extent the old electoral system helped create a fragmented party system, given its semiproportional nature. More also could have been done to analyze the effects of the new electoral system on the development of electoral alliances. The new system allows

candidates running in single-member districts also to appear on proportional representation party lists. Potentially, this encourages parties to run candidates in single-member districts in order to raise the profile of the party, regardless of whether such candidates have a chance to win. This seemed to be much more of a problem in the 2000 election for the opposition parties than for the ruling coalition.

*Ending the LDP Hegemony* is a welcome addition to the literature, especially because little has been written about opposition parties in Japan in recent decades. With the continuing changes in Japan’s party system and among its political parties, Christensen’s study will be valuable to anyone interested in understanding these puzzling developments.

**Revolutionizing the Family: Politics, Love, and Divorce in Urban and Rural China, 1949–1968.** By Neil J. Diamant. Berkeley: University of California Press, 2000. 458p. \$55.00.

Vivienne Shue, *Cornell University*

This is a thoroughly revisionist study, in the best sense of the word. Starting from the conviction that a close look at marriage and divorce in China can open “a wide window onto what might be called the ‘interface’ between state and society” (p. 14), Diamant sets out to capture a better sense of the quality of “everyday interactions between citizen and state” (p. 15). He uses these observations to shed light on larger questions about the degree to which citizens in different social strata may have regarded the state as legitimate or illegitimate, as well as the extent to which state interventions designed to alter power relations in both rural and urban society were effective.

Diamant first lays out the dominant findings and interpretations in the literature on marriage and divorce in Maoist China. This literature teaches that the effect of the 1950 Marriage Law’s guarantee of the right to seek divorce was generally limited. Especially in rural areas, where traditional patriarchal family relations held stable after the revolution, women bold enough to demand a divorce were most often frustrated by community norms and power structures or foiled by the firm opposition of local state officials, who (mostly male) sympathized more with the interests of husbands than with the ideals of freedom expressed in the Marriage Law. Divorce in the 1950s and 1960s was more an urban than a rural phenomenon, pursued more by better educated and more cosmopolitan elite groups than by ordinary working-class or rural people. Diamant takes these widely accepted hypotheses and interpretations, tests them against some fascinating new sources of data, and delivers a radical reinterpretation of who sought divorce and how different levels and institutions of the state behaved in the handling of divorce cases.

Among the first wave of Western political scientists to gain access to local government archives in China, Diamant rests his arguments on painstaking data collection from several different municipal, district, and county government archives in the Beijing and Shanghai areas as well as in the southwestern province of Yunnan. This unprecedented access, and his own prodigious scholarly effort, allow him to compare Women’s Federation documentation, Bureau of Civil Affairs records and statistics, labor union reports, and court materials in several urban, periurban, and rural localities. In these rather candid, unedited, and colorful new sources, originally compiled by local officials and staff for internal use only, Diamant finds evidence that divorce was far more common in

the countryside than has been believed and that rural women, when they did face family and official opposition, nonetheless frequently pursued divorce with blunt and combative determination. Some went on foot when necessary to a number of different government offices to pour out their grievances and demand relief, threatened suicide, or feigned madness if it would help gain them sympathy.

In urban working-class districts, marriages were quite often quickly entered into and just as often hastily dissolved; many spouses clearly regarded marriage more as a matter of pleasure and convenience than a sober and binding lifetime commitment. Furthermore, in the urban districts where the more educated elites resided, divorce was rare and shunned as an unacceptably face-losing option. Diamant argues that educated elite families, influenced by residual Confucian styles of belief and behavior, held to stern codes of propriety and rather prudish values where sex, marriage, and divorce were concerned. They also manifested some more modernist notions concerning “privacy” and were reluctant to air their dirty linen to outsiders, especially since involving state officials in their domestic disputes could have what they regarded as fearful political repercussions. In contrast, Diamant argues, peasants and workers sustained a less fussy, less modernist, more frank, and even “Rabelaisian” sexual culture as well as a more tenaciously “feisty” legal culture. Because they lived in communities where the neighbors were likely to know their business already, they had less “privacy” to lose by drawing the state into their affairs, and they had much to gain in the way of income or personal satisfaction by switching partners.

Diamant stresses the bumbling ineptness of state officials, on the one hand, and women’s determined “agency,” rather than their victimization, on the other. He shows how, when refused a divorce by one local office, women frequently pressed their cases in other offices, often higher up the administrative system, to secure the outcome they desired. He develops this observation into a general hypothesis that local state institutions were often regarded by peasants as less “legitimate” than offices closer to the center, a plausible and promising beginning for future projects that would disaggregate not only the state but also the very concept of state legitimacy.

Yet, just here there is an apparent weakness in Diamant’s argument. After laboring long to persuade the reader that Chinese peasants enjoyed a rough and frank, not a prudish, sexual culture, the author offers as the main reason for the relative illegitimacy of local officials the common knowledge peasants often possessed of those officials’ own moral weaknesses and sexual improprieties (e.g., pp. 234, 318). Perhaps Diamant is right that these delegitimized local officeholders, but if peasants are so accustomed to a culture of sexual transgression, then there are some missing steps in the logic. Were local officials held to a “higher” moral standard? If so, why?

It is possible to point to a few loose ends such as this, but loose ends may be inevitable in a highly original study that boldly generates hypotheses. Diamant’s book is interestingly structured from a methodological point of view, and it is written with arresting insight into the Chinese social condition. It lays out a coherent, strikingly original vision of family-state relations, of contrasting class-based sexual and legal cultures coexisting within Chinese society, and of the frustrating dynamics of state-led social reform during the Maoist era. *Revolutionizing the Family* concludes with an afterword that usefully relates the author’s findings to trends in the post-Mao period as well.

**Can Democracy Take Root in Post-Soviet Russia? Explorations in State Society Relations.** By Harry Eckstein, Fred-eric J. Fleron, Jr., Erik P. Hoffman, and William M. Reisinger, with Richard Ahl, Russell Bova, and Philip G. Roeder. Lanham, MD: Rowman & Littlefield, 1998. 420p. \$64.00 cloth, \$21.95 paper.

Jeffrey Kopstein, *University of Colorado at Boulder*

A decade after communism’s collapse, what do we have to report? For one thing, some states certainly have it easier than others. A handful of the postcommunist states located in close proximity to the West have made admirable progress in constructing viable market economies and meaningful institutions of democratic representation, but a much larger group has yet to taste the fruits of what Western politicians in the 1990s called “market democracy.” Among this latter group, some retain at the time of this writing a formal commitment to democracy, but others have never moved very far from the authoritarian cronyism where they started.

On a continuum between meaningfully democratic and outright autocratic, in 1999 Russia rests about half way between the two extremes. The authors of this volume start with two questions, one substantive and the other methodological. First, which way will Russia go—democratic or authoritarian? Second, how should we go about studying postcommunist Russia? Both are important questions, but the authors concentrate most of their attention on the methodological one.

Is such a concern justified? Have our choices of theoretical models and methods skewed our research so that the things we have learned about postcommunism are not the things we need to know? What are we missing? A great deal, the authors contend. Approaches to the study of postcommunism, they claim, have been one sided, concentrating either on the state (as in much of the transition and institutionalist research) or on society (as in all survey research), but never on both simultaneously.

Philip Roeder gracefully takes on the role of sacrificial lamb by contributing an interesting and carefully researched article that uses a model of elite bargaining to explain variation in the constitutions of the 15 post-Soviet states. Russell Bova provides a stimulating chapter on the conditions under which institutional choice either matters or makes little difference to outcomes. He argues that from the outset Russia chose precisely the wrong institutions (semi-presidentialism with mixed proportional representation and single-member-district electoral rules), ones that were sure to hinder party development, foster an irresponsible grand-standing parliament, and isolate a presidency from the society over which it rules.

But that is the kind of work the primary authors of this volume do not like or at least believe needs to be grounded in an approach more sensitive to society. What is the alternative offered? Given the list of contributors, it should not be surprising that the preferred approach is Eckstein’s congruence theory, which is really a theory of political stability. A polity will be stable to the extent there is congruence between the authority patterns of the state and those of other social units. Polities that work best (are the most stable) have a mixture of authoritarian and democratic features, which in the jargon of the theory is called “balanced disparities.” Too much hierarchy produces autocracy, and too little produces chaos.

What such a theory requires is very careful study of primary social units (such as the family and schools) or adjacent social units (such as parties) as well as a comparison of the authority patterns within the state. Thereafter, a

judgment must be made about the congruence among the patterns. But, as William Reissinger notes in his insightful summary of congruence theory, such a research design is extraordinarily demanding. It also may be indeterminate. The call for fieldwork is laudable and important, but how does one go about judging whether authority patterns are congruent? Does this not lead us back into the older (and not very productive) debates about the nature of historical Russian culture (how democratic it is/was)? One imagines teams of fieldworkers eating breakfast with families, dashing off to attend classes, and then accompanying politicians in their daily routines in search of congruence. Left unclear is the procedure for determining when congruence is there or not. The chapters that attempt a preliminary application of congruence theory (preliminary because none involve primary fieldwork) underscore this concern.

Notwithstanding these criticisms, the volume is a valuable contribution to the field of postcommunist studies. By explicitly attempting to identify what an established theory in the social sciences would have to do in order to be tested adequately in the Russian context, the authors challenge the rest of the field to provide an alternative. Furthermore, although doubts remain about whether such a demanding approach as congruence theory can yield definitive results, the call for more fieldwork in Russia is timely and important. The sad fact is that we still know very little about how Russian political and social institutions work. What comparativists need at present is a whole range of detailed institutional and social studies on postcommunist states and societies. If the inspiration for them is congruence theory, then the authors of this volume will have accomplished a great deal.

**Reconstructing Citizenship: The Politics of Nationality Reform and Immigration in Contemporary France.** By Miriam Feldblum. Albany, NY: SUNY Press, 1999. 227p. \$54.50 cloth, \$17.95 paper.

Mark J. Miller, *University of Delaware*

As recently as a quarter-century ago, virtually no one knew or cared about French nationality law and policy. But in the wake of the May-June events of 1968 there was a *prise de conscience* about international migration and its effects upon French society and the immigrants themselves. New Order, the then quite obscure extreme rightist group, began to organize protest rallies against illegal immigration. In 1974, the post of Secretary of State for Immigration was created to symbolize the growing concern accorded international migration by the French government. Successive governments, whether rightist or leftist in orientation, pledged to recover control over migration.

Miriam Feldblum examines the growing saliency of laws and policies concerning French nationality and citizenship as a political process, drawing from the contentious politics approach championed by Doug McAdam and Charles Tilley, among others. She critiques structuralist and institutionalist analyses of citizenship politics in Europe as pointing to the need "for differentiated but dynamic" (p. 9) understanding of citizenship politics. She thereby situates her case study in the broader comparative sweep of citizenship politics, which emerged only in the late 1980s, with a few notable exceptions, in the pioneering works by Rogers Brubaker and Tomas Hammar.

Feldblum endeavors to correct what she views as a flawed understanding of the politicization of citizenship that privileges the role played by the extreme Right. The immigration issue provided the window of opportunity for the long

discredited, marginalized, and splintered extreme Right to recover electoral appeal, but immigrant organizations also played a key role in politicizing citizenship issues. Although there is an extensive French-language literature about immigrant associations and their campaigns and activities in the 1980s, very little has been written about them in English. Immigrants emerged as political actors in France in the 1970s with their factory strikes, the long SONACOTRA rent strike, and their protests against racist violence and for legalization. By the 1980s, organizations comprised largely of French citizens of North African Muslim background began to press for acceptance and equality of rights and mobilized against prejudice and racist attacks. They frequently asserted a right to be different. In so doing, they challenged the longstanding, almost sacrosanct, Jacobin republican tradition that expects immigrants to become French, not only legally but also culturally.

Feldblum is at her best in explaining why the call for acceptance of difference challenged Jacobin ideals. From the period of the bitterly contested French Revolution, regional or hyphenated identities were associated with subversion and disloyalty to the Republic. In the twentieth century, various institutions, including the Roman Catholic Church, trade unions, military service, and the French Communist Party, long functioned to assimilate immigrants. By the 1980s, however, the waning influence of these institutions was palpable. The National Front burst onto the national scene after stunning municipal elections in Dreux in 1983. In the 1986 parliamentary elections, it won 30 seats in the Chamber of Deputies. Feldblum acknowledges that the National Front's emergence helped trigger the mid-1980s debate over national identity, but all parties to the debate used the language of nation and national identity to articulate positions on immigration, pluralism, and the extreme Right. She cites René Gallisot's assessment that the antiracism of the Left was "caught in the trap of national identity" (p. 54).

In 1985, revision of the French Nationality Code was proposed by a moderate conservative deputy to restrict access to citizenship to those born on French soil of noncitizen parents. Feldblum provides an exhaustive account of the politics surrounding this measure, which ultimately was scuttled by mass protests against proposed reforms of the French university system. She concludes that the failure of restrictive citizenship politics in the mid-1980s was due to "institutional and political process constraints that limited the Right's attempt to revise nationality code" (p. 99). She contrasts her analysis to explanations that rely on national models and culturalist notions.

In 1987, Prime Minister Jacques Chirac appointed the Nationality Commission, headed by Marceau Long. This highly respected body held a series of televised hearings in 1987 and 1988 before issuing recommendations, which in Feldblum's view reconstructed French citizenship in a manner consistent with the Jacobin tradition. The work of the commission coincided with the drama created by the adoption of the Single European Act in 1986, which seemed to challenge French national identity from above, as it were, by creating a truly single market among member states of the European Community.

The commission's recommendations reiterated such traditional republican ideals as national integration, secularity, and limited universalism (p. 128). There was much more continuity than discontinuity. The moderate Right and the Left endorsed the recommendations, which were rejected by the National Front. Many of the immigrant associations also embraced the recommendations and in so doing distanced themselves from the earlier calls for a right to difference. The overall tenor of immigrant-background politics changed, as

many French Muslims openly affirmed their allegiance to French republican ideals.

The final chapters of the book analyze the controversy over the *foulard*, the headscarf worn by Muslims, and nationality law reforms in the 1990s. Since 1989, French educational establishments have been roiled by recurrent debates about whether Muslim girls should be permitted to wear a scarf to class. For many French, it signifies a repudiation of French culture and society as well as the secularity of state-provided education. The scarves evoke fear of Islamic fundamentalism within France. The government response was complicated by the fact that there was no institutionalized representation at the national level for France's burgeoning Islamic community. Feldblum's analysis reveals how limited is the space for American-style ethnic politics in republican France. Indeed, the Socialist government of Prime Minister Michel Rocard explicitly rejected that approach to handling the affair. Disputes over headscarves continue to erupt from time to time, but the outcomes in the 1990s were generally consistent with the Nationality Commission's vision of a tolerant, inclusive France anchored in republican tradition. There can be pluralism and difference, but within carefully delimited republican bounds.

In the 1990s there were first conservative and then Socialist nationality law reforms. Indeed, a centerpiece of the Socialist electoral campaign in 1997 was the call to rescind the nationality law of 1993. As soon as Prime Minister Lionel Jospin's government took power in 1997, a procedure was begun that resulted in legal residency for 80,000 technically illegal aliens. For the Socialists, most of those who qualified had been unfairly treated under the unduly restrictive 1993 law. A principal focus of that reform had been a provision that conferred citizenship upon the children born in France to non-French citizens who had been born in Algeria (and certain additional areas) before 1962, when it was part of the French Republic. The nationality and immigration laws of 1993 were intended to coopt electoral support for the National Front. By 1999, in the wake of the Socialist victory in 1997, the National Front had splintered and lost electoral support.

Feldblum provides an authoritative account of a complex subject that has become an important focus of inquiry only in recent years. This book is highly recommended for graduate-level classes on European politics and comparative migration policies.

**A Place in the Sun: Marxism and Fascism in China's Long Revolution.** By A. James Gregor. Boulder, CO: Westview, 2000. 231p. \$45.00.

William A. Joseph, *Wellesley College*

Few scholarly observers of Chinese politics today would disagree with A. James Gregor's assessment that "little remains of the Marxism of Communist China" (p. 215) other than its use as an ideological subterfuge by the ruling party to bolster its claim to a monopoly on political power in that country. But few would agree with the central thesis of this book that the People's Republic of China (PRC) is best classified by scholars and responded to by Western policymakers as "a variant of contemporary fascism" (p. 166).

Much of the book is a taxonomic exercise that Gregor undertakes in order to dispel what he sees as the "great confusion" exhibited by social scientists in trying to "categorize the system that emerged out of the wreckage of what had been Maoist China" (pp. 163–4). The book is structured to build the case that Chinese communism, which killed millions of people and brought economic and social ruin to the

country under Mao, has given way to Chinese fascism. In the author's view, post-Mao China has achieved considerable economic success but still maintains an iron-fisted grip on the nation's political life while exhibiting prototypically fascist irredentist tendencies in its international behavior.

Gregor places fascist China within the "genus" of political movements he labels "reactive developmental nationalism," which he sees as the key to "Understanding the Twentieth Century" (the sweeping title of the opening chapter). These movements and the regimes they have spawned reflect the common reactions by revolutionary parties and leaders in countries around the globe to the common challenges of economic "retardation" and humiliation, bullying, or outright imperialist aggression by stronger powers. All reactive developmental nationalist systems share political traits, such as a "single, elitist, hegemonic party . . . led by a 'charismatic' and 'inerrant' leader" (p. 65), mass mobilization to build regime support, and a political army. They also pursue similar objectives for their beleaguered nations, namely, rapid industrialization and, most important, "a place in the sun" that will establish or recapture the country's security and status in the international system.

One "species" of the reactive developmental nationalism genus is comprised of totalitarian regimes, which in turn incorporate several "subspecies," including fascism, national socialism, and communism in its many national variants, such as Bolshevism, Stalinism, and Maoism. These regimes are ruthlessly dictatorial and exhibit a "bitter anti-imperialism" (p. 92) that often leads to an irredentist and aggressive foreign policy. In contrast, the nontotalitarian species of the genus are not implacably hostile to democracy and combine a staunch antiimperialism with an eagerness to engage the world on the basis of mutual respect and benefit. The examples of this species that receive the most attention in Gregor's analysis are prefascist Italian nationalism and, particularly, Sun Yat-Sen's "Three Principles of the People" ideology and his Kuomintang (Nationalist Party).

At several points Gregor praises the foresight of the nineteenth-century theorist, Friedrich List, for his early formulation of ideas about the dynamics of world politics that are very similar to those of reactive developmental nationalism. He also argues that fascist theory, because it recognized the central role of nationalism in motivating political movements, has provided a particularly accurate and astute guide to understanding the twentieth century. The perceptive insights of both List and the fascists are sharply juxtaposed to the "theoretical incompetence" and "howling implausibility" (p. 68) of Marxism's emphasis on misguided class analysis and spurious internationalism.

How does Gregor fit contemporary China into this taxonomy? He emphatically contends that the reforms introduced by Deng Xiaoping beginning in the late 1970s completely severed the even tenuous ties with Marxism that the PRC had during the Mao years and led to the establishment of a paradigmatically fascist system in China. He notes that Maoist China had some fascist traits, but the socially divisive insistence on class struggle and the imposition of a command economy ran counter to fascism's core value of national unity and the role of the market (under the supervision of a strong state) to promote economic development. Deng remained committed to political dictatorship, but he renounced class struggle and rallied the nation to unite—under the leadership of the Chinese Communist Party—around the goal of modernization via a revived semimarket economy.

Furthermore, post-Mao China under both Deng and his successor, Jiang Zemin, has embarked on a foreign policy marked by aggressive irredentism. Gregor's evidence in-

cludes Chinese policy toward Taiwan and in the East and South China Seas; steps by the PRC to build up and expand the reach of its naval forces; and a regime-sponsored effort to construct a racially based “myth of descent” (p. 162) to prove that China is the birthplace of the earliest human civilization. Such systemic and policy changes lead Gregor to conclude that “the regime on the Chinese mainland has taken on those critical features that have always been employed to identify fascist rule everywhere in the world” (p. 142). He musters further support for his argument by drawing on the works of “the best Fascist theoreticians in the interwar years,” whose own “defining traits of fascism” (p. 216) provide a near-perfect description of post-Maoist China.

Another major thread of Gregor’s book is the analysis of Sun Yat-Sen’s legacy in China’s long and still unfolding revolution. According to Gregor, Sun was a classic proponent of the most positive subspecies of reactive developmental nationalism. He was prescient in his belief that China’s salvation could come only through capitalist-friendly economic growth and an antiimperialism that both safeguarded Chinese sovereignty and welcomed foreign trade and investment. In fact, Sun so clearly emerges as the paragon of Gregor’s analysis that it is very tempting to read an intentional or unintentional pun into the book’s title.

Gregor sees considerable consonance between the ideology of Sun Yat-Sen and that of Deng Xiaoping—but with at least two fundamental exceptions. First, Sun’s “affable nationalism” has little in common with the “exacerbated and aggressive nationalism” (p. 95) of fascists like Deng. Second, although Sun recognized the need for a protracted period of political tutelage under a single party, he never wavered from his “unqualified commitment to ultimate democratic rule” (p. 165) for China. Gregor refutes the view that Chiang Kai-Shek’s Kuomintang or one of its most powerful and influential factions, the Blue Shirt Society, turned toward fascism following Sun’s death in 1925. The party was thwarted in its efforts to implement all or part of Sun’s mandate on the mainland by communist insurrection and Japanese invasion. But the full democratization of Taiwan under the auspices of the Kuomintang in recent decades is cited as proof that the party has been “inspired throughout its history by Sun’s democratic, non-totalitarian, reactive and developmental nationalism” (p. 217).

Besides offering his taxonomic exercise as a guide for scholarly analysis of Chinese politics, Gregor has an explicit second purpose in identifying the proper classification for the regime that governs the PRC: to sound a warning to the Western world about the impending threat posed to its vital interest by a fascist China. He notes that fascist systems “have been singularly hostile and aggressive” and “have been prepared to employ massive violence” (p. 217) in pursuit of their irredentist objectives. In his view, China will be no exception.

The greatest strength of this book is its thought-provoking, erudite, and eloquent—if often redundant—analytical survey and classification of some of the great political movements of modern times. Students of comparative revolutions will find much to contemplate in Gregor’s observation that, whatever their ostensible ideological differences, these movements share many fundamental features as variants of reactive developmental nationalism.

But Gregor is on somewhat thinner ground in his attempt to fit modern Chinese political history and contemporary politics into this framework. Many China specialists will, for example, find his depiction of the Kuomintang under Chiang Kai-Shek to be naively benign and question the validity of citing Taiwan’s recent political transition as evi-

dence for Chiang’s adherence to Sun Yat-Sen’s untested commitment to eventual democratization.

As one of the world’s leading scholars of fascism, Gregor certainly has the bona fides to know a fascist regime when he sees one. But his unflinching portrayal of the PRC today as a diehard fascist regime seems a rather injudicious conclusion to reach about a political system that is still very much in a state of flux. No doubt there are important and troublesome features of the regime that point in the direction of fascism. But there are many countervailing trends in China that a more nuanced effort to classify the nature of the current regime would have taken into account. Gregor would have made a much stronger case had he noted, for example, the Communist Party’s increasing inability to exert control over or even maintain an organizational presence in various sectors of society; the rapidly expanding influence of the Internet and other modern means of communication; and the spread of at least quasi-democratic elections on the village level.

The last three chapters read, in large part, like a scholarly tract intended to support the views of the so-called Blue Team of conservative politicians, think tank types, journalists, and others who see it as their mission to alert Americans to the “China threat” and steer the country away from the delusional policy of constructive engagement and toward strategic containment. In these chapters, the polemic ultimately overwhelms the scholarship and detracts significantly from what is otherwise a very stimulating book.

**Elections in Japan, Korea, and Taiwan under the Single Non-Transferable Vote: The Comparative Study of Embedded Institution.** Edited by Bernard Grofman, Sung-Chull Lee, Edwin A. Winckler, and Brian Woodall. Ann Arbor: University of Michigan Press, 1999. 495p. \$74.50.

Tun-jen Cheng, *The College of William and Mary*

Under an electoral system of single nontransferable vote (SNTV) with multiple-seat districts, each voter can cast only one vote and only for one candidate, surplus votes cannot be transferred between candidates, and seats go to those candidates with the plurality of votes. Initially crafted by Japanese oligarchs in 1900, this unique system was continuously employed for Japan’s lower house elections till 1995, with a brief interlude during the Allied occupation. The SNTV system has been in use in Taiwan since World War II and was adopted in Korea during the Fourth and the Fifth Republic (1973–88). It is ironic that academic interest in this electoral system should increase just when it is being abandoned in its birthplace, Japan, in a fin-de-siècle political act that also ended political dominance of the ruling Liberal Democratic Party (LDP).

Whereas previous studies have focused on single cases or particular aspects of SNTV, this volume vows to compare the system across time and nations as well as with other types. It also attempts to situate the SNTV system in a broader institutional context in order to reveal linkages among various political games that actors may play simultaneously. Thus, aside from delving into the choice, nature, and consequences of the SNTV system, the four editors (especially the chief editor) are also eager to present a theory of institutional embeddedness. This ambitious volume begins with an excellent introduction of the three cases and an exhaustive formulation of hypotheses, and it ends by refining the findings of various chapters into ten propositions that can be retested in the future. It not only comes close to being a definitive study of the SNTV system but also duly directs our attention to the

integrated research on electoral rules, forms of government, and regime types, which is arguably a new frontier for the next round of research on political institutions.

The volume is thorough in answering the question of what SNTV entails (the use and consequences of this system). There is a near consensus among contributors that the system is conducive to money politics, personal voting, and factionalism. For a political party, optimal nomination as well as even distribution of votes among its candidates are crucial to maximizing seats. Overnomination, undernomination, and lopsided vote distribution lead to either wasted votes or collective defeat. For any individual candidate, factional membership enhances the probability of party nomination. Moreover, given that s/he is competing not only with other parties but also with candidates from the same party, party label does not matter that much, whereas personal support base (*koenkai*) is vital to victory. Maintaining *koenkai* is expensive, and money is indeed the milk of politics. Money politics is also influenced by campaign finance regulations, and the selection process for party leadership and the rules for nomination can reinforce or alleviate factionalism under SNTV.

There is an interesting dialogue on the disproportionality between votes and seats under SNTV. Does this system benefit large or small parties? It facilitates the entry of small and opposition parties in Korea and Taiwan, but it also gives a seat bonus to large parties in Japan and Taiwan. District magnitude (the number of seats to be elected from a district, or  $M$ ) seems to be a critical variable: The smaller the  $M$ , the more discriminatory is an SNTV system toward smaller parties; the larger the  $M$ , the more it functions as a proportional representation (PR) system. The other factor is the organization strength and skill in solving the vote-distribution problem. As I-Chou Liu points out, party identification and institutional devices help a party segment its supporters into discrete voting blocs and help maximize seats. Gary Cox and Emerson Niou are equally convincing in arguing that all parties, large or small, face the possibility of error in nonoptimal nomination and unequal distribution of votes. "Seat bonuses" accrue to whichever party can alleviate these problems.

There is also a fascinating exchange among country-specific chapters on the fragmentation of the party system under SNTV. Steven Reed and John Bolland extend Durverger's law to SNTV and argue that the number of parties at the district level is  $M$  plus one, and the number of factions in larger parties is the same. As the average size of district in Japan was four, the country had a five-party system, and the largest party, LDP, had five factions. The reason for this is the pervasive fear of everyone losing (*tomodaore*) if nomination is out of control, hence the impulse for opposition parties and LDP factions to restrict the number of their candidates in each district (the so-called downward ratchet effect). Edwin Winckler argues, however, that the SNTV's logic can be overridden by issue structure, social cleavage, and public preference. Given the deeply entrenched identity cleavage, Taiwan essentially has two main parties, although district magnitude is generally larger than that in pre-1995 Japan. Three chapters on Korea allude to regionalism (versus electoral system) as the principal maker of the party system there. As Grofman aptly summarizes, the same electoral rules lead to different consequences, which illustrates the importance of institutional embeddedness in shaping the outcome.

The volume is also deft in comparing across types. Arend Lijphart shows that SNTV, if controlled for malapportionment and district magnitude, does not create more dispro-

portionality than the list PR systems. He proposes to classify SNTV, and for that matter limited vote systems, as PR, eliminating the intermediary category between plurality and PR. The chapter by Kathleen Bawn, Gary Cox, and Frances Rosenbluth also connects the study of SNTV to the intellectual stream of the American voters.

Cross-time comparison is neglected (Winckler being a notable exception). Woodall (pp. 48–9) makes an interesting prediction on the change of party system in post-SNTV Japan, but space prevents him from pursuing a comparative static study. Another weakness is the lack of discussion on possible institutional diffusion from Japan to Taiwan and Korea. The origin of SNTV in Taiwan is left unexplained, and adoption of the system in Korea is attributed to Park's power ambition only, as if it had nothing to do with his learning from the Japanese experience. In addition, several chapters allude to but do not systematically address the relationship between SNTV and regime type or the form of government. Finally, the three Korean chapters overlap considerably, as do three chapters on campaigning in Japan under SNTV. These minor criticisms should not detract from the tremendous value of this astutely integrated volume, which is unquestionably the definitive book on SNTV.

**States and Power in Africa: Comparative Lessons in Authority and Control.** By Jeffrey Herbst. Princeton, NJ: Princeton University Press, 2000. 280p. \$55.00 cloth, \$17.95 paper.

Donald Rothchild, *University of California, Davis*

In this important reappraisal of Africa's political evolution over an extended period, Jeffrey Herbst engages in a dialogue with scholars of comparative and African politics on the causes of state weakness and the possibilities for state redesign. Herbst argues that state consolidation in Africa has been complicated by the problem of extending authority over its distant territories. Low population densities have been a long-standing obstacle encountered by precolonial, colonial, and independent African rulers alike. It has proven expensive for leaders at the political center to project power over peoples and territories far from the capital city. As a result, rural areas have been neglected, particularly by colonial regimes, and urban areas have been favored in terms of public services and amenities.

Independent African governments largely embraced the state system inherited from colonial times. They maintained the boundaries established by the Berlin Conference of 1884–85 and in doing so accepted limits on central administration of rural areas nominally under their control. "How power was actually expressed was often similar to the precolonial model of concentric circles of authority. States had to control their political cores but often had highly differentiated control over the outlying areas" (p. 134). Despite limited state capabilities, borders proved valuable in strengthening the ability to affirm sovereign authority within them and to deny any ethoregional claims to self-determination.

The boldness of Herbst's inquiry and analysis is clear to see. The conventional wisdom is challenged on a wide variety of controversial issues: colonialism, neocolonialism, the role of chiefs, ethnicity, boundaries, the state, and international conflict in Africa. For example, rather than view colonial rule as "chang[ing] everything" (p. 29), Herbst stresses the continuities between the colonial period and those that preceded and followed it. On neocolonialism, he raises doubts about the common wisdom regarding multinational control over independent African governments in the contemporary pe-

riod; among other factors, he notes that currency controls by African states dampened the climate for foreign investment by greatly overvaluing exchange rates.

Herbst rejects the common conclusion that national boundaries have been a hindrance to state consolidation in Africa. "It is not that they are alien to current African states but that African leaders have been extraordinarily successful in manipulating the boundaries for their own purposes of staying in power rather than extending the power of their states" (p. 253). A contradiction is evident, then, between the state's claims to sovereignty and the incompleteness of its control of the hinterlands. This contradiction becomes painfully evident in the worst cases, when public order declines, autonomy leads to secessionist attempts, refugees seek sanctuary within their state or across the border, and state collapse occurs.

Inevitably, perhaps, the author's analysis of the contradiction between claims to sovereignty and actual control is more compelling than the solution presented. Arguing that the inherited state system is artificial and static, Herbst calls for alternative designs to reflect the political realities of the post-Cold War era. His recommendations include dealing with African problems on a regional basis, donor support for regional integration, the decertification of states that do not exercise physical control over their hinterlands, the recognition of viable breakaway states, and increased interaction by international organizations with autonomous towns and subregions. These recommendations require the willingness of the great majority of states to work together in an unconventional manner to cope with the challenges of weak states. Because many states are not prepared to deal effectively with the reality of genocide, it seems unlikely that these cautious and uncertain countries can be mobilized anytime soon to address Herbst's suggestions regarding incomplete state control.

Herbst deserves enormous credit for breaking with current approaches to African studies, using broad strokes to paint a vivid and highly original canvas. Because he does not shrink from challenging the conventional wisdom on many critical questions, his book will become a centerpiece of the dialogue on African politics for years to come. Even so, several points deserve further discussion. First, the focus on political geography points to constraints on the state but offers a limited insight into the nature of conflict. As Herbst puts it, "geography is only a given" (p. 159). Such an approach tells us something significant about the limits on central political and military control of hinterlands, but it offers little information about the ambitions of leaders or the uncertainties of patrons or groups that give rise to state-ethnic conflict.

Second, there is little attention to the options for accommodating ethnic conflict through various types of formal and informal power-sharing measures. Herbst concentrates throughout on the elite's preference for the "nation-state," but many African countries are in reality multinational states that have frequently negotiated stable relations on their own. The choices are broader than Herbst examines. Thus, weak states have often resolved their problems politically, forming relations with powerful ethno-regional actors in the hinterlands to compensate for their low level of capability there. In particular, they have included subregional leaders in the cabinet, legislature, and high party positions, making up for their lack of control through the use of such noncoercive incentives as purchase, insurance, and legitimation.

Third, Herbst stresses the strength of boundaries but tends to obscure the extent to which weak African states fail to control the movement of people, arms, and finance across international borders. Diamonds found in Sierra Leone, the

Congo, and Angola are carried across borders and sold on international markets to finance insurgencies; illegal arms are shipped in bulk across international boundaries, fueling such terribly destructive wars as that between Ethiopia and Eritrea; and military forces from seven foreign countries have interceded in the fighting in the Democratic Republic of the Congo. Rather than "escaping the brutal history of continued war" (p. 112), Africans are increasingly brutalized by a variety of international forces beyond their control. The African state, which is often responsible for much of this brutality, lacks the capability to protect its citizens from this externally sponsored havoc.

As with any broadly ranging study, *States and Power in Africa* has problems of approach and analysis, but this is a book of very considerable significance. To his credit, Herbst faces up to many of the major problems confronting Africa in a direct and unflinching manner. This volume will undoubtedly become part of the future intellectual dialogue on comparative and African-related issues and deserves wide attention from both academics and practitioners.

**Of Centaurs and Doves: Guatemala's Peace Process.** By Susanne Jonas. Boulder, CO: Westview, 2000. 299p. \$64.95 cloth, \$24.95 paper.

**Dictating Democracy: Guatemala and the End of Violent Revolution.** By Rachel M. McCleary. Gainesville: University Press of Florida, 1999. 297p. \$49.95.

Edelberto Torres-Rivas, *Harvard University*

Bibliography dealing with the armed conflict in Central America, aptly called crisis literature, was abundant until the latter part of the 1990s, although North American academicians paid little attention to Guatemala. Now two books have been published, both with titles that refer to the same phenomenon. The peace process that concerns Susanne Jonas corresponds to the termination of the violent revolution in Rachel McCleary's work.

McCleary's explanation for the end of the violent conflict rests on the role played by the elite principals of Guatemalan society, the business sector and the military. The central argument is that both elites were in continual disaccord, which at times became open conflict, about the authority of the state to make economic policy and to participate in the allocation of resources for mutual benefit. McCleary bases her analysis of the game of the elites on the well-known views of Richard Gunther, John Higley, and Michael Burton (*Elites and Democratic Consolidation in Latin America and Southern Europe*, 1992, and "Types of Political Elites in Post-Communist Eastern Europe," *International Politics* 34 [June 1997]: 153-68) and on Seymour Lipset's research on democracy and economic development ("Some Social Requisites of Democracy," *American Political Science Review* 53 [March 1959]: 69-105, and *Political Man: the Social Bases of Politics*, 1981). She then analyzes Guatemala's recent history with this theoretical instrument. She describes the transition to democracy, introducing a useful distinction between the first transition (1982-86) and a second one (1986-93), and argues that during the first period the military was in power; not until 1986 was Vinicio Cerezo, the first civilian president in thirty years, elected. She calls the first transition the period of formal democracy.

Since McCleary is interested in relating events that could lead to the consolidation of democracy in a country that does not have much democratic background, she examines the game of the military vis-à-vis the government, especially in the 1980s, and the crisis that divided them and forced them to



cede power to the political parties and the civilians. In the advent of democracy, capitalism is important as a means to ensure that the means of production are private property as well as to safeguard the fulfillment of contracts and open competition.

Most of McCleary's work focuses on a transcendent moment in recent political history: the auto-coup of President Jorge Serrano Elías on May 25, 1993. The rivalry between the elites finally ended when they joined forces in defense of constitutional rights and produced a lengthy agreement, the *Instancia Nacional de Consenso* (National Law of Consensus), which united all the factions of society in favor of institutionalism, law and order. More than political players, they were social players headed by the CACIF (the Coordinating Committee of the Agricultural Commercial Industrial and Financial Associations) which is the highest authority of the business sector, and the military high command. They managed to achieve a unity and consensus that would have been difficult to bring about during any other time in the country's history. In its narrative, *Dictating Democracy* finds the root of its theoretical explanation and its empirical justification: the important agreement of the elites, whose convergent strategies lend stability to the democratic regime. This agreement opens the way for new opportunities for economic change and, more important, for an end to the armed conflict that punished this country for many long years.

The *Instancia Nacional de Consenso* is considered a civilian movement that arose in response to the collapse of the fledgling democratic order. It was organized by the elites but had the backing of popular organizations that succeeded in representing the political will of Guatemalan civilian society. Because it was sheer coincidence, it cannot be given historical dimension because of the consequences McCleary enthusiastically attributes to it: the end of the conflict between the military and the business sector; the unanimity of the players that lead society; and democratic stability. May 1993 was a decisive moment in the process of democratic construction, and this process has continued with some of the same players and some new ones, without serious setbacks. To be sure, a step forward was accomplished with the election of Ramiro de León Carpio to the National Congress, but new difficulties are still to be encountered along the path to the consolidation of democracy.

*Of Centaurs and Doves* analyzes the same period but through a different lens. It is not the elite sectors that explain history but rather the fights, agreements, and failures of the multiple players who move about a stage markedly influenced by what Jonas calls a prolonged civil war. Her interest in the democratic transition revolves around what happened at the end of the counterinsurgency. The first part of her book is a noteworthy account of how the peace process developed between the guerrilla forces and the government and the military. It is a journey that took a sinuous, ten-year path, and it leads Jonas, correctly, to question why at the end there were negotiations and how the dialogue between enemy forces led to the end of the war.

Susan Jonas is without a doubt the North American academician most familiar with the Guatemalan political scene. She has dedicated more than a quarter-century to studying this society carefully and at close range, a study that has resulted in five books. *Of Centaurs and Doves* is the end of a long, personal intellectual cycle that coincides with the end of a tragic period in the history of this country. The peace process is analyzed as a movement to define a new society, a better society. The difficulties and most especially the successes are emphasized. In fulfilling the peace agreements

setbacks arose, and the second part of the book deals with the negative role of the players Jonas calls the "dinosaurs."

Jonas analyzes the constitutional reforms as a decisive element in the construction of the peace, which she does with her customary precision. As for the "no" vote in the Popular Assembly in May 1999, she attributes more negative political effects than there were in actuality. This part of the book provides a careful, although somewhat pessimistic, analysis of the democratic possibilities, especially as concerns the army, comparing the situation to the experience of El Salvador. The rejection of the reforms is evidence of the antidemocratic character of the parties that constructed the democracy (the National Advancement Party and the Guatemalan Republican Front) and the racist culture of Guatemalan society. Jonas makes a comprehensive analysis of this entire process, including the role of North America, which promoted the war and then favored the peace. The author also describes the important part played by United Nations Verification Mission in Guatemala, the murder of Monsignor Gerardi, and the shadows thrown on the guilty ones, who enjoy impunity. The book ends by pointing out that the less drastic and more probable alternative is the institutionalization of uncertainty.

These two books are important because of the information they contain, but their different theoretical focuses are of diverse heuristic value. Both use personal interviews extensively. McCreary draws heavily on business people and others in the private sector, so the narrow vision of the CACIF ends up being hers. Jonas has a wider viewpoint; she interviewed all the opposing factions and did not allow herself to be won over by any. The interpretation she offers is more comprehensive. Her distinct conceptual perspective and the diverse sources of her data explain why, although the subject studied is the same, the results of the two books are different. Democracy has not been consolidated in Guatemala, as McCreary claims, but neither are we on the road to uncertainty, as Jonas proposes. We are in a period of contradictory learning, but we are making progress.

**Creating the Zhuang: Ethnic Politics in China.** By Katherine Palmer Kaup. Boulder, CO: Lynne Rienner, 2000. 221p. \$52.00.

James E. Seymour, *Columbia University*

For communists, the "nationalities problem" has always been the major stumbling block to the realization of their social goals. Mao Zedong believed that it would take the longest to solve. "First, classes will be eliminated, then nations, and finally nationalities. Throughout the world it is the same" (p. 113). Inasmuch as policies on nationalities have been the Achilles' heel of the various Chinese communist experiments, it is surprising that it took so long for scholars, especially those in the West, to examine the mainland's largest ethnic minority, the Zhuang. Kaup's fine little book goes a long way to filling the void.

The Zhuang are especially interesting to political scientists because, to a considerable extent, this nationality is a result of political engineering. There have always been Zhuang, but before the 1950s they never loomed large on China's ethnographic landscape. How this was changed is interesting enough. How it all worked out, with unintended but understandable consequences, is simply fascinating.

In the discourse on nationality formation, Kaup places herself, or at least the Zhuang, largely within the formulation best articulated by David Laitin, according to which ethnic groups are constructs of (often hegemonic) state authority. "The Chinese case," Kaup argues, "is not simply one of the

government's creation of an incentive program for various nationalities. It is one of the state mandating which individuals would be considered part of which bounded and non-overlapping nationalities categories" (p. 20). Of course, it is not quite as simple as this, and Kaup employs various other theoretical approaches to explain the Zhuang phenomenon.

In 1958, when the province-level Guangxi Zhuang Autonomous Region was created, very few people thought of themselves as "Zhuang." As in Xinjiang, the communists found it in their interest to break down local loyalties and forge a fragmented population into a "nationality." The purpose has always been to weaken local loyalties, as opposed to the divide-and-conquer approach applied to the more robust, troublesome, and real Tibetan nation. Unlike other commentators, Kaup believes that pressing political considerations made it imperative to create this region: "Special policies were . . . needed to integrate them into the greater Chinese state" (p. 53).

Such policies have suited communists well, but what have they done for the minorities? The illiteracy rate has declined but is still high, exceeding 50% in many Zhuang counties. The literacy problem is complicated by the many Zhuang dialects. "Despite the impressive figures on the number of books published in Zhuang, I did not once see a Zhuang book for sale the entire time I was in Guangxi and Yunnan" (p. 143). With little in their own language to read, literacy for the Zhuang means literacy in Chinese rather than their mother tongue.

Although data are difficult to obtain, it is well known that economic achievement among China's ethnic minorities is (with the conspicuous exception of Koreans) far below the national average. Guangxi is in the lowest 10% of provinces, and Guangxi's Zhuang are far poorer than the region's Hans (who are Chinese and are in the majority). Zhuang officials now complain that the new loosening of restrictions leaves the Zhuang simply "free to be poor" (p. 178).

There may not have been much reality to Zhuang nationality originally, but it became a sort of self-fulfilling prophecy, and there must be moments when the communists feel that they have created a Frankenstein's monster. "As we . . . move into the new millennium, Zhuang elites are forming action groups to solidify ethnic solidarity and to pressure the government for a great number of preferential policies" (p. 21).

In the light of the breakup of the Soviet Union along ethnic lines, the nationalities in China, including the Zhuang, bear watching.

**Unbroken Ties: The State, Interest Associations, and Corporatism in Post-Soviet Ukraine.** By Paul Kubicek. Ann Arbor: University of Michigan Press, 2000. 275p. \$49.50.

Robert S. Kravchuk, *Indiana University*

This year marks the tenth anniversary of Ukraine's historic Declaration of Sovereignty, and the country will celebrate its first full decade of independence in August 2001. Yet, Ukraine's record as an independent state has been disappointing at best. Even after ten years, living standards for much of the population continue to fall. Development of civil society has been slow, if nonexistent. Political parties are very much nascent institutions. Opinion surveys indicate high levels of frustration among citizens, coupled with extremely low levels of perceived political efficacy. The economy remains dominated by large, inefficient state enterprises and collective farms, dinosaurs left over from the former regime. In many respects, Ukraine appears unable to move forward;

the vision of its future has been shaped largely by its recent past.

This book addresses the mystery of why this country of 50 million, with a proud past, vibrant culture, and abundant human and natural resources (excepting cheap energy), should fail so miserably to develop a market-oriented pluralistic democracy. To answer this question, Paul Kubicek examines the structures, means of influence, and modes of organizational participation in post-Soviet Ukrainian politics. He provides a highly detailed analysis of the relationship of organized economic interest groups to the state, an approach that has both academic merit and instinctive appeal. The effort focuses on the "institutional overhang" left behind in the wake of Soviet communism, a subject either downplayed or ignored in much of the literature, except in purely theoretical terms. The result is perhaps the most thoroughgoing examination yet of interest associations in a post-Soviet state, and its appearance is most welcome.

Most studies of postsocialist transitions privilege civil society, viewed by many as the lynchpin that brought down the USSR. The argument is that development of civil society is necessary for democratization and economic reform, but few have addressed the question of what fills the vacuum when civil society fails to appear, as in Ukraine. Kubicek challenges the notion that institutions of civil society will develop naturally as a consequence of political normalization in transition societies. Society may not grow strong enough to resist the power of the state. In Ukraine, however, the state also remains weak, so it can be described as neither monolithic nor authoritarian. Especially on key reform policy questions, certain organized interests have been able to frustrate the government's attempts at genuine political and economic reform. As Kubicek rightly points out, the result is a "rather paradoxical situation" in which the dominant Soviet-era elites have preserved or even consolidated their positions, but Ukraine nevertheless suffers from a prolonged crisis of power (p. 48).

The author characterizes Ukrainian politics as "weak corporatism," which derives from both a weak civil society and serious divisions among the ruling elite. The result is a process of interest intermediation that is "corporatist in content, if not in form" (p. 132). Institutionalized tripartism among state, business, and labor is weak, if it exists at all. Organized labor has been marginalized, whereas the largest business association, dominated by directors of large state-owned enterprises, has been invited into certain select governmental policy decisions. Agricultural interests continue to be dominated by directors of collective farms, and even though the usual corporatist structures are lacking, one group has been given exclusive rights to represent farmers and to receive and distribute state budget monies to farmers. In sum, economic reform has been hijacked by the powerful industrial and agrarian lobbies, whose positions have been sanctioned by the state. Clearly, the web of ties between the state and interest associations has affected the course of reform, and for the worse.

Kubicek concedes that certain peculiarities make it difficult to classify state-society relations as purely corporatist in Ukraine. He fairly states in his conclusion that corporatism "cannot be used as a term to capture the whole of the Ukrainian reality" (p. 206). Political power is concentrated in a narrow circle at the top, the so-called Party of Power. But this is not a monolithic bloc, and its boundaries are not well defined. Rather, it is a somewhat amorphous group, many within it disagree about reforms, and its fortunes can change (and have) at the ballot box. He thus finds that Ukraine is not wholly an instance of "state corporatism" (dominated from

above) or of “societal corporatism” (power is shared). Furthermore, he finds that it is difficult to identify a single factor as the most crucial to explain corporatist tendencies in Ukraine. Kubicek is surely correct, however, that the primary goal of “Ukrainian-style (i.e., weak) corporatism” is to enable state control over interest associations that may threaten the existing social and political order. There is broad agreement among Ukrainian elites that proliferation of autonomous groups would produce political instability. Avoidance of social upheaval, perhaps leading to violence, is the “bedrock consensus” upon which the entire Ukrainian social order rests. No one wants it, and the populace appears willing to endure nearly anything to avoid it.

Kubicek boldly asserts that “the preexisting transitions literature, with its teleological assumptions and its focus on political parties and public opinion, needs substantial revision” (p. 198). Perhaps. But Kubicek’s text also would benefit from some revision. The book is a dissertation and reads like one, which detracts somewhat from its substantive contribution. It is rich in detailed analysis but is lengthier than need be. There are relatively few citations for much of the detailed information presented. Finally, although the author mentions “path dependency” several times, he fails to define or describe it, and he does not rigorously argue for it. What the book does describe are certain inertial tendencies inherited from the former regime, which have been reinforced by a broad consensus to slow the drive toward a more pluralistic society.

**The Art of Comparative Politics.** By Ruth Lane. Needham Heights, MA: Allyn and Bacon, 1997. 147p. \$32.46.

**Political Science in Theory and Practice: The Politics Model.** By Ruth Lane. Armonk, NY: M. E. Sharpe, 1997. 178p. \$58.95 cloth, \$27.95 paper.

Ronald H. Chilcote, *University of California, Riverside*

In both these short volumes, Ruth Lane assumes an optimistic stance, generally within the mainstream of political science, and attempts to synthesize past and present trends in an effort to show progress. She argues in *The Art of Comparative Politics* that, despite the disparate approaches, real advances have occurred within the field. Her interpretative essay focuses on the recent history of the field, with an assessment of the behavioral movement during the 1960s and subsequent emphases on development, state, grassroots and peasant politics, and the new institutionalism. In *Political Science in Theory and Practice* she affirms that a core consensus has appeared in the independent investigations of prominent political scientists. Thus, a coherent working model of political behavior guides political scientists to understand political realities. She argues that this concrete model coincides with scientific realism and the current understanding of a philosophy of science.

Lane’s valiant effort to formulate a politics model attempts to resolve the prolonged struggle of political scientists to find coherence in the discipline. Lane argues that this framework is founded “not on human subservience to social, economic, or governmental institutions, but instead emphasizes the power of strategically situated individuals to understand, confront, and change circumstances” (p. 6). She defines the model in terms of multilevel interactive situations, individual decision makers, recognition of participant views and goals, interactions among individuals and the resources they employ, rights and duties in decisions about just distribution, and negotiations over the rules and institutions that distribute resources and rights. She insists that this model “strips

away surfaces and finds universal processes at the heart of events. While historical differences will mark these processes, the processes themselves are universal—they are what everyone calls “politics” (pp. 9–10).

The argument evolves through an elaboration of “concrete” rather than abstract theory as illustrated by mainstream examples of theorizing in various political research subfields. The summaries of this work help in defense of Lane’s model, although the emphasis on similarities and conformities obscures attention to alternative possibilities. In this way, Lane demonstrates that we have moved beyond positivism and attention to universal laws and rigid emphasis on falsifiability and instead have adopted scientific realism and model-theoretic forms of explanation and the search for causal processes, so that “it now has become possible to qualify as a scientist without being a positivist” (p. 148).

Many of the examples that buttress the politics model also appear in the essay on comparative politics. Initially, Lane lays out definitions and her framework in a way that may be helpful yet also distracting to the student. The argument that comparative politics is a constantly evolving discipline suggests hope in finding direction among different approaches, and the stress on seeking theory to find order and explanation in the real world is an effort to rise above a level of description. Yet, the push toward a “new model” severely limits possibilities of reaching into the history of the discipline to uncover other approaches or to strike out in new directions. Presumably, the framework leads to a science of politics and comparative politics. Models, however, do serve generally to sort out ideas within a particular context, and they are subject to modification, especially when the conformism of mainstream and traditional political science is challenged. The push by Lane to define political behavior institutionally and the relegation of politics to an individual level not only reveal her preferences but also allow for understanding of trends that are presently popular.

Lane argues somewhat convincingly that comparative politics is the “essence” of political science and that a comparative politics that aspires to be a science is focused on understanding people, not judging them. She explains her emphasis on how political science can be a science by relating art to science: “If art is seen as the creative approach to reality, and science is seen as the discipline of inquiry into the real world, then the distinction disappears and art becomes an integral part of science” (p. 5).

Lane divides the history of comparative politics into four major periods: the behavioral revolution, the development movement, the return to the state, and individual choice. The latter period marked a tendency toward disillusion with the earlier trends. Each period is discussed in the light of mainstream contributions, successes, and weaknesses, but gaps are evident in the discussion.

For one, rather than identifying system and culture theory with particular periods in the historical evolution of comparative politics, Lane integrates them into her critique of behavioralism. She identifies David Easton as the spokesman of the behavioral revolution in political science and Gabriel Almond and Sidney Verba as the originators of behavioralism in comparative politics through their study of political and civic culture. Justifiably she criticizes Easton’s theoretical abstraction, but she ignores his empirical work on schools and political socialization. Indeed, political socialization, once a major research area for political science and comparative politics, is overlooked altogether. She delineates Easton’s “creed” of behavioralism without reference to his *APSR* presidential address, which acknowledged postbehaviorial counterinfluences in the discipline. In her elaboration of

interesting case studies that reflect the behavioral trends, the description reveals the results of Robert Dahl's important study of elite rule in New Haven. It overlooks, however, William Domhoff's followup, which is based on different questions to the same people interviewed by Dahl and reveals different understandings about rule and power in New Haven.

Gaps also appear in the attention to developmental theory. There is reference to Rostowian economic stage theory but no acknowledgment of A. F. K. Organski's futile attempt to apply such theory to politics. Lane reviews the criticism of structural-functionalism without linking it, for example, in the work of Gabriel Almond, to the underlying ideological belief that development appears only in systems of formal, indirect, and representative rule. What is missing in an otherwise instructive review of Samuel Huntington's notions of order in changing society is criticism of his reliance on authority and his conservative skepticism about revolutionary change for Third World society. In linking dependency theory with Marxism, Lane overlooks that few dependentistas actually relied on Marxism or failed to cite Marx. Furthermore, although Marx in the case of India emphasized the long path to socialism through development of the capitalist means of production, Lane ignores his writings on Ireland that come remarkably close to the dependency perspective.

The characterization of dependency theory as "vulgar Marxism" belies the fact that writers such as Paul Baran, André Gunder Frank, Theotonio dos Santos, and others all wrote about dependency and underdevelopment in defiance of distorted sectarian positions advocated by traditional and orthodox communist parties. These writers captivated intellectual interest about why capitalist development was not occurring in the Third World, and their views eventually became influential in mainstream North American social science. If, as Lane argues (p. 71), both Adam Smith and Karl Marx failed to outline a developmental path, leaving the task to comparative political scientists to find a different way, then it would seem that we might benefit from a close look at development in terms of capitalism and socialism as economic systems that dramatically affect politics.

Likewise, Lane's discussion of the return to the state emphasizes the work of Evans, Rueschemeyer, and Skocpol during the early 1980s without recognition of the seminal work of European political scientists, such as Ralph Miliband, Claus Offe, and Nicos Poulantzas, who elaborated a theory of the state a decade earlier. Also, Lane does not take us back to the origins of American political science, which was deeply influenced by the notion of state and formal notions of government and state in nineteenth-century German literature. Her focus on more recent theory of the bureaucratic authoritarian state overlooks the extensive literature and debates around corporatism that preceded it.

Finally, the interesting review of the new institutionalism turns to studies of the peasantry, grassroots, resistance, and rebellion. Lane elaborates on the history, principles, and influence of rational choice in the new institutionalism and sets forth an institutional model. A concluding chapter reviews various theories in an effort to justify her assertion that comparativists, working on their own over the years, have unconsciously put together the politics model she so carefully delineates throughout the book. These lapses should not deter comparativists from delving into these two interesting works which attempt to look at traditional and current thinking in new and challenging ways.

**Privatization South American Style.** By Luigi Manzetti. New York: Oxford University Press, 2000. 373p. \$74.00.

M. Victoria Murillo, *Yale University*

Luigi Manzetti fills an important gap in the literature on market reforms in Latin America by providing a comparative analysis of privatization in Argentina, Brazil, and Peru. He engages the literature on economic reform in developing countries by focusing on the implementation of this single policy and complements a burgeoning scholarship on the economics of privatization in the region. The main contribution lies in underlining the relevance of political factors for explaining the success of privatization policies. The "South American" style, he suggests, reinforces the view of those who, like John Williamson and Stephen Haggard ("The Political Conditions of Economic Reform," in Williamson, ed. *The Political Economy of Reform*, 1994) and Guillermo O'Donnell ("Delegative Democracy," *Journal of Democracy* 5 [January 1994]: 53–69), associate the rapid implementation of market reforms with the concentration of executive authority at the expense of the checks and balances of liberal democracies.

The most extreme cases in this trend are Peru, with the self-staged coup of President Alberto Fujimori and the dismissal of nonsubservient judges, and Argentina, where President Carlos Menem packed the Supreme Court with political loyalists (p. 322). Nonetheless, Manzetti recognizes the importance of political bargaining for building coalitions in support of privatization. In particular, his comparison between the success in Brazil of President Fernando Henrique Cardoso's privatization efforts relative to those of his predecessor, President Fernando Collor, highlights the importance of bargaining in building a proprivatization coalition (chap. 4).

Manzetti proposes an analytic framework that incorporates many of the variables mentioned by the literature on economic reform in developing countries. The five main concepts are: the willingness to privatize, the political opportunity to do so, the government capabilities to implement the policies, the political responses generated by the decision to privatize, and the technical difficulties for accomplishing the process. In addition, the author presents a rich empirical description of privatization efforts by presidents Raúl Alfonsín and Carlos Menem in Argentina; presidents Fernando Collor, Itamar Franco, and Fernando Henrique Cardoso in Brazil; and presidents Fernando Belaunde Terry, Alan García, and Alberto Fujimori in Peru.

Although Manzetti compares privatization experiences across time and across countries, he could have used the rich information in the empirical chapters for comparisons across or within sectors. That approach could have answered other comparative questions, in addition to the success of privatization efforts, such as those regarding the type of privatization. Why was electricity privatized without vertical integration as a strategy to promote competition in Argentina and Peru, whereas telecommunication companies were privatized as monopolies in both countries? Why did Brazil, in contrast, include competition from the onset in the privatization of telecommunications? Although implicit comparisons are made across privatization cases in different countries, an explicit analysis of sectors might have yielded answers to this type of question. In fact, variables that Manzetti examines—such as the effect of different bureaucracies, fiscal emergency, and interest groups—influenced the decision on how to privatize. His cross-national comparisons lay the ground for future studies to undertake this task.

The organization of the book makes it difficult to test the

analytical model, especially for readers who are not country specialists. Often, too much empirical description leaves little space for an extensive discussion of alternative explanations. The author begins with a thought-provoking and easy-to-follow list of plausible hypotheses to be tested, but his summary of findings would have been clearer if he had employed a similar analytic device in the concluding chapter. A graphic summary of the qualitative measures for each case study would have highlighted the explanatory power of the model vis-à-vis alternative hypotheses, even if no quantitative measures could be provided. In addition, the use of alternative research designs, such as Charles C. Ragin's Bayesian method (*The Comparative Method*, 1986), would have allowed Manzetti to state more clearly which of his five variables are either necessary or sufficient for the success of privatization.

This detailed comparative analysis is a point of departure for future studies on privatization in the region. Manzetti illuminates the politics of a policy that reshapes developmental states and modifies the ways in which the state is used for political goals, such as patronage or subsidies, in many developing countries (e.g., Robert Bates, *Beyond the Miracle of the Market*, 1991). Building upon a field characterized by single-country monographs and a literature that usually is too technical, Manzetti provides a comprehensive synthesis of various explanations of the success of privatizations in South America while emphasizing the political character of such processes.

**Policy Representation in Western Democracies.** By Warren E. Miller, Roy Pierce, Jacques Thomassen, Richard Herrera, Sören Holmberg, Peter Esaiasson, and Bernhard Wessels. Oxford: Oxford University Press, 2000. 180p. \$65.00.

Valerie R. O'Regan, *North Dakota State University*

Those who study the concept of representation are undoubtedly familiar with the 1963 study by Warren Miller and Donald Stokes ("Constituency Influence in Congress," *American Political Science Review* 57 [March 1963]: 45–56), which had a profound effect on scholars' understanding of the relationship or "congruence" between representatives and constituents. Others (see Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality*, 1972; Heinz Eulau and Paul D. Karps, "The Puzzle of Representation: Specifying Components of Responsiveness," in Heinz Eulau and John C. Wahlk, eds., *The Politics of Representation*, 1978) have made their own distinguished contributions by venturing to conceptualize and measure representation in an effort to further our understanding of the relationship between the representative and the represented. In the same mode, this collection of articles contributes to the study of the mass-elite relationship by providing a variety of approaches, methods, and measures to broaden the literature.

In the past, work frequently has been limited to single case studies of various countries, including France (Philip E. Converse and Roy Pierce, *Political Representation in France*, 1986), Sweden (Sören Holmberg, "Political Representation in Sweden," *Scandinavian Political Studies* 12 [March 1989]: 1–35), and the United States (Richard F. Fenno, *Home Style: House Members and Their Districts*, 1978), among others. Difficulties regularly associated with cross-national research, such as unavailable and noncomparable data, have hindered attempts at comparative analysis. The focus on specific cases,

however, limits our understanding of any cross-national similarities and differences.

The central contribution of *Policy Representation* is its thorough, comparative analyses of five industrialized democracies. They represent various types of systems (presidential and parliamentary, two-party and multiparty, majority and party list) found in democracies throughout the world. In addition, the contributors adopt different approaches that incorporate basic theories of representation, individual and aggregate data, diverse measures, as well as innovative and established methods. Similarities can be found among some of the chapters in the application of certain models as well as the significance of communication between elites and masses in the representation process, which was a critical point made by Miller and Stokes in 1963.

An introduction explains the motivation and circumstances that resulted in this collection, followed by six different perspectives that draw on diverse theories, measures, and methods. The second chapter explores the linkages between issue positions of voters and political parties by applying the responsible party model of representation. An original measure, Pierce's Q, is used to evaluate the mass-elite issue linkages in the five political systems. In the third chapter, two models of representation are applied in the study of issue congruence, and emphasis is on the usage of the left-right dimension in communications between the masses and political elites. Chapter 4 analyzes the political discourse and levels of understanding between masses and elites. In chapter 5, the Galtung system of distribution curve shapes is used to analyze collective policy congruence. The sixth chapter examines the occurrence and effects of geographical distinctions in voters' policy views. Chapter 7 examines the relationship between the different political systems and policy representation based on the characteristics inherent in the systems. The book concludes with an overview of the findings from each chapter.

Each chapter takes a distinct approach to the subject. In most cases, the authors present their arguments, methods, and analyses logically and with originality and clarity. A couple were challenged with unexpected results that they thoughtfully explain and point to as foundation for further cross-national research on mass-elite linkages. One slight criticism involves the explanation of the curve shape analysis of mass-elite congruence. The author is so immersed in explaining methods that some confusion is created. Overall, however, the book is well written and interesting. In addition, some of the chapters offer innovative methods, such as Pierce's Q, that will advance comparative research on policy representation.

In addition to the six different approaches that can be developed in future research, a major contribution of this work is the comparative format. All the chapters incorporate data sources that allow the authors to provide a truly comparative analysis of mass-elite linkages. The study also produced a noteworthy international data pool, and it is hoped this will be expanded as future research incorporates other countries into the policy representation analysis.

Ultimately, "policy representation is a multifaceted phenomenon" (p. 111). This book does a fine job of educating the reader on the complexity and difficulty in determining what policy representation entails. Furthermore, the authors make a significant contribution to the comparative study of the subject. Those who are intrigued and challenged by the concept of representation will find this book to be an interesting piece of the policy representation puzzle.

**Money, Markets, and the State: Social Democratic Policies since 1918.** By Ton Notermans. New York: Cambridge University Press, 2000. 312p. \$59.95.

Carles Boix, *University of Chicago*

Notermans has written a bold and ambitious book in which he purports to explain the conditions under which social democratic policies, and therefore the social democratic project, have been successful in modern democracies. The book, which relies heavily but not exclusively on historical data, examines the ebb and flow of social democratic dominance in five countries—Germany, the Netherlands, Norway, Sweden, and Britain—since roughly the introduction of (male) universal suffrage after World War I.

What constitutes a social democratic program has been a regularly contested issue among political economists. To Notermans, social democracy requires, above all, the pursuit of accommodating monetary policies. Several reasons are advanced to support that choice. In contrast to a policy merely oriented toward price stability, an expansionary monetary strategy makes full employment possible, which maximizes the welfare of traditionally social democratic electorates. More important, and in line with the insights of postwar consensus literature, an expansionary monetary policy reconciles the divergent interests of both workers and employers. Probably for that reason, any reference to the expansion of the welfare state as a key contribution to social democracy is rather muted in the book. The zero-sum nature of purely redistributive policies was the stumbling block of social democracy in the 1920s. It was only when social democrats could deliver growth, through cheap money, that they could make redistribution palatable to all social sectors. In short, expansionary monetary policy is the essence of social democracy because it constitutes the precondition to overcome a potential trade-off between equality and efficiency.

The changing fortunes of the social democratic program throughout the twentieth century are examined, in vivid and fresh detail, in the three central chapters of the book. Notermans first explores why there was no social democratic breakthrough in the 1920s—this is probably one of the best parts of the book, giving original historical information on a period (pre-Keynesianism) that has been less well examined in previous work on the adoption and diffusion of demand management ideas. Notermans then delineates the successive steps that culminated in the golden age of postwar social democracy: labor and agricultural markets stopped being neoclassical, the gold standard collapsed, and, after World War II, Bretton Woods embedded interventionist domestic policies in a common international architecture. The triumph of social democracy started to unravel in the 1970s when inflationary tensions finally became unmanageable, but, as the book shows in a pristine way, the tempo varied substantially across nations. Keynesianism was rapidly abandoned in Germany and the Netherlands and came to a very abrupt end in Britain in 1979. By contrast, Scandinavian governments remained committed to accommodating policies for another decade, although there, too, the commitment to Keynesianism eventually became almost impossible to maintain.

The historical inquiry of the book is always bound by a theoretical tension to understand the causes underlying the growth and fall of the social democratic program in the last century. Three types of explanations are rejected: ideational factors, understood as the emergence of a set of technical beliefs that convinced policymakers; shifts in the electoral arena; and the internationalization of the economy. Instead,

Notermans relies on the combination of what can be called an exogenous shock argument and an institutionalist explanation. The book points out (in a less explicit way than I would have liked) that fundamental shifts in policy regimes (especially from neoclassical economic policy to Keynesianism in the 1930s, but to some extent from the latter to monetarism in the 1980s) took place almost by default. The impression one gathers is that politicians (conservatives and socialists alike) always resist jumping from the old ship until they are literally forced to do so by events and catastrophes much superior to their forces. Once they are in a new territory, they rapidly cling to it until a new shock shatters the prevailing equilibrium.

Why both regimes (neoclassical and Keynesian) have not been completely stable is attributed by Notermans to domestic institutional conditions. Following the neocorporatist canon, he acknowledges that no expansionary policy can survive without moderate wage agreements (which in turn require certain types of unions and businesses). The explanation is compelling—it fits the neocorporatist agenda but gives it a much needed historical perspective—but it may be insufficient for two reasons. On the one hand, explaining why Keynesian policies fail does not address the question of why an orthodox monetary regime may break down, and this is not tackled in the book other than by saying that orthodoxy is always susceptible to be endangered by a deflationary “cumulative process” (p. 32). Of course, one can always maintain that expansionary policies are the natural regime of any democracy (due to popular demands for full employment), yet the short-run nature of the Phillips curve periodically leads to the breakdown of demand management. The problem is that this explanation is belied by the long-run success of non-Keynesian recipes in several contemporary democracies. On the other hand, the book omits discussing an important part of the corporatist literature: Wage pacts can only be sustained over time if some sort of political (and budgetary) exchange is enforced. In other words, Keynesianism goes hand in hand with either the construction of welfare states or the development of microeconomic policies to increase workers’ skills and productivity.

Although the book is a very rich story of social democracy, there are three aspects that remain problematic. First, not all social democratic governments have followed Keynesian recipes to achieve full employment and equality goals. With the aid of the literature on market failures and, later, new growth theory, the construction of the welfare state has been justly defended for its positive effects on both growth and welfare. Similarly, supply-side policies, oriented to the creation of physical and human capital—to make sure that the marginal productivity of labor increases in such a way that equality is not detrimental to efficiency considerations—has been systematically used by the Left. Note the application of Rehn-Meidner plans in Sweden or the expansion of the stock of public capital in France and Spain in the 1980s under a regime of price stability.

Second, Notermans does not explain well how internationalization affects Keynesianism, particularly in the last decade or so. Although the process of internalization is not a *deus ex machina*, we should recognize that what other countries did and how capital markets changed affected the judgments social democratic regimes made about the economic and political feasibility of their expansionary policies. Their decision to dismantle in the mid- and late 1980s the set of capital controls and financial regulations that had helped them to sustain moderate (and relatively stable) levels of inflation was not simply a random event or the result of miscalculations (Notermans is not very precise about the causes of deregu-

lation). Social democratic policymakers probably calculated that, in the wake of extensive deregulation in other areas of the world, they could not risk being left out of the long-run benefits of growing capital mobility.

Third, electoral politics is strangely missing in the book, especially in view of the attention paid to the putative decline of social democracy at the polls in the 1980s and early 1990s. If, as the author claims, ideational considerations did not affect the choice of different policies, then material interests necessarily explain why governments changed their policies. These interests necessarily worked through the ballot box. Once certain institutions (or the lack of them) did not work to achieve noninflationary growth, Keynesianism was abandoned either because voters switched to conservative parties or because socialist politicians themselves changed in anticipation of the reaction of voters. But elections and electoral decisions as the mechanisms of change and, more generally, the question of which were the past and which will be the future political bases of social democratic parties are only given short notice in the book.

**Gender Matters: Female Policymakers' Influence in Industrialized Nations.** By Valerie R. O'Regan. Westport, CT: Praeger, 2000. 168p. \$49.95.

Georgia Duerst-Lahti, *Beloit College*

Political globalization makes cross-national comparisons ever more important, especially those that reach widely and over time. Such policy analysis offers potentially practical applications for improved democratic representation. Valerie O'Regan provides such analytic reach in her study about the influence female policymakers have achieved in 22 industrialized nations. She considers the effects and effectiveness of female legislators and executives in representing women's interests. The central thesis, that gender differences in policy priorities will be reflected in policy outcomes as the number of female policymakers increases, is supported. The secondary thesis, that (wage) policy comprehensiveness will improve, is not.

The study is commendable on several fronts. O'Regan provides a thorough venting of contradictory definitions and approaches on most key concepts considered, including alternative ways to define women's issues and policy responsiveness as well as the varied approaches to analyzing policy influence. Change over time can be considered because the data cover an unusually long span. Unlike most such studies, she is able to construct and compare conditions and policies from 1960 to 1975 with those since the women's movement reignited, from 1976 to 1994. Her sample of 22 nations captures more of the industrialized world (e.g., Joni Lovenduski, *Women and European Politics*, 1986) over a longer period (e.g., Pippa Norris, *Politics and Sexual Equality*, 1987) than other endeavors. It also updates knowledge about a rapidly changing field. It includes both qualitative and quantitative analysis, which enriches the study and overcomes some weaknesses. The approach to this prodigious undertaking is intelligently thorough, although it is not without some flaws.

O'Regan groups the 22 nations into six categories according to region and policy similarities. The categories, such as North American nations, Mediterranean nations, and Scandinavian nations, make sense, but Japan awkwardly stands alone as its own category. The study provides a great service by pulling together equal wage policies as they evolved over the two periods for all the nations. These policies are usefully summarized in Figure 4.1. Each nation's policy is considered

for its orientation toward equal pay for equal work, equal value, or comparable worth as well as for jobs covered, policy targets, constitutional basis, and more.

Two sets of quantitative analysis pursue the presence of women's wage and social policies and the comprehensiveness of wage policy in pooled time-series analysis. Many facets of this analysis demonstrate adroit conceptualization. By adopting the notion of "nation years" (p. 41), O'Regan increases the number of observations: Each of the 22 nations is considered 35 times, thereby building more robust data. She controls for the right conditions throughout the analysis. For example, in the first analysis on social policy, abortion is treated separately, and in the second analysis on wage policies, she excludes the four nations whose wage policy is determined by collective bargaining. Factors such as percentages of Catholics, socialists, and females in the labor force and of childbearing age become variables. In several instances she drops particular nations because of unique circumstances. With her large sample, the analysis can proceed unhindered by these wise exclusions.

The central results confirm the importance of more female policymakers to the existence of women's employment and social policies, and some tantalizing surprises emerge in the findings. As expected, all nine regressions show that an increase in female policymakers is associated with policies beneficial to women. Also as expected, a higher proportion of women involved in unions and collective bargaining policy proves beneficial to policy. The finding that "female heads of government were more likely to have social policies and less likely to have employment and wage protection policies" (p. 98) than their male counterparts needs better explanation. Other surprises are that more policies on behalf of women are associated with a stronger Catholic influence, and conservative parties in power.

The analysis on the comprehensiveness of wage policy proved poor for women's representation. Only two variables were significant as predicted: nations with stronger economies and more women in their childbearing years. Contrary to predictions, nations with a higher percentage of female policymakers were less likely to produce progressive equal wage policies, with all other variables showing as significant in the opposite direction. O'Regan explains these negative results most of all as a methodological failing in the construction of the scales. This failure and her explanation of the contrary findings point to some of the study's shortcomings.

First, coding requires simplification of complex policy realities so that only a limited range of insight can be garnered. The first set of analysis coded merely whether a policy existed. It could not capture more about the nature of the policy or the quality of its implementation. The second analysis attempts to capture something more complex, and it does not succeed. The inconsistency in interpretation depending upon the success of the hypotheses nag at the validity of both analyses.

Second, and more fundamental to the study, although the data run through 1994, very little literature beyond 1990 is used. As a result, the explanations cannot benefit from the extensive development in gender theorizing since then. For example, never does the author suggest that the women who occupy policymaking seats, especially as heads of government, must "perform" the masculine gender despite being biological females. There is no reference to varying gender ideologies, but there is a real possibility that the particular women who have succeeded might well believe in gender universalism so support such policies. This advanced understanding of gender theory would explain apparent inconsistencies at least as well as problems with coding. Finally, the

book reads somewhat like a dissertation, although it also offers the benefits of being straightforward and thorough.

Scholars of comparative politics who care about democratic representation and policy should read this book, not only scholars of women or gender. Much can be learned from it.

**Islam in Contemporary Egypt: Civil Society vs. the State.** By Denis J. Sullivan and Sana Abed-Kotob. Boulder, CO: Lynne Rienner, 1999. 159p. \$49.95.

Sami Zubaida, *Birkbeck College, London*

“Civil society” has become a key concept and a central quest in the search for paths to democracy and liberty in many parts of the world. This search has been particularly notable in Egypt, where an increasingly totalitarian state has sought in recent decades to project an image of democracy but at the same time attack and undermine all potential bases of social autonomy and political action. These are the central issues discussed in this book. The picture is complicated by the prominent part played by religious and religio-communal politics on the Egyptian stage. Are Islamic associations and forms of political action forces of civil society engaged in the quest for social autonomies and liberation from authoritarian strictures, or do they themselves add another tier of repression in the name of religious conformity and moral conduct?

The authors argue, quite correctly, that there are many different kinds of Islamic movements and tendencies in Egypt, with various orientations and programs. Many of them, especially the nonviolent political strands of the Muslim Brotherhood, are seen as forces of civil society, working for pluralism and democracy. The various religious charities and social services associations, some of them linked to the Brotherhood, others ostensibly nonpolitical, clearly form a vital component of the active nongovernmental organization (NGO) sector in the country. Islamists are also active in many spheres of public life, crucially in the professional syndicates of doctors, lawyers, and engineers, which they controlled at one point, until government intervention dislodged them. On the whole, the authors present a rosy picture of “moderate” Islamism, as a benign force for civil society and democracy against an authoritarian state. This is enhanced for them by the fact that the Muslim Brotherhood is the one oppositional political current with a broad social base and political constituency, in sharp contrast to “secular” parties, which are mostly feeble talking shops.

In common with the bulk of the literature, the authors identify civil society in voluntary associations, NGOs, charities, parties, and syndicates, and they note the prominent input of Islamism into these sectors. They point out the extensive volume and vitality of these associations in modern Egyptian history, despite close regulation by the authorities, who use wide-ranging, catch-all decrees that allow them to intervene in the direction and finances of these organizations as well as prohibit or even merge them at will. There are many scandalous examples, notably the feminist organization that was closed down and its assets passed to a religious, state-controlled women’s charity. In the early 1990s the government issued equally arbitrary decrees to control professional syndicates and exclude the predominantly Islamic elected executives. It would seem that, far from being bases of social autonomy, these formations are constantly at the mercy of a capricious authority, concerned primarily with eliminating political opposition and tolerant only of associations and actions it controls, or that pose no challenge.

The diversity of the Islamic movement noted by the

authors extends to the Muslim Brotherhood itself (which they tend to present as a unitary “moderate” movement). A crucial divergence at present is generational, between the conservative old guard and a younger more modern intellectual elite, the generation of the 1970s student movement. The quest for some measure of democracy and pluralism, attributed by the authors to the whole movement, may be more realistically found among the latter, who espouse political and social programs (mostly developed from earlier nationalist and leftist projects), as against their conservative elders, who take a predominantly moralistic and authoritarian stance. The authors cite many examples of Islamist intellectuals calling for democracy and pluralism as well as women’s rights. The language of democracy, however, is spoken by most if not all sectors of Egyptian politics, including the president and the political directorate, but the actions and policies of most belie these proclamations.

Islamists of all tendencies have not shown particularly liberal attitudes in dealing with challenging opinions and have always been ready to censor and denounce any cultural product they deem at variance with the true religion. The secularist publicist Farag Fauda was assassinated by violent militants, who were subsequently defended in court by a prominent figure of the Brotherhood on the ground that apostasy is punishable by death. The persecution of “apostasy” (as defined by the accusers) has been extensively pursued in recent years, most notably in the case of the academic Abu Zaid, who writes textual critiques of canonical sources. He was brought to court by Islamist lawyers associated with the Brotherhood, who demanded that he be divorced from his wife, because an apostate cannot be married to a Muslim! Eventually their petition was granted by a higher court, but its execution was stymied (not reversed) by government maneuvering.

Other censorship campaigns have purged university libraries of a wide range of books judged harmful to religion and morality and have led to the banning of many publications, restrictions on performances and exhibitions, and attempts to censor or ban films. These acts are carried out by respectable “moderate” Islamists in high positions in ministries, the media, and legal institutions. This type of conservative Islamism, which seeks the moralization of public life rather than political reform, is the dominant trend in Egypt, and its networks and personnel span government and opposition, business and the professions. As one commentator has asked, is this society civil? Because this book glosses over these negative aspects of Egyptian society, it overlooks the deep complicity between the authoritarian state and many of those it rules.

**Transitions from State Socialism: Economic and Political Change in Hungary and China.** By Yanqi Tong. Lanham, MD: Rowman & Littlefield, 1997. 280p. \$69.00 cloth, \$26.95 paper.

Tianjian Shi, *Duke University*

Tong addresses one of the most important issues in comparative politics: What are the key factors that determine the courses and outcomes of transitions from state socialism? The book makes three contributions to this field of study. First, it reminds us that the relationship between state and society with regard to either power or objectives may not be zero-sum, as widely accepted by students of politics. The author argues that state and society may share a wide range of objectives, laying the groundwork for a cooperative rela-



tionship. This helps explain why the regime and a society may compromise in the transition process.

Second, Tong incorporates political culture in an innovative way. To her, reform of state socialism involves not only economic and political transitions but also an ideological transition. Reform usually starts with a break from orthodox communist ideology, but its replacement is not at all clear. Even though many people would prefer to see liberalism selected, deeply rooted cultural values can significantly influence the choice. More important, since the ideological transition is usually among the last to be accomplished, the absence of a value system that can provide norms for social behavior impedes the other two transitions. Democratic and market institutions require an appropriate cultural foundation if they are to function properly.

Third, since the transition involves a shift from state dominance to more societal autonomy, neither the state-centered nor the society-centered approach can fully explain the process. Tong suggests instead a “transitional” approach that focuses on both the state and society and on their power relations, which are determined at least in part by their mutual interaction. The approach is dynamic, for it pays attention to the objectives and resources that state and societal actors bring to their interactions. Because state policies can empower certain social groups by granting them access to resources that the state controls, the state’s decisions on social, economic, and political matters will create new interests among the sectors of society affected by those decisions.

The comparison of reform experience in Hungary and China shows that the growing crisis of legitimacy stimulated regimes in both countries to launch economic reform. Its implementation called for a certain political flexibility to allow intellectual innovation and popular participation. In both societies, the reforms created complicated pressures for political change. By departing from traditional conceptions of economic structure, the reforms continuously challenged the previous notion of socialism and demanded its further redefinition. Moreover, economic reform increasingly erodes the ability of the state to control society in the old way. Finally, economic reform led to inflation, inequality, and corruption, which produced increasing and contradictory political pressure on policymakers to slow the pace of reform for the sake of stability and to speed it up to hasten completion of the process.

In both countries, socioeconomic grievances produced societal challenges to the political establishment. The communist parties in Hungary and China made considerable efforts toward accommodation. In comparing the forms of adaptation in both countries, Tong found an observable difference in the level of institutionalization. In Hungary the trade unions had a more routinized role in decisions concerning workers’ interests than did their counterparts in China. Both the electoral and legislative reforms in Hungary were also more institutionalized. In China, there were few sustained institutional changes. To Tong, the traditional political culture in China, which emphasizes consultation over formal accountability, largely explains this critical difference.

When political and ideological liberalization permitted the emergence of a popular opposition movement that could express its grievances more openly in these two societies, the political leadership in both China and Hungary split over the speed and extent of reform, whereas the opposition movements divided into moderates and radicals. In Hungary, a coalition emerged between the reformers in the leadership and the moderates in the opposition, which produced an agreement on free elections and a noncommunist govern-

ment; in China, radicals seized control of the antigovernment protests in Tiananmen Square, and conservatives seized control of the party establishment. The result was the crackdown on demonstrators in 1989.

For Tong, the critical question is why the reformer-moderate coalition formed in Hungary but not in China. Through a structured, focused comparison, she identifies the following differences in these two societies: (1) the relative strength and sources of conservatives and reformers; (2) the degree of compatibility between the objectives of the establishment and those of the opposition; (3) differences in strategy and structure between the two opposition movements; and (4) the different contexts within which the confrontation occurred. The successful coalition in Hungary was due to a reliance by conservatives on support from the Soviet Union, which proved unreliable; the compatibility between the objectives of the reformers and the opposition; the relatively restrained strategies adopted by the opposition; and the ability of all the political forces to conduct pragmatic negotiations in an institutionalized setting. Such a coalition never formed in China because of the relative strength of the conservative forces in the leadership, the incompatibility between the aims of the reformers and the opposition, the radical moral and emotional orientation of the protest movement, and the intransigence of party hard-liners and the radical opposition alike.

In sum, the book is methodologically sophisticated, innovative, and information rich. It reveals the complicated dynamics of transition, especially the institutional logic that is generic to a shift from state socialism. It should be read both by China specialists interested in reform processes in that country and by general comparativists interested in the dynamics of transition.

**After the Deluge: Regional Crises and Political Consolidation in Russia.** By Daniel S. Treisman. Ann Arbor: University of Michigan Press, 1999. 262p. \$57.50.

Thomas F. Remington, *Emory University*

Daniel Treisman offers an ingenious explanation for the fact that the Russian Federation held together after the collapse of the Soviet regime. Unlike the three other ethnic federations in the communist world—Yugoslavia, Czechoslovakia, and the USSR itself—the Russian Federation, which was the largest of the 15 nationally based constituent republics in the Soviet Union, avoided disintegration. Many observers in the early 1990s feared the same pressures that had led to powerful separatist movements among the Soviet republics would prove too strong for the fragile central government to resist. Yet, Russia managed to maintain itself as a federal state, albeit weak. The one constituent republic in which separatism ultimately led to armed confrontation was Chechnia, where a brutal war began in 1994, paused in 1996, and erupted again in 1999.

Chechnia was a tragedy, as Treisman observes, but it was also an anomaly. Russia’s federal forces launched a military offensive there after the wave of regional challenges to the central authority had subsided, so the ebbing of that wave cannot be ascribed to the deterrent effect of the invasion. Therefore, a different explanation must be found for the center’s ability to establish a rough but workable system of federal relations with its own restive regions. Treisman argues that the answer may be found in the center’s adroit use of selective fiscal appeasement, by which it blunted the edge of the most rebellious challenges through a mixture of tax breaks, credits, and budget allocations. Rather than fuel

demands for greater autonomy, Treisman shows, these fiscal transfers translated into higher per-capita spending in the targeted regions. This in turn boosted public support both for the Yeltsin camp in Moscow and for regional leaders.

There are three elements to Treisman's account. First, by selectively accommodating the most credible rebels among the regional leaders (even at the expense of allowing friendly regions to lose out in the fiscal federalism game), the center had more resources with which to punish less credible challengers. Second, the center's transfers enabled regions to increase per-capita spending in the social sector, an area largely under regional jurisdiction and chronically underfunded. Third, as public support grew in the targeted regions, local leaders were likelier to support Yeltsin and his fellow democrats in their collisions with the communists at the center.

A key test was the fateful showdown in September–October 1993, when Yeltsin dissolved the old parliament and called for new elections. Opposition-minded leaders in the Duma refused to go along, and both they and Yeltsin appealed for support from local leaders. In regions that had benefited from federal transfers and (presumably as a direct consequence) there was public approval of Yeltsin, governors read the poll results and supported him against his enemies. Treisman shows that the use of federal transfers to fuel regional social spending tied governors directly to the Kremlin, implicating them with Moscow in the public's eyes. Therefore, they were more likely to try to maintain a cooperative relationship with the center.

Treisman's thesis is counterintuitive in many respects and is derived directly from a simple formal model of relations between a center and a set of federal units. It assumes a certain disposition to support challenges to the center in each region, and the center's resources for quelling challenges are finite. If the center can funnel rewards to a few regions where the public disposition to support challenges is greatest, and where threats to secede are therefore the most credible, then it becomes riskier for other regions whose rebelliousness is less credible to challenge the center.

The theory is tested empirically using an impressive body of data on the flows of resources between the federal center and the regions in the 1990s. Piecing together these records was a considerable achievement, considering the rapid change in Russian fiscal relations in the early 1990s and the spottiness of recordkeeping and reporting. Treisman also generalizes his argument to other cases, showing that Russia's strategy for defeating disintegrative pressures through the selective use of fiscal levers was precisely what the three other communist ethnic federations failed to do. In Czechoslovakia, Slovakia lost ground in its relations with the center in 1990–91; so did Slovenia and Croatia in Yugoslavia in the same period; so did the Baltic states in their relations with the USSR government. This is not to say that different policies would have prevented these states from breaking away, but it tends to support the proposition that Russia, faced also with serious separatist pressures, took a very different path and held together.

Treisman's account of fiscal federalism in Russia is consistent with other studies treating constitutional arrangements that divide power, such as separation of powers and federalism, as inducements to continuous bargaining among political actors. Leaders with competing electoral mandates use the institutional resources available to them to obtain the best possible terms before ultimately reaching agreements that enable them to benefit from cooperation. Conflict, including much ethnic nationalism, thus is not an indication of system

breakdown or irreconcilable differences but is an inevitable part of the give and take of the political process. The originality of Treisman's study lies in showing that Russia's leaders, whether through strategic calculation or dumb luck, happened upon a formula that enabled them to preserve Russia as a federal state at a critical moment. Treisman presciently concludes by suggesting that it should be the first priority of Yeltsin's successor to recapture administrative control over the federal government's own fiscal authority from lower-level governments. By his actions in summer 2000 to recentralize power at the expense of the regional governors, President Putin seems to have acted on Treisman's advice.

**Feminists and Party Politics.** By Lisa Young. Vancouver: University of British Columbia Press, 2000. 227p. \$75.00.

Jill M. Vickers, *Carleton University*

This comparison of the relationship between organized feminism and partisan politics in Canada and the United States addresses two questions. First, Young asks how much organized feminism has influenced partisan and electoral politics in each country. Second, she asks how political parties in each country have responded to organized feminism. She answers these questions by examining the relationship between each country's largest feminist organization and its party system and by showing how each relationship changed between 1970 and 1997. The result is an important and readable book that demonstrates the value of feminist political science as an approach, especially in comparative politics. The book is head and shoulders above many other texts about feminist political activism, mainly because of Young's ability to bridge between feminist ideas about politics and the comparative politics literature about political opportunities.

Many women question why they should participate in electoral politics when parties and legislatures seem to be dominated by elite men who use their position to advance their own interests. Young poses a somewhat different question. Rejecting the radical feminist thesis that parties and legislators are inherently patriarchal, she argues that, if they are historically male dominated, they can be transformed under some circumstances. What circumstances make transformation possible? Young does not assume that it is simply a matter of women choosing to participate or that their participation will always be translated into feminist policy outcomes. Indeed, she demonstrates that even different forms of democracy present women with different political opportunities because of their structures, practices, and values. Although organized women may choose or reject participation in partisan politics because of their ideological position, collective experiences with their own political system also shape their stance. When women face a political opportunity structure that offers limited access, their views about parties and the electoral system will be less optimistic than when the structure is more open. Despite women's struggles to establish the principle that "democracy without women is not democracy," therefore, it is still assumed that "democracy" can exist without women's participation or presence, either in personnel or in gender-specific programs.

What insights does Young's approach provide for comparing the influence of organized feminism in Canada and the United States over three decades? Despite the widespread suspicion of government in the United States, Young concludes that the National Organization of Women (NOW) has

become more involved in electoral politics and developed close ties with the Democratic Party. In Canada, where there has been more trust of government historically, the National Action Committee (NAC) has moved from a cautious, multiparty engagement with electoral politics to less involvement and a more radical, antiparty stance. In these two organizations, more of the Canadian than U.S. feminists concluded that “power is not electoral” and advocate activism outside the electoral system.

Young’s thesis is that both ideology and women’s collective experiences in each country are needed to explain the different trajectories. The U.S. movement—seen through the prism of NOW at least—has been dominated by liberal feminists who are more trusting of parties and electoral politics. The Canadian movement—seen through the prism of NAC—has been more influenced by radical and socialist feminism, which moved it away from partisan involvement. (NAC also went from being largely funded by the federal government to minimal state funding, which permits greater radicalism.)

Young demonstrates the importance of moving beyond ideology to explore women’s collective experiences with the political opportunity structure. She concludes that the U.S. congressional system, with its weak political parties and more independent legislators, provides stronger incentives for feminist participation than does Canada’s closed parliamentary system, whose exclusionary and strongly disciplined parties provide less opportunity for infiltration. The passage in 1982 of the Charter of Rights and Freedoms, which included a sex equality clause and explicitly sanctioned affirmative action, opened up the attractive alternative of legal remedies. The movement’s success in gaining these clauses as well as government funding for organizations seeking to influence the courts, made quite clear the value of having women who favor feminist goals in party and elected positions—and on the bench. (At one key point, there were three women judges with women-centered perspectives on the Supreme Court.)

Young’s systematic comparison of structural and ideological differences shows the strengths of a feminist political science approach. Her research demonstrates, for example, that although the Canadian New Democratic Party (NDP) responded more positively to feminist policy demands than the Democratic Party, a nongoverning party’s inability to deliver in a parliamentary system was a negative experience

for Canadian feminists. Another was that Progressive, Conservative, and later Liberal governments co-opted prominent women but did not adopt NAC’s feminist policy agenda. By contrast, the electoral gender gap in the United States made Democrats more open to adopting NOW’s policy as well as to co-opting women. Young notes, however, that in both countries parties were more open to participation by nonfeminist women, who represented “women” symbolically, than they were to feminist goals. This validates Young’s thesis that transformation of the male dominance of electoral politics is possible, but it will not be achieved simply by women choosing to participate in parties and electoral politics.

There are few weaknesses in this excellent book. One is Young’s tendency to focus on majority culture (white, anglophone) feminists, with little attention to other women-centered political movements. I would have valued more discussion of the views and experiences of U.S. black and Hispanic women, who are marginalized in society compared to white feminists. We know from gender gap analysis that these large minorities behave differently in important ways from majority whites, who can afford the luxury of “stand-alone feminism” focused exclusively on gender issues. More attention to Franco-Quebec feminists would have strengthened the Canadian analysis, especially since NAC failed almost from the beginning to incorporate those groups, who were drawn by the nationalist debate to focus on the Quebec state. Moreover, after 1995, NAC was led by a coalition of “women of color,” lesbians, and women with disabilities; as NAC increasingly represented marginalized women, it was increasingly marginalized, losing especially support from elected and partisan women and women in the media.

Both cases suggest that the relative power or lack thereof of women’s organizations shapes their views and experiences with parties and electoral politics. Feminist political science, therefore, needs to move beyond “women” as its central category of analysis to consider how women who are different because of their minority “race,” language, or nationality experience opportunities to participate differently. This means recognizing that women are not uniformly powerless, as some feminist ideology suggests. These are minor quibbles, however, about an excellent book that should be read by every political scientist interested in United States, Canadian, and comparative politics, as well as by every feminist activist grappling with questions about politics.

## International Relations

**The Spy Novels of John le Carré: Balancing Ethics and Politics.** By Myron J. Aronoff. New York: St. Martin’s, 1999. 316p. \$49.95 cloth, \$21.95 paper.

John Nelson, *University of Iowa*

Literary forms of inquiry into politics range far beyond the journal article and the scholarly monograph. Myron Aronoff’s monograph serves us well by respecting the novel of international intrigue as an insightful form for analyzing the politics of diplomacy, bureaucracy, covert action, and international regimes. Aronoff targets the latter-day dean of spy novelists, David Cornwell, who writes under the name of John le Carré.

These days, spy novels have trouble gaining serious attention even from literary critics. Their popular form often

prompts disdain, even though the likes of Graham Greene and le Carré are conceded high marks for their literary structures and styles. The latest obstacle is the end of the Cold War. This has led some reviewers to the strange supposition that espionage and other modes of intrigue have ended—or at least stopped being useful as devices for addressing dynamics of international relations. On the way to lambasting le Carré’s *Our Game*, no less a figure than John Updike states that “the end of the Cold War should have put an end to Cold War thrillers” (*New Yorker*, March 20, 1995, pp. 102–3). Aronoff knows better.

My own interest is in le Carré’s attempt to make sense of the international politics taking shape in the wake of the Cold War. His novels of the 1990s provide one telling analysis after another of modes emerging for states, nations, militaries, economies, communications, ecologies, and migrations throughout the planet. Aronoff’s concern is more with le

Carré's "ambiguous moralism." This is Aronoff's name for the problematics of political action in our times by individuals who know too much for some pure idealism to seem plausible, but who care too much for sitting on the sidelines to feel responsible.

Le Carré has enjoyed one of the more sustained and successful careers of political analysis in novel form, and Aronoff traces this search for "skeptical balance" throughout his writing. As Aronoff argues, le Carré's most famous creation, George Smiley, epitomizes this posture, which makes him one of the more complicated characters to recur in the imaginative annals of postwar action. The first seven chapters keep coming back to Smiley, testing his attempts to strike a balance between the moral and political imperatives that confront western democracies—and especially modern individuals—with one dilemma in action after another. The eighth chapter compares fictional intrigues to "the Real World of Espionage," and the ninth chapter casts brief glances toward the novels of the 1990s. The book concludes with exceptionally helpful notes, a fine index, and a roster of dramatis personae for the le Carré spy novels that can stand any fan in good stead.

This is a work of liberal humanism. Aronoff's terms of art are a clear indication: balance, ambiguity, means and ends, individuals and institutions, dilemmas, skepticism. Certainly, this fits le Carré as the prime heir to Graham Greene. In their skein of liberal tradition, the practical details matter. Accordingly, Aronoff devotes particularly effective chapters to le Carré's portraits of bureaucratic politics, domestic as well as foreign, and to his cumulative account of espionage as both a culture and a craft. This binocular focus brings out the political depth of le Carré's settings. These stay informed almost up to the minute, and they manage an intelligence about international relations that makes his novels a good education even for professional students of statecraft and soulcraft.

Aronoff concentrates mostly on the soulcraft. He is fascinated by characters such as Smiley. He wants to know how they balance idealism and realism, how they combine sentiment and skepticism, how they manage loyalty and betrayal. The issue for Aronoff becomes whether such a morally ambivalent and politically ambiguous figure as Smiley can be appreciated as a hero—or even a human. The final chapter poses these questions directly, and it answers them emphatically in the affirmative. Aronoff's book plays this familiar game of humanism in terms at once insightful and persuasive. As they say on the cover of popular potboilers, it is a good and enjoyable read.

In a way, however, the game stays a little too familiar. It is no surprise to anyone at this point that the spy can be a human and a hero. But is the novelist and literary theorist Samuel Delany right to suggest in his *Nevèryon* tales that the hero in Western civilization must always be a spy? Is le Carré himself right to imply in his novels of the 1990s that a spy is simply a politician by another name and set of means? Might those same novels supplement individual actors with structuralist and poststructuralist sensibilities about regimes of transnational relations? It can be fun and instructive to read the best of liberal humanists for their leanings also toward postmodern politics, and this seems especially appropriate for a novelist who pays such sophisticated attention to the kinds of political institutions we have been constructing for the twenty-first century. Perhaps there is need or at least opportunity for a sequel from Aronoff. That would be a pleasure to anticipate.

**Open-Economy Politics: The Political Economy of the World Coffee Trade.** By Robert H. Bates. Princeton, NJ: Princeton University Press, 1997. 221p. \$59.00 cloth, \$18.95 paper.

John S. Odell, *University of Southern California*

For three decades political scientists have attempted to show that markets reflect the political institutions and politics within which they function. Also, many scholars have traced states' foreign economic policies to their domestic politics. *Open-Economy Politics* pushes both these projects forward with an extended case study of the world coffee market. Beginning in the late nineteenth century, Robert Bates takes us chronologically through key shifts in policies of the chief coffee trading countries—Brazil, Colombia, and the United States—especially the formation, operation, and collapse of the International Coffee Organization from 1962 through 1982.

The central argument appears on page 7: "What is required for the study of international political economy is a domestic theory of politics. Indeed, I will demonstrate that the foreign economic policies of the great powers that created the International Coffee Organization [ICO] represent the product of domestic political struggles, . . . a political process that is structured by institutions."

The book's most important theoretical contribution arises from what Bates means by institutional effects. He does not mean that centralized governments will tend toward one policy, and federal states with divided powers will tend toward another (p. 163). Rather, he emphasizes the incentives different institutions create for producers and politicians living in open economies, as well as the resulting behavior of these individuals toward one other. For example, although Brazil had become the dominant world coffee exporter by the 1880s, it did not use its market power to maintain the price until 1906 (chap. 2). The lag is a puzzle for the economic theory of cartels and for political hegemony theory. Earlier efforts by producers in São Paulo state to get their government to intervene in the market failed because of Brazil's federal structure, according to Bates. São Paulo needed support from other states. Brazil began to act like the unitary actor assumed by systemic theories only when coffee politicians hit upon a side payment (currency depreciation) that attracted sufficient support in other states.

Colombia entered the world market after 1906 with a deliberate strategy of taking a free ride on the price floor Brazil was enforcing by itself (chap. 3). During the depression Colombia spurned Brazilian appeals to accept some of the burden. These choices also resulted from a fascinating political story. The Colombian government wanted to cooperate with Brazil, but Colombian coffee growers defeated their own government's efforts in three different policy domains. They could do so for two reasons, Bates claims. First, politicians formed a producers' association that overcame their own collective action problems. Second, in Colombia during this period there was serious competition between two political parties, and the coffee growers played one off against the other.

Building on what is already known about the ICO's formation, Bates (chap. 4) adds the insight that, after World War II, both Brazil and Colombia had more centralized political institutions than before. Politicians in both countries were better able to exploit their coffee industries for the sake of national development. But after efforts to form a cartel failed again in the late 1950s, exporters turned to the United States to help enforce a price floor. To earlier accounts of U.S. support, chapter 5 adds that Congress delayed the commit-

ment's implementation for three years, another lag that is explained by domestic politics.

Chapter 6 offers an ingenious argument, supported by primary research, to explain how the ICO maintained a stable equilibrium for twenty years. Exporters negotiated long-term contracts that gave lucrative discounts to large U.S. coffee roasters, such as General Foods. These in turn lobbied Congress to support the organization that held up world prices (which smaller competitors paid in full) at the expense of U.S. consumers.

The book uses the method of deploying theories to interpret a narrative. Bates considers five alternative theories but finds each lacking in some respect. In addition to cartel theory and hegemony theory, he reports anomalies for dependency theory, the "new trade theory" of imperfect competition, and Rogowski-Frieden arguments that emphasize international markets as cause and domestic politics as response.

The presentation could have been more effective in a few places. When interpreting Brazil's key 1906 intervention in the coffee market, Bates neglects to describe who did it and how (p. 37). The actor was the São Paulo state government, according to Stephen Krasner ("Manipulating International Commodity Markets: Brazilian Coffee Policy 1906 to 1962," *Public Policy* [Fall 1973]: 498–9). A chapter emphasizing federalism might have clarified why federal legislation and the package deal were necessary. Furthermore, since São Paulo itself would benefit from currency depreciation (the side payment), it is not clear why other states would have "paid" for it with support for the new coffee policy. The point about party competition in Colombia during the 1930s is not supported with specific evidence showing the two parties' relative strengths, which makes it more difficult to evaluate the claim that coffee growers were pivotal. After finding that one particular game-theoretic interpretation falls short, the book generalizes too quickly to all of them ("game theoretic approaches too thus prove unsatisfactory" (p. 160)).

These quibbles notwithstanding, this study makes significant original contributions. The extended case study turns up theoretically relevant insights that would have been missed otherwise. The reach of the argument across so many decades is impressive. This is the first book to read on the political economy of coffee. The analytical approach of looking for ways in which institutions shape markets and political behavior in open economies should be fruitful in many other cases as well. Finally, Bates is part of a group working to show that rational choice theories and narrative history can enrich each other. (See the debate over their 1998 book *Analytical Narratives* in *APSR* 94 [September 2000]: 685–702). The group's efforts to transcend unproductive tribal warfare between academic schools deserve enthusiastic applause.

**The Spiral of Capitalism and Socialism: Toward Global Democracy.** By Terry Boswell and Christopher Chase-Dunn. Boulder, CO: Lynne Rienner, 2000. 281p. \$55.00 cloth, \$23.50 paper.

William R. Thompson, *Indiana University*

In introductory international relations courses, we were once accustomed to contrast three alternative approaches: realism, liberalism, and Marxism. The collapse of the Soviet Union and the proclaimed triumph of liberal politicoeconomic ideas has led to a deemphasis on the third paradigm or, in some cases, its substitution by constructivism. But, contrary to Fukuyama, history has not quite ended. Neo-Marxist interpretations of international relations persist, and new and

interesting ones continue to emerge. The latest entry, Boswell and Chase-Dunn's new book, is a case in point. As long-time and leading contributors to world systems theory, they employ their theoretical interpretation of modern history (the last 500 years) to explain what went wrong with socialism and how the socialist strategy might still be salvaged in a future world-system (with a hyphen).

The book is divided into six chapters. The first gives an overview of the authors' version of world-system dynamics, with an emphasis on the continuities of a capitalist world economy, an interstate system, and a core-periphery hierarchy. Within these structures, five long-term trends have proceeded: commodification, proletarianization, state formation, increases in economic enterprise size, and capital intensification. Rounding out the focus are several cyclical phenomena, including globalization, long waves of economic expansion and contraction, and hegemony. In brief, it is the interaction of these systemic trends and cycles within the world-system structures that produce the dynamics of change.

The second chapter surveys revolutions within a world-system context. The emphasis is placed on "world divides"—the eras of upheaval that occur in between movements from one type of accumulation regime to another. The outcomes of these struggles, it is claimed, have altered periodically the nature of capitalism. In particular, living standards and political rights for labor have been enhanced. The spiral of capitalism and socialism then results from transformational opportunities in periods of organizational breakdown. No improvement or progress is guaranteed, but the probability of progress has at least been increased during these windows of upheaval and opportunity.

The last four chapters focus primarily on the past and future of socialist strategies. The basic arguments are that these strategies were adopted by semiperipheral actors seeking upward mobility in the world economy. The strategies worked to a point but ultimately imploded in the face of stagnation and changing modes of economic production. One of several problems with the strategies was that socialist economies were adopted in some national economies. To succeed in a capitalist world economy, socialism must be adopted on a systemic basis. Similarly, contemporary resistance to globalization tends to be nationalistic and is therefore equally doomed to fail.

To develop socialism on a global basis, and to resist more successfully the costs of globalization, the authors propose a "global democracy" strategy. The spear carriers would be social movements operating in regional and global arenas, as opposed to national ones. Labor unions, women and children rights movements, and green environmental groups, all contending as transnational actors, could transform the nature of world governance by working toward improved standards of living and expanded rights, as well as the creation of global institutions with agendas that go beyond conserving traditional capitalism. The outcome would be greater national and global democratization.

There is much to approve in this analysis, just as there is much with which to disagree. If one is more concerned about analytical dynamics than outlining future social movement strategies, chapters 1 and 2 could easily have been expanded into several more chapters. This would have permitted space for a more detailed examination of the basic world-system model. It combines elements of now conventional world-system dynamics with greater explicit attention to transformational principles and agents. Such an examination could easily have been a book in itself, and it is hoped that the authors will return to the elaboration of these theoretical issues in the future.

Another point of disagreement worth highlighting is the source of inspiration for some of the arguments. Boswell and Chase-Dunn believe that progressive labor and other social movements were critical in transforming national capitalist systems and strategies. They may well be right, but their argument would have been enhanced if they had developed this interpretation further. By showing how, under what conditions, and to what effect social movements altered national political economies, they might have better demonstrated how these processes might or might not work similarly at the global level. At the same time, more attention to nation-level transformations might also suggest that a perceived struggle between capitalism and socialism has become an increasingly obsolete way of framing the problem. The authors would have done better simply to emphasize the democratization dimension already present in their argument.

But continuing disagreements over how to frame world-system dynamics (with or without the hyphen) is precisely the overall point. Disputes about ways to analyze global dynamics will persist. So, too, will disagreement about the relative superiority of political-economic and social strategies. The triumphalism of the Cold War ending was something of a mirage. True, liberal democracies defeated aristocratic, fascist, and communist autocracies over the span of the twentieth century. But there are still many major policy problems to resolve, and people will continue to disagree about how best to manage global problems. However one feels about the relative virtues of socialist strategies per se, Boswell and Chase-Dunn definitely contribute to the theoretical “globalization” of our perspectives about what sort of critical processes and dynamics we are attempting to survive in the short run. In the longer run, the goal of a better world in which to live can hardly be sneered at. Just how we will attain that better world will be a leading question of the twenty-first century.

**Maneuvers; the International Politics of Militarizing Women's Lives.** By Cynthia Enloe. Berkeley: University of California Press, 2000. 418p. \$45.00 cloth, \$17.95 paper.

Mary Fainsod Katzenstein, *Cornell University*

When I was an undergraduate in the 1960s, as the Vietnam conflict was escalating, I took Stanley Hoffmann's mesmerizing course, “Causes of War.” I thought back to this class as I read Cynthia Enloe's book, which deserves all the superlatives it has accrued. The experience of reading now and remembering back left me wondering: Without Enloe to consult (her first book on militarism and gender came out in the early 1980s), what were we missing in Hoffmann's class? The answer, I think, is this: We could understand well enough the contending theories about why nations go to war; but in the absence of Enloe, we were less able to ask how militaries could manage such massive mobilizations that required the often calamitous sacrifice of precious lives even for wars whose purposes seemed remote or unconvincing.

Militarism and its gendering, Enloe argues, prepare the ground for mobilization. If men and women are to go to war in whatever combat or noncombat capacity or are to encourage or suffer the tolls of war on themselves and their loved ones, then their identities must be militarized. A good mother will be one who wants to send her son not to school or to work but to war; a good son or husband is one who proves his manliness not so much on the sports field or behind a plow as in uniform; a good “militarized” prostitute is one who will have sex with whomever it is in the military's

interest for her to do so. Thus, militarized gender is like nationalism. Benedict Anderson writes that an imagined national identity sows the seeds of battle: The idea of the national community has made “it possible over the past two centuries for so many millions of people not so much to kill, as willingly to die for such limited imaginings” (*Imagined Communities*, [1983] 1996, p. 7). Enloe's work speaks of an imagined and “idealized” gendering, one suited for war. The brave male soldier, the wife who serves in wartime factories when the nation needs her, the prostitute who gives comfort to soldiers away from their loved ones, the nurse who tends the sick and dying without attending to what they are dying for. When gender identities are militarized—when they are “controlled by, dependent on or derive [their] value from the military as an institution or militaristic criteria” (p. 291)—it is more likely, Enloe argues, that violent conflict can occur.

For any core course in international relations, Enloe should be mandatory reading. Those already among Enloe's wide readership will know some of this text's central arguments, but *Maneuvers* offers a trove of new insights. A thesis even more powerfully developed here than in Enloe's earlier writings is the title of the book—how policymakers maneuver to make strategic choices. By emphasizing the purposefulness of policy choices, Enloe shows how the very different experiences of women located in varied ethnic, national, class, and occupational contexts are tailored to the needs of militarism, a project that is not always consistently successful, as she observes. But *Maneuvers* has more than a functionalist lesson; by emphasizing policy choices and variability across time and national context, Enloe shows that militaries are not governed by primeval identities. Gender identities must be created, including even those revealed in wartime rape, which Enloe argues are too often mistakenly presumed to be caused by “raw primal misogyny” (p. 134).

The policy choices made to foster and routinize prostitution (“Prostitution seems routine. Rape can be shocking” [p. 108]), which effectively disguise what often should be recognized as institutionalized rape, are some of the most vivid descriptions in the book. Yet, even as Enloe writes about such emotionally laden terrain, she never mocks or derides, never oversimplifies or closes the reader off to the complex motives and human pathos associated with even some of the most dire acts. From her description of the much publicized 1995 rape of a young Okinawan schoolgirl for which three American soldiers were indicted, one comes to understand the perspectives not only of the outraged Okinawan protesters but also of the mother of one of the soldiers and the American admiral whose career prospects crumbled when he said to the media, in criticizing the “stupidity” of the three soldiers: “For the price they paid to rent the car, they could have had a girl” (p. 117).

As in her previous books, Enloe insists on the importance of not just studying women in the military but of understanding the militarization of women's lives everywhere—in and out of uniform, in the United States, the Philippines, Bosnia, Afghanistan. Indeed, her description of the many diverse international expressions of the militarization of gender is one of the most important contributions of this book. Her knowledge and observational powers are formidable and impressive for their specificity and accuracy. Whether she is discussing the star wars satellite-shaped pastas in a can of Heinz tomato and noodle soup, which many mothers might hope will persuade their sons to like their lunchtime soup, or the question of whether male marines should be allowed to carry umbrellas, or the dollar-per-day renting of a Thai woman outside the base gates (known as a “teafuck”

[p. 231]), Enloe never lets us forget the “normalization” of militarism.

Enloe makes her readers see differently. Next time you visit Washington, stop by Walden Books in Dulles Airport, terminal D. With the kind of curiosity Enloe instills, you will not fail to notice that under the categories “history,” “world history,” and “American history” (unless the shelves have been rearranged), easily 80% of the books are about militaries. Enloe’s *Maneuvers* is featured on bookshelves in Sarajevo, Tokyo, Delhi, and Sydney. You may not find it in terminal D, but be sure to read it.

**Politics: Authority, Identities, and Change.** By Yale H. Ferguson and Richard W. Mansbach. Columbia: University of South Carolina Press, 1996. 476p. \$49.95 cloth, \$24.95 paper.

Claudio Cioffi-Revilla, *University of Colorado*

This complex, ambitious, and large book seeks to question, reformulate, and enhance the scope (and methods) of international relations theories, particularly those formulated within a realist framework. The authors question that framework, the Westphalian model of putatively unitary nation-states, quantitative methods of empirical investigation, and the levels-of-analysis paradigm. The breadth of their critique is extensive and, consequently, highly ambitious. Rather than approach international relations as a system of unitary nation-states inspired by realist principles, which is arguably a partial and simplified portrayal of contemporary research, the authors favor a complex system of “overlapping, layered, and linked polities” that have both horizontal and vertical dimensions. The latter dimension is particularly important, as it concerns patterns of authority, identity (including ethnicity), and institutions that compete for the loyalty of individuals.

The fifteen chapters are grouped into three parts: a theoretical introduction, historical applications, and conclusions. The first two chapters critique what may be called “empiricist Westphalian realism” and propose a framework of nested polities. The next twelve chapters present six historical case studies on major polity systems of the past—Mesopotamia, Greece, China, Mesoamerica, Islam, and ancient Italy—with the first chapter in each case being dedicated to the “horizontal dimension of politics” (spatial interactions in terms of diplomacy, warfare, trade, and the like) and the second chapter making the case for the “vertical” dimension (the “overlapping, layered, and linked polities”) within the horizontal space. This structure provides the book with a high degree of organization, which seems vital, given the ambitious objectives. The summary chapter returns to the main themes introduced in the first two chapters.

*Politics* is highly original because it presents a challenging framework with several praiseworthy features. First, it stretches the temporal scope of most contemporary theory and research, which is based mostly on the recent historical past and, consequently, has a myopic view of the present and the future. As the authors demonstrate, and a few other political scientists would agree (myself included), international politics is a phenomenon of ancient origin and long evolution; it is not a recent pattern of behavior. Second, the book demonstrates the existence of international relations and world politics in non-European international systems, such as East Asia and Mesoamerica. This is essential for establishing the cross-cultural universality of patterns. Third, it addresses the complex phenomenon of nested polities and conflicting loyalties in world politics, a very real phenomenon

with observable consequences in the post-Cold War international system. Fourth, although the authors did not intend this, the framework also offers some new scientific challenges and opportunities for quantifying the past and testing new intriguing hypotheses. For example, empires are nearly universal polities that can be systematically measured and investigated, as Rein Taagepera (not cited) and others have demonstrated.

The originality and ambition of this book make it important not only for IR scholars but also for political and social scientists in general. Its style is eminently readable, if not always totally accurate (for example, the characterization of empirical approaches is somewhat simplistic, the much criticized levels-of-analysis framework is in fact used in the so-called vertical dimension of politics, and aspects of ancient history and long-term change could be stated differently). Regardless, this is an important book.

There are no other recent books like this written by American political scientists, although several scientifically oriented scholars (e.g., M. I. Midlarsky, G. Modelski, R. Taagepera, W. R. Thompson, D. Wilkinson, and certainly this reviewer, among others; none of them cited by the authors) have published on the origins and long-term evolution of international relations. A recent similar work by a British political scientist is K. R. Dark, *The Waves of Time: Long-Term Change and International Relations* (1998), although the framework here is quite different (Dark is also a theoretical archaeologist). Archaeologists have produced some important works related to the problematic of this book, among them G. M. Feinman and J. Marcus, *Archaic States* (1998), J. A. Tainter’s classic, *The Collapse of Complex Societies* (1988), and a host of monographic regional works on the case studies included in this book.

Ferguson and Mansbach are political scientists known for their interest in historical change as well as for their skepticism regarding contemporary scientific approaches to IR theory and research. Ferguson is also known for his work on ancient Greece. Both authors have collaborated before on substantive and epistemological issues in the field, so this book may be seen as a recent product in their on-going joint project.

*Politics* is written primarily for IR scholars, but the issues raised should also be of significant interest to scholars of comparative politics and case study methods. In fact, many of the topics addressed, as highlighted in the first and last chapters, affect all contemporary multiethnic polities. The Balkans, the former Soviet Union, Latin America, the EU, and even the United States are good examples of polities with a significant “vertical” dimension (and levels of analysis). The book can certainly be used as a “Devil’s advocate” within a graduate survey in IR. Its use in undergraduate instruction is more problematic, but feasible. (I have used it in an honors Introduction to IR; the “commoners’ section” cannot handle it.) The background assumed by this book is considerable, particularly if one is to derive maximum profit from both the proposed framework and the case studies. At a minimum, it assumes familiarity with contemporary IR theory and research; basic scientific epistemology; case study methods; and a considerable range of prehistoric and historic materials for the six case study areas.

While *Politics* has numerous praiseworthy features, as should be apparent from the above remarks, some of its limitations tend to hinder the achievement of its full potential. Certain of these are substantive, others are stylistic or technical. On the substantive side, there is far too much reliance on secondary and tertiary sources (including textbooks) and not enough on the specialized primary sources (archaeology and epigraphy), which are essential in a project

of this nature, however challenging the use of primary sources may be. For example, the primary materials in the works of K.-C. Chang, D. Keightley, L. Liu, D. Nivison, A. Underhill, and other scholars of early China demonstrate that the emergence of political complexity in China dates to the Xia (Hsia) state, starting in about 2100 B.C. at the latest, not the much later Zhou (Chou) periods chosen by the authors. This is a difference of 1,000 years of political evolution, which is significant even in terms of China's long history. Similarly, primary sources demonstrate that political complexity—the rise of state-level polities—in West Asia had twin origins in Mesopotamia (southern Iraq) and the Susiana (western Iran), not just the former. Moreover, chiefdom-level polities originated thousands of years earlier, and it was Uruk that “created the first large regional Mesopotamian polity” (G. Algaze, W. Hallo), not Agade under King Sargon (p. 67). Again, this is a difference of ca. 2,000 years, which cannot and must not simply be ignored when the focus of investigation concerns the origins and evolution of polities. These are not issues of marginal chronological detail; they have to do with the main claims of the book, as presented in the first chapters and summarized again in the set of questions in the concluding chapter (p. 381). There are also some puzzling lacunae, such as the exclusion of precontact South America, where the Andean system of polities was as significant as those that developed elsewhere in the world, from Chavín to the Inca empire. These and other substantive problems are probably caused by the inclusion of too many general and tertiary sources to the exclusion of more specialized primary sources.

From a methodological perspective, the case selection criteria are not clear, because at least one major “primary” polity system is omitted (Andes), almost all polity systems skip over the truly formative stages (e.g., the transition from chiefdom to state-level polities), and insufficient attention is dedicated to the key issue of case selection. For example, the distinctions, similarities, and overlaps among areas of Mesopotamia, the Levant, Hatti, and the Susiana in West Asia are no clearer than those among the Olmec, Maya, Zapotec, or Aztec areas in Mesoamerica. Case units such as China, Greece, Mesoamerica, and Italy are in many respects incomparable without rigorous conceptualization and empirical operationalization, at least not on the same polity scale, even allowing for the “vertical” phenomena of nesting and layering. Far more work with primary sources needs to be undertaken. Such work will eventually provide us with systematic, comparable, and empirically reliable descriptions of these early polity systems.

On the stylistic side, the authors should be commended for the use of maps (IR needs far more in the area of historical cartography), although some errors escaped their attention (e.g., Umma was not an empire in the 24th century B.C., and ca. 1700 B.C. the shore of the Persian Gulf came close to Ur, not where it is today [p. 69]; Palenque is not located in the Gulf of Mexico [p. 226]). Yet, for a work of this nature the lack of chronological timelines is somewhat troublesome, particularly for students who lack solid historical background across the case areas. Brief tables with “key dates” (pp. 67, 170–1, et seq.) lack the much richer and accurate information that only side-by-side cross-polity timelines can convey.

On the whole, *Polities* is a highly provocative and stimulating book, one that should inspire the fields of international relations and comparative politics to investigate more ambitious horizons through collaborative efforts, including interdisciplinary approaches and methods, to understand better the present and future of world politics.

**Immigration and European Integration: Towards Fortress Europe?** By Andrew Geddes. Manchester: Manchester University Press, 2000. 196p. \$69.95 cloth, \$24.95 paper.

Jeannette Money, *University of California, Davis*

Andrew Geddes provides a European analysis of European migration policy. He asks two questions: To what degree has the European Union (EU) garnered control over migration policies of member states? What is the policy outcome? In answering these questions, the author makes two contributions to the literature.

First, Geddes documents the evolution of EU control over migration policy, from the origins of European integration through ratification of the Treaty of Amsterdam. (I refer to EU in the text, although this entity has had different names at different times.) He correctly points out that migration policy involves both intra- and extra-European population movement. Free movement for Europeans was central from the original Treaty of Paris (1951), which founded the European Coal and Steel Community, and its “constitutionalization” and “institutionalization” gradually evolved. Although the original texts were ambiguous, citizenship in a member state became the criterion for free movement. Ultimately, European citizenship was created in the Treaty on European Union (1992), which granted all citizens of member states the right to reside (and work) in any member state. Therefore, third country nationals (TCNs) do not qualify.

As for the EU's extra-European migration policy, the Single European Act (1986) created a single market and, more important, the Schengen Agreement (beginning in 1985) broke down internal frontiers, both of which led member states to cooperate on immigration and asylum policy. Geddes describes the gradual incorporation of these decision-making arenas into the EU's institutional structures, but intergovernmental cooperation was accompanied by “a resistance to integration” (p. 67), so these arenas remain subject to national control through rules that require unanimity for policy change. Moreover, according to Geddes, the shift to EU policymaking reduced democratic and judicial oversight in member states, which could increase internal security measures with respect to extra-European migrants. His account summarizes the trajectory of EU migration policy and provides a useful and detailed history of the changing institutional locus of migration policy decisions.

Second, Geddes provides a careful description of policy outcomes, especially those associated with TCNs. He discusses the “co-existence of restrictive and expansive tendencies in immigration policies” (p. 172) and the securitization of immigration policy. This is a useful corrective to such authors as Yasemin Nahoglu Soysal (*Limits of Citizenship*, 1994) and David Jacobson (*Rights Across Borders*, 1996), who emphasize the advent of a “postnational” citizenship. Geddes is not alone in his observations (see Gallya Lahav, “International vs. National Constraints in Family Reunification Migration Policy,” *Global Governance* 3 [September–December 1997]: 349–73, for an earlier analysis of the restrictive elements of European migration policy), he reminds us that restrictions on TCNs are still considerable.

Having established the unequal treatment of TCNs in chapters 2 through 5, Geddes devotes chapters 6 and 7 to an evaluation of immigrants' efforts to extract more equal treatment from member states, using the EU as an institutional level. He points to political opportunity structures as a determinant of organizational efforts. Again, his conclusions differ from postnational analyses, which emphasize principles of human rights. Geddes argues that, because free market principles underpin free movement in Europe, immigrant



lobbies build on those principles to legitimate their demands. This is a persuasive argument that complements rather than contradicts Soysal's and Jacobson's focus on human rights, international law, and the courts as mechanisms for expanding migrant rights. Despite careful attention to the institutional context of migration policy, the author fails to provide a clear theory—and therefore no predictions—about either the degree of European integration on the immigration policy dimension or the inclusiveness of future policies.

Ultimately, the book makes primarily an empirical rather than a theoretical contribution. The author, in chapter 1, argues that he is moving beyond the theoretical dichotomy of intergovernmentalism and supranationalism by adopting a “multi-level approach,” which documents that “powers and authority are now shared” by local, national, and supranational institutions (p. 37). His description of the different trajectories of free movement versus immigration and asylum policies is useful empirically but disappointing theoretically. The variation across policy arenas suggests that we need a theory to explain why most of the powers and authority for free movement are vested at the European level, whereas most authority over immigration and asylum remains at the national level. Geddes argues that the process of integration has not been uniform, and sovereignty is variously dispersed at local, national, and supranational levels for different policies. Without a theoretical framework, however, we cannot understand why and cannot predict future policy courses.

Finally, there are contradictions in the presentation that are glossed over rather than explored. Immigrant lobbies maintain that current policy, which provides national social benefits to immigrants based on residence but requires citizenship for supranational benefits, is “illogical.” But few political scientists would argue that logic is the driving force behind most political decisions. Also, Geddes argues that EU institutions reflect a “democratic deficit” and require greater citizen participation; yet, the more generous efforts to integrate migrants are attributed in part to the EU's insulation from local political pressures.

These unexplored contradictions point to an underlying tension in the text. The author correctly chides (some of) the literature for its normative bias and argues that an empirical analysis is necessary. Moreover, he states that “policy needs to be based on a valid theory of cause and effect” (p. 24). Yet, because Geddes provides no theoretical framework, he cannot do more than caution against optimistic expectations for immigrant policy in Europe: “It is not possible to prejudge the outcome” (p. 169).

One might chide the author for some of his interpretations, but the basic empirical analysis is solid. If there is little by way of hypotheses and hypothesis testing, the book does not deceive. It lays out empirical questions and answers them with a careful review of the multilayered institutions governing free movement, immigration, and asylum policy in Europe. It will be widely cited by those interested in EU migration.

**America and Political Islam: Clash of Cultures or Clash of Interests.** By Fawaz A. Gerges. New York: Cambridge University Press, 1999. 282p. \$59.95 cloth, \$18.95 paper.

K. A. Beyoglow, *Marine Corps Command and Staff College*

The central theme of this book is that U.S. strategy in the Middle East is fundamentally flawed but not irreparable. This may be the result of the inherent mismatch between strategy and policy and, more significantly, between America's principles and interests. In particular, the author argues that the U.S. approach toward Islam is “beset with ambiguities and tensions” (p. 3). Furthermore, he stresses that there is a

somewhat dangerous growing gap between the American people and their representatives in Congress, on the one hand, and presidential administrations, on the other, when it comes to dealing with “islamists” (those who espouse greater religious activism in politics). The former lean toward a confrontational attitude that is fed by cultural differences, stereotyping, and negative images of Muslims, whereas the latter strive to accommodate or tolerate a majority of moderate or pro-West Islamic forces and states.

The crux of the problem is that American presidents have gone out of their way to paint the majority of Muslims in a positive light, arguing that Islam is not the new “ism” in the post-Cold War world replacing communism, and therefore the United States should not have policies focused on religion, but they have not gone far enough. According to Gerges, they could have pressured America's allies in the Muslim world into more substantial political reforms and could have held the latter more accountable on domestic political reform. The United States opted not to do so for fear of upsetting the strategic partnership with these allied regimes. The result has been a policy of equivocations and inconsistencies. The other side of the coin is that American policymakers have been reluctant to apply the basic tenets of democratization to the Middle East, where only 25% of Muslims live, because the stakes are much higher there than in other regions (e.g., Asia-Pacific), where the majority of Muslims reside. Israel and oil tend to skew American policy toward seeking influence and power in the Middle East and North Africa instead of the focus on democracy pursued in the Pacific basin.

As an alternative, Gerges recommends that U.S. policymakers bet on reform-minded “islamists” worldwide, but particularly in the Middle East, in order to help bring about healthy, peaceful, and orderly change. After all, political reformists in this part of the world seek a better standard of living for their societies, an end to corruption and arbitrary rule, and pluralism—objectives inherent in America's principles and moral makeup. This book implies that not much will change, however, until the United States comprehends fully that the current political struggle in the Muslim world is really a struggle between civil society and the status quo, and between military authoritarianism or totalitarianism and democratic idealism.

According to Gerges, successive American administrations have gotten only part of the picture right. To its credit, the Clinton administration realizes that it is not on a collision course with Islam, but only with a handful of Muslims bent on using Islam as a vehicle for violence and nonconventional political participation. Yet, America needs to go a lot farther to ensure that its principles of compassion and liberal idealism are coordinated with its vital economic and security interests. This will not be easy for two reasons: (1) American presidential administrations will continue to face strong domestic opposition to any abrupt change toward reformist Islam, and (2) as Gerges correctly implies, not all islamists share America's vision of human rights, democracy, and economic globalization. U.S. policymakers, however, should not judge the current theocratic elite in Iran or Muslims elsewhere dichotomously, since categorizations such as radicalism and conservatism, status quo and revolution, and moderate and militant are at best misleading. A better measure of political attitudes is the saliency of sociopolitical and socioeconomic issues. Some issues unite Muslim factions, and others divide them. There is truly no such thing as a monolithic Islam.

*America and Political Islam* is rich in data, references, and policy recommendations and should be of great interest to

the serious student of Islam, regional politics, and American interests in the Middle East. The book is divided into ten chapters, including an outline that links culture and history to the making of U.S. foreign policy in the region; an effort to highlight the ongoing tension between American confrontationalists and accommodationists toward Islam; an analysis of American images and perceptions that mistakenly connect terrorism with spiritual and political Islam; a discussion of the evolution of U.S. policies toward the Muslim world from the perspective of three American presidents and of the rationale behind those policies; and an analysis of the Clinton administration's efforts to play down the clash of civilization between Islam and the West. The rest of the chapters focus on U.S. response to Islamic resurgence and activism in Iran, Algeria, Egypt, and Turkey. In the conclusion, there is a serious attempt to recapture many of the policy recommendations strewn throughout the book on how to improve current U.S. policy toward the Muslim world.

I have two concerns with this book, one procedural and the other substantive. On the procedural level, although the chapters are well organized, they often lack a conceptual framework that ties them together in some systematic fashion. They come across as a mixture of academic research and investigative journalism. There is also an inherent problem with sourcing, which at times conveys a superficial treatment of the subject. For example, a sizeable portion of the information is attributed to interviews with unidentified individuals. In particular, a recurring interview is attributed to an unnamed U.S. Department of State official and is taken at face value as authoritatively credible, factual, and sound. Such sourcing could be potentially misleading and unconvincing. Likewise, the chapter on Iran is dependent narrowly on selected works of former policymakers, or current journalists. In short, some sections may lack rigorous analytical treatment and the originality of thought found elsewhere in the book.

On a substantive level, Gerges does not always capture the increasing complexity of making U.S. foreign policy in today's regional and global milieus: Foreign and domestic issues are becoming increasingly intertwined in the United States. U.S. foreign policy on any issue is part and parcel of a complex web of personal and institutional interactions that involve daily interagency debates and turf battles, aggressive congressional involvement in foreign policy decisions, an ambitious National Security Council staff, and a more assertive Department of Defense stimulated by the outbreak of multiple national, ethnic, and religious conflicts that require American military intervention. The Department of State is being preempted by these newcomers and by global and regional events, such that its traditional conflict resolution and preventive diplomacy often are forced to take a back seat to greater assertiveness and leadership by other agencies. For these reasons, American policy toward Islam will continue to muddle through rather than be expressed in a bold, explicit manner, as the author advocates.

Despite these concerns, *America and Political Islam* is a breath of fresh air that forces serious scholars and policymakers to rethink their positions on a very important topic with far-reaching political implications.

**Exploring European Social Policy.** By Robert R. Geyer. Cambridge, UK: Polity Press, 2000. 272p. \$50.00 cloth, \$14.99 paper.

William Walters, *Carleton University*

Social scientific interest in "social Europe" pales in comparison with the attention that has been directed toward the

economic and political dimensions of the European Union (EU). This is perhaps hardly surprising; for much of its relatively short history, the system that is today the EU has been almost exclusively economic in its focus. Only since the 1980s has the project of European integration acquired a significant social dimension. Given this imbalance, Robert Geyer provides a welcome and timely addition to the literature.

The major strength of this book resides in the significant empirical effort that underpins it. Early chapters deal with the historical emergence of a social policy for the European communities. The rest of the book "explores"—the author's favored term for his approach—this policy on an area by area basis, covering such topics as employment, gender, regional, and antiracism policy. Those new to the subject will be indebted to Geyer for a very accessible, systematic, and generally comprehensive overview. European social policy presents a bewildering array of social action programs, social protocols, social chapters, social charters, social dialogue, employment pacts, and so on. Geyer does an excellent job of untangling these and placing them in a coherent historical narrative. (Because the book is an introductory text, however, it would be helpful to have a glossary of terms.)

A major weakness of the book stems from the author's reluctance to engage with theoretical debates about social policy. Instead, Geyer presents his task as one of "mapping" or "exploring" EU social policy. His ambition is modest: "to provide academics and policy practitioners with an accessible foundation which they [can] use to explore their own particular questions" (p. 207). There is an attempt to relate what has become the standard classification of theories of European integration to the social field, but it is not very illuminating. Geyer also promises but fails to deliver a method informed by historical institutionalism (p. 6). Consequently, the book lacks a sound foundation to account for the developments it describes. Too often, the success or failure of a particular policy initiative is framed in terms of propositions about the general climate of the "integration" project at that time, as though integration were a singular process.

In no way does the ambition of exploring rather than theorizing about EU social policy invalidate this project. It does create problems, however, such as how one should define the scope and content of social policy. Geyer's solution is to accept the EU's "practical definition" (p. 5). Hence his empirical focus is basically the activities of the European Commission's directorate-general for employment, industrial relations, and social affairs. Yet, the social is not something fixed but an historically and politically structured field. Today, European welfare is heavily influenced through such mechanisms as the social regulation of consumer affairs and environmental issues, as much as through the more traditional social policy instruments (e.g., see Giandomenico Majone, "The European Community between Social Policy and Social Regulation," *Journal of Common Market Studies* 31 [June 1993]: 153–70). To ignore something like consumer policy, as Geyer does, is to miss something important about how the social field is being reconfigured and the new ways in which individuals and groups become active as consumer-subjects in their own government.

The book also would have benefited from some consideration of the relationship between the European social field and national policy dynamics. Geyer tempts fate somewhat by claiming to have "reviewed all major English language texts on EU social policy" (p. 7). This perhaps demonstrates the dangers of confining research too narrowly to a particular subfield. Geyer seems to have overlooked one of the foremost economic historians and assured voices on the subject

of European integration, Alan Milward. To grasp more fully the relative neglect of a social policy for most of the EU's history, one surely must consider Milward's *The European Rescue of the Nation-State* (1992). If European social policy has lagged behind developments in economic policy, it is perhaps in large part because the Common Market helped provide national economies with a framework for growth and prosperity. In so doing, it made postwar national social policies all the more financially viable and politically entrenched. In this way, perhaps the relative success of the Common Market has contributed to the failure—at least in its early years—of European social policy.

One final point: It would be more accurate, if slightly less elegant, to title the book “Exploring European Union Social Policy.” Its concerns are almost exclusively the social policy programs and institutions of the EU. It is quite important that one not conflate Europe with the European Union. If the present EU has come to speak in the name of Europe, this should not be taken for granted or naturalized by using the terms interchangeably. Rather, it is a phenomenon that needs to be interrogated critically. The EU may be the most successful and hegemonic construction of Europe in recent times, but it is not the only one. Alongside the exploration of EU social policy that Geyer proposes—or any other policy field for that matter—we need to explore the other ways that international social policy has been attempted (e.g., see Carl Strikwerda, “Reinterpreting the History of European Integration: Business, Labor, and Social Citizenship in Twentieth-Century Europe,” in Jytte Klausen and Louise Tilly, eds, *European Integration in Social and Historical Perspective*, 1997). This might often be a tale of failure, but it nevertheless should expand our capacity to think about different possibilities for governing in the future.

**The Challenge of Global Capitalism: The World Economy in the 21st Century.** By Robert Gilpin, with the assistance of Jean Millis Gilpin. Princeton, NJ: Princeton University Press, 2000. 373p. \$29.95.

Sylvia Ostry, *University of Toronto*

The word *globalization* first appeared in the second half of the 1980s and now has become the most ubiquitous in the language of international relations. It has spawned a new vocabulary: globaloney (Why all the hype when the global economy was more integrated in the age of Queen Victoria?): globophobia (the new, mainly mistaken, backlash); globberatti (the members of the international nongovernmental organizations [INGOs] who travel around the world from conference to conference, except when they are on the Internet mobilizing for the next conference), and so on. For Robert Gilpin, among the world's most eminent scholars of international relations, globalization is insightfully defined as the deepening and widening integration of the world economy by trade, financial flows, investment, and technology. This, he notes, is the “second great age of capitalism.” The basic theme of the book is whether, like the first great age, it will end with a bang (or even a whimper) or survive and thrive.

Gilpin provides a clearly written and comprehensive tour d'horizon of the drivers of integration—trade, finance, transnational corporations—and highlights for each domain the major policy issues. He spells out the main features of the growing backlash and is most acute in his critique of the simplistic views that globalization is either nirvana or Armageddon. But the most important thesis of the book—and its most important contribution to the discipline of international

political economy—is that the future of globalization ultimately rests on political foundations, not on the merits or demerits of the now dominant neoliberal paradigm. Like Schumpeter (and unlike Marx), Gilpin stresses that policy is the product of politics, not economics; unlike the international regime theorists who extend to the political domain the basic optimizing assumptions of economic theory, he stresses that the real world of political economy is too messy and prone to accident and error to support this reductionist model of policymaking.

At the core of the political economy of postwar global capitalism, Gilpin argues, was the Cold War. It is certainly true that the Cold War was vital to the launch of the Marshall Plan and the construction of the architecture of international economic cooperation. Churchill termed the Marshall Plan “the most unsordid act in history.” That it was. But it was also, in Gilpin's classic realist approach, very much a response to fear of Stalin's communist missionary zeal. And there is little doubt that it was an immensely successful diplomatic maneuver to outwit the Soviet Union and score a triumphant preemptive move, the first salvo of the Cold War. It was also important that Stalin refused to participate and that both the Marshall Plan and the creation of the Bretton Woods institutions and the GATT were strongly supported by American business and labor, not for fear of communism, but because American industry was far more efficient than any potential competitors in Europe (let alone postwar Japan). Furthermore, although the Cold War constrained the spillover from “low” to “high” policy (e.g., from trade to security issues), the first major crack in the postwar architecture—the end of the Bretton Woods system of exchange rates—was catalyzed by the Nixon administration in the early 1970s, when the Cold War was still alive and well. Also, the shift to a multitrack trade policy and the rise of system friction with respect to Japan are related more to the increasingly complex evolution of the American trade policy agenda than to the threat of the evil empire.

As is clear from Gilpin's account, however, by far the most important effect of the end of the Cold War in the United States has been the decline in congressional deference to the executive branch in international economic policy. Gilpin cites Clinton's failure to secure fast track in 1997 and the struggle to win approval of IMF funding in 1998 as examples. Again, is it the end of the Cold War or the absence of a coherent view of America's role in a global economy that accounts for the increased domestication of foreign policy? What role is played by the media and, more broadly, enhanced communication in the erosion of the “permissive consensus” of the earlier postwar decades, which provided far greater scope for policy action in areas that did not resonate directly with the broader public? Obviously, no precise answer is possible, but it would have been useful to explore some of these factors. The American genius has always been creative ad hocery, and in a sense the United States became an “accidental hegemon” after being forced to enter World War II by the Japanese bombing of Pearl Harbor.

The uncertainty about America's willingness or ability to play a leadership role in sustaining and reinforcing the political foundations of the global system lies at the heart of Gilpin's concern with the future of this second age of capitalism. The main threats to the system, he argues, arise from regionalism, financial instability, and increased protectionism.

In reviewing the ongoing debate among economists as to whether regional trade agreements (RTAs) are building blocks for or stumbling blocks to preserving and enhancing the rules-based multilateral system, Gilpin comes down

strongly—although for more than purely economic reasons—on the side of multilateralism. Many economists (myself included) would agree. But this analysis should have been broadened to include the proliferation of subregional agreements among southern countries (as tools to increase bargaining clout in the World Trade Organization). Indeed, the North-South divide, created in part by the Uruguay Round (and vividly manifested in Seattle by the walkout of virtually all non-OECD countries) is not mentioned. The threat may be a proliferation of RTAs without the United States, especially if the labor and environmental standards issues remain high priorities for the next administration. If a new round of negotiations cannot be launched within a reasonable time after the U.S. election, then the future of the WTO does not look promising.

For whatever set of reasons, we cannot dismiss the threat of fragmentation of the global system or the threat of another financial crisis—especially in light of the fact that the Clinton administration's ringing declaration to create a "new international financial architecture" has yielded little but modestly improved plumbing. As Gilpin points out, there is disagreement among economists and among governments about what should be done, and the United States is either unable or unwilling to lead the charge.

Gilpin wisely does not try to predict the outcome of these threats and challenges. He is certainly right in emphasizing that the political foundations of the global economy will determine its future and that the United States must play a leading role. But perhaps there are possibilities in other configurations of global leadership? Broader engagement was needed to launch the Uruguay Round. The creation of the WTO was due to a Canadian proposal latterly supported by the European Union to constrain U.S. aggressive unilateralism. The EU rescued the financial services negotiations after the outcome was rejected by the United States. These are only examples. It would worth exploring, especially by someone like Gilpin, whether a new pluralist system of global governance is possible. From the viewpoint of Realpolitik, perhaps the alternative would be bad enough for world leaders to give it a try.

**Seeking New World Vistas: The Militarization of Space.** By Roger Handberg. Westport, CT: Praeger, 2000. 304p. \$62.50.

Larry Martinez, *California State University, Long Beach*

Space is a seductive place, where technology-induced vistas often blur the policy vision of earthbound military planners, scientific explorers, or commercial entrepreneurs. This is the message that beams down from Roger Handberg's book on space militarization. He leads the reader through the twists and turns of technology, law, and policy, through the bureaucratic labyrinth of the U.S. military and space industrial complexes. In the end, one is faced with the same imponderables that confronted President Clinton in deciding whether to deploy the National Missile Defense (NMD) system. Like an astute player on fourth down, he punted that space football to his successor, and the Handberg volume gives you the Monday morning quarterback advantage.

Handberg provides a brief history of space policy, segmented by technology digressions that help capture the flavor of the current policy environment. These background factors heavily influence the bureaucratic politics dominating space militarization debates. Military space policy, on the macro level, involves a debate about the intrinsic contradiction in

attempting to mold dual-use (military-civilian) technologies to fit military objectives.

Perhaps the most pervasive example is the wildly successful global positioning satellite (GPS) system, which originated with experiments in the 1960s. The U.S. Department of Defense (DOD) deployed some of its 24-satellite flotilla in 1990–91 in time for combat testing during the Gulf War. Even only partially deployed, GPS made a direct technology hit during Desert Storm. But the ancillary damage to military exclusivity was widespread; anecdotal stories abound of parents rushing to the local electronics stores to buy off-the-shelf GPS receivers so their soldiering children could guide their humvees back to home base during blinding dust storms. The dual-use applicability—and profitability—of GPS has wrenched its control out of the hands of DOD designers and operators. In effect, due to its civilian and commercial appeal, GPS is becoming another Internet.

Just as the Internet represented a paradigm shift in networking architectures that continues to shape information-intensive societies and economies alike, Handberg raises the question of whether the outer space region performs an analogous task for global military configurations of power in general and for U.S. strategic doctrine in particular. The Internet obliterates the informational concept of distance, and Handberg wonders whether mastery of space will erase the concept of territory, enabling the U.S. military establishment to confront threats in a world that is "becoming a much more politically fragmented place with security threats becoming both more diffuse and very specific in certain regions" (p. 5). Space, in this regard, constitutes the ultimate high ground for the military establishment skillful enough to master it.

The basic seduction of space is the possibility of developing and using the perfect weapon, capable of "antiseptic" destruction of subnational or terrorist enemy threats without the messy political costs of noncombatant tragedies broadcast on CNN. Perhaps even more seductive is the prospect of constructing a global defensive shield against missile or aircraft attacks by rogue nations. Handberg astutely observes that the history of military space is littered with failed weapons, strategies, and paradigms. So what drives the current space military debate? He clearly locates decision making deep within the DOD corridors, as space architects battle it out with rogue commands that threaten to usurp long-range plans.

The latter chapters delve deeply into bureaucratic politics, detailing agency attempts to herd a workable ballistic missile defense proposal into their deployment corral. The problem is, very few are willing to bet the farm on an untestable and perhaps unreliable defensive technology. From the earliest Nike-Zeus systems through the Strategic Defense Initiative (SDI) to the current crop of NMD proposals, the technology of hitting a bullet with a bullet in the fog of war is a dicey undertaking at best, and a disaster even with a 90%+ success rate. Nevertheless, the dream persists that it can be done, which fuels the bureaucratic battles among space architects, space commands, and stove piped systems.

The Gulf War was the first "space war." Widespread integration of space-based systems greatly enhanced the ability of Desert Storm forces to carry out missions and achieve objectives at minimum cost of lives and material. Whatever the final assessment of the Patriot's effectiveness as an antimissile weapon, the Desert Storm experience compelled a fundamental rethinking of U.S. space strategy, which Handberg identifies as taking place on two levels. The first is the theory-rich development of military doctrine and policy, and the second entails the applications-oriented process of

deciding which missions are most appropriate for space-based systems.

According to Handberg, on the first level there are four chief mission components for space-based systems: space force support, space force enhancement, space force applications, and space control. These missions are juxtaposed against four doctrines of action on the second level: space sanctuary (preserving space for free overflight and surveillance), survivability (a force enhancement tool), control (actual U.S. control of the outer space region), and high ground (space as the dominant theater of military operations). Handberg locates current U.S. military space policy within the sanctuary and survivability schools of thought, corresponding to prevalent “political and military needs.” Whether the four missions move out to the control and high ground applications will depend largely on the technological and economic feasibility of future systems to make orbital access cheaper (control) and on the prevailing world security climate for the United States (high ground). Another and perhaps more crucial factor is whether these missions match programmatic and budgetary needs of the agencies that advocate them.

Handberg warns about the stealthiness of the space militarization issue and its proclivity to slip beneath the political radar screen. The debate is directed by bureaucratic infighters versed in the technolodge that quickly alienates the uninitiated. Although the book attempts to impart a detail-rich view of the space militarization policy process, what emerges is a comprehensive but somewhat bewildering view of a policy morass, which just might be an apt description of current realities. Handberg advocates an open public debate about what the “new world vistas” mean for the ability of the United States and other countries to confront the diffused yet specific threats in the coming decades with military space infrastructures. Whether the vista matches the vision is anyone’s guess.

**International Relations and the Challenge of Postmodernism: Defending the Discipline.** By D. S. L. Jarvis, Columbia: University of South Carolina Press, 2000. 288p. \$34.95.

Jeffrey W. Taliaferro, *Tufts University*

Over the past twenty years, the so-called third debate, or the constructivist turn in international relations theory, has elicited a great deal of attention. Various critical theories and epistemologies—sociological approaches, postmodernism, constructivism, neo-Marxism, feminist approaches, and cultural theories—seem to dominate the leading international relations journals. Postmodernism (also called critical theory), perhaps the most radical wave of the third debate, uses literary theory to challenge the notion of an “objective” reality in world politics, reject the notion of legitimate social science, and seek to overturn the so-called dominant discourses in the field in favor of a new politics that will give voice to previously marginalized groups.

D. S. L. Jarvis’s book is a wake-up call to international relations scholars who have become increasingly preoccupied with meta-theory and epistemology to the detriment of explaining “real world” phenomena, such as the causes of war and the conditions for peace. Jarvis offers a lucid and highly critical appraisal of the rise and fall of postmodernism in the study of world politics. For Richard K. Ashley and Robert Walker, the postmodernist challenge signals “a crisis of confidence, a loss of faith, a degeneration of reigning paradigms, an organic crisis in which, as Gramscians would say, ‘the old is dying and the new cannot yet be born’” (Richard

K. Ashley and R. B. J. Walker, “Speaking the Language of Exile: Dissident Thought in International Studies,” *International Studies Quarterly* 34 [September 1990]: 259–68). Jarvis persuasively argues that news of the discipline’s demise and the ascendancy of postmodernism are greatly exaggerated.

Jarvis begins by tracing the history of international relations as a field of inquiry distinct from philosophy, international law, and history. “Rather than strong foundations and the building of a robust stock of theoretical knowledge, international theory looks to be cracking at the edges, its foundations crumbling amid the onslaught of perspectivism and epistemological debate” (p. 43). He then traces the evolution of postmodern theories and their importation to international relations in the late 1970s and early 1980s. Next, through a close examination of the writings of Ashley and Christine Sylvester, Jarvis traces the effect of “subversive and deconstructive” postmodernism on the field. In doing so, he is unsparing in his criticism of the extreme relativism (and in many cases nihilism), excessive jargon, polemical argumentation, repudiation of basic social science canons, lack of empirical evidence, and tortured prose that have come to characterize the so-called postmodern school. Finally, he concludes with an appeal to the continued utility of positivist research programs.

It is important to note the scope and limitations of the analysis. Jarvis does not launch a broad assault on the rise of constructivism and critical theories in international relations in general. For example, he does not take issue with the so-called conventional constructivism of John Gerard Ruggie, Alexander Wendt, Ted Hopf, and David Dessler. These scholars argue that discursive practice can fundamentally change states’ foreign policies but seek a middle ground between the mainstream research traditions in international relations (realism, liberalism, and Marxism) and critical theory. This, however, is the portion of constructivism that has had the more lasting influence on the discipline. As Hopf notes, “to reach an intellectually satisfying point of closure, constructivism adopts positivist conventions” and, by doing so, can challenge realist, liberal, and Marxist theories of international politics (Ted Hopf, “The Promise of Constructivism in International Security,” *International Security* 23 [Spring 1998]: 171–200).

Jarvis focuses on what he terms “subversive-deconstructive postmodernism,” a body of scholarship that “displays a thematic concern with negation and resistance to modernist practices and discourses, primarily via a deconstructive-textual analysis of logocentric practices, modernist knowledge systems and language” (p. 66). Drawing upon the philosophy of Friedrich Nietzsche, Michel Foucault, and Jacques Derrida, as well as the deconstructive literary theory of Frederic Jameson, postmodernists repudiate and seek to undermine the entire intellectual tradition of the Enlightenment. Instead, all knowledge is located in the fact of textual analysis and situated in the subjectivity of each individual.

Jarvis presents a detailed critique of Ashley, who in the 1980s both brought constructivist accounts of the state, political power, and the practice of realpolitik into international relations and raised questions about construction of knowledge, meaning, and truth. “Never before have international theorists been so assaulted by excursions into meta-theory, especially when the depth of this excursion questions not only the ontological but also the epistemological and ‘axiological foundations of their scientific endeavors’” (p. 90). Jarvis divides Ashley’s work into two parts. In the “heroic” phase (the late 1970s to early 1980s), Ashley sought to highlight many of the epistemological and ontological premises upon which neorealist theory rests. In his seminal

essay, "The Poverty of Neorealism," Ashley criticized the theory for its undeveloped treatment of the state and showed how Hans Morgenthau's classical realism and Kenneth Waltz's neorealist balance-of-power theory rested upon normative assumptions (Richard K. Ashley, "The Poverty of Neorealism," *International Organization* 38 [Spring 1984]: 225–86). Jarvis notes that this stage of Ashley's research had a profound and positive effect on both the emergence of constructivism and the subsequent refinement of realism.

Jarvis is sharply critical of the second, or poststructuralist, phase of Ashley's work, which draws upon postmodernism to overthrow the dominant epistemology in international relations. By doing so, Ashley eliminates the "real world" problems of war, violence, poverty, and bigotry. Jarvis rightly asks: "Is Ashley really suggesting that some of the greatest threats facing humankind or some of the greatest moments of history rest on such innocuous and largely unknown nonrealities like positivism and realism" (p. 128)?

The main strength of this book lies in the author's efforts to assess the effect of postmodernism on the study of world politics against the standards set by Ashley, its foremost proponent. Paradoxically, this also is the book's main weakness. Postmodernism has not transformed the study of international relations, let alone the practice of statecraft, for two simple reasons. First, most international relations scholars are unwilling to wade through the postmodernists' abstruse prose to uncover the substance of their arguments. Instead of engaging Ashley, Walker, and others, most scholars (even their fellow constructivists) are quite content to ignore them. Second, by waging war on positivism, rationality, and realism, postmodernist scholars have marginalized themselves. As Jarvis himself acknowledges, "these are imagined and fictitious enemies, theoretical fabrications that represent arcane self-serving debates superfluous to the lives of most people and, arguably, to most issues of importance in international relations" (p. 128).

**Immigration and the Nation-State: The United States, Germany, and Great Britain.** By Christian Joppke. Oxford: Oxford University Press, 1999. 356p. \$72.00 cloth, \$22.95 paper.

Gary P. Freeman, *University of Texas at Austin*

This book will enhance Christian Joppke's growing reputation as one of the most thoughtful commentators on the politics of international migration and citizenship. *Immigration and the Nation-State* is an impressive cross-national comparison that builds on elite interviews and reanalysis of primary materials, but its chief value is in its bold synthesis and critique of a rapidly growing and highly disjointed secondary literature. Although it assesses a variety of theoretical concepts, the book is primarily a historically rooted, richly empirical work of analysis and interpretation. Joppke deals expertly with three liberal states with different nationhood traditions and immigration histories. The United Kingdom is distinctive in that it was at once a nation-state and an empire. The United States is the only case of the three in which governments deliberately sought to foster immigration for settlement. Germany was a divided nation whose commitment to reunification, embedded in the Basic Law, posed particularly troublesome issues for immigration and citizenship policy.

The key organizing concepts of the book are sovereignty and citizenship. The first argument is that liberal states still exhibit sovereign power over immigration policy, contrary to proponents of the globalization thesis (e.g., Saskia Sassen,

*Losing Control? Sovereignty in an Age of Globalization*, 1996), but they are self-limited by interest-group pluralism, autonomous legal systems, and elite moral obligations toward particular immigrant groups (p. vii). "The major dilemma of immigration control is to reconcile the popular constraints of states with their parallel, inherently unpopular, mandate to protect the human rights of migrants" (p. 5). Joppke's second principal argument is that national citizenship is critical for the incorporation of migrants (contrary to the postnational membership model advanced most prominently by Yasemin Soysal in *The Limits to Citizenship*, 1994), but nationally distinct citizenship regimes result in diverse multicultural arrangements. Citizenship practice in the three states is inclusive internally but exclusive externally. "Immigration has opened up a post-Marshallian view of citizenship, which stresses its externally exclusive dimension" (p. 6).

Joppke is unusual among immigration scholars because he does not accept a number of commonly held views and often tilts against the conventional wisdom. He disagrees, for example, that an overweening pattern of immigration restrictionism is sweeping across "Fortress Europe," even less across the United States. He discerns instead a marked trend toward inclusive and expansive policies in two of his cases. Only Britain fails to conform to this pattern. This does not mean, however, that European democracies are destined to open their doors to immigration in the manner of the United States. He disagrees with those who claim that the distinction between endogenous nations and nations of immigrants is moot (p. 9). European states, he observes, are not and have not been nations of immigrants. Among liberal democracies it is the settler societies—the United States, Australia, and New Zealand—whose immigration experience stands out as unusual.

Joppke writes especially well about Germany, whose immigration and citizenship policies are widely misreported by the press and scholars alike. He notes, for example, what is obvious but rarely appreciated: The *ius sanguinis* citizenship tradition is not a bizarre German idiosyncrasy but is, in fact, standard practice in continental Europe. The puzzle is not why Germany persists in claiming that it is not an immigration country, but why it should think of itself in that way (p. 62). He points out that the "as of right naturalization" procedures adopted in the early 1990s make German law more liberal in that respect than the American because there is no assimilation test for citizenship. On the enormously controversial issue of reforming the Basic Law's asylum guarantee, Joppke argues that the Federal Republic's grant of rights of appeal to the whole world was nonviable and out of step with all the other liberal democracies: "In the end, Germany has only adjusted its asylum law to the international standard. If this adjustment has appeared drastic and deviated from its usually incremental policy style, it is because an essential function of sovereignty, control of territorial access, has had to be recovered from a unique impairment" (p. 94). In the context of division, the "no immigration country" mantra seemed necessary. Germany was the homeland for all Germans. Reunification and the return of the *aussiedler* with the end of the Cold War permit a relaxation of the maxim and the normalization of immigration policy. The author shows that the "no immigration country" stance was in any case balanced by the provision of extensive rights for foreigners in the legal system. There was no real effort after 1974 to send the guest workers home, and they enjoyed access to the benefits of the welfare state equal to that of citizens.

No non-American has written with as much confidence and insight as Joppke brings to his discussion of the U.S. case. His tone is at times sardonic, and from his European standpoint

he obviously finds the chaotic American system entertaining. His review of race policies and affirmative action as well as the unseemly competition among immigrant groups to get in on the spoils provided by the American liberal conscience is especially telling.

Whereas Joppke sees aspects of immigration politics in America as faintly ridiculous and the German case as less appalling than commonly held, he expresses more or less unreserved scorn for British policy. He admits that Britain had a problem at least as serious as that created by Germany's imperative to be a homeland for the Germans. If the German problem was political boundaries too narrow to encompass the nation, the British problem was political boundaries that were wider than the nation (p. 100). The British Nationality Act of 1948 created no British citizenship but granted rights of entry and residence to hundreds of millions of British subjects around the globe, rights that no British official expected or wished to see exercised in large numbers. "The logic of British immigration policy is thus determined by the devolution of empire . . . [it] was to carve out the historical homeland nation from the vast empire, and to subject the rest to immigration control" (pp. 100–1).

British attempts to keep foreigners out have been more determined and successful than those of its democratic neighbors. According to Joppke, one reason British policy is so tough and mean-spirited is that it had never been deliberate government policy to encourage immigration and settlement from the empire and Commonwealth. Consequently, British elites felt few of the moral obligations expressed by their German counterparts toward the guest workers they had recruited. A second factor is the largely docile judicial system in Britain, which deprives migrants of constitutional protections from either administrative or legislative abuse. Due to the weaker legal and moral constraints, British policy moved in the opposite direction to that of Germany, "embracing rather than rejecting drastic solutions" (p. 114).

Joppke usefully criticizes and amends my argument that client politics is the typical form of immigration policymaking in liberal democracies (Gary P. Freeman, "Modes of Immigration Politics in Liberal Democratic States," *International Migration Review* 29 [Winter 1995]: 881–902). He agrees that client politics largely explains the development of U.S. policy throughout the postwar period and that of Germany until the recruitment stop of 1974, but he finds the model misleading for Germany thereafter and inappropriate altogether for Britain. Activist courts account for some aspects of U.S. expansionism, a factor that fails to fit into the client framework. In post-1974 Germany, the courts, welfare state rules, and the moral commitments of elites produced inclusive policies toward resident foreigners, while domestic security concerns motivated the government's resistance to new rounds of labor recruitment or the admission of large numbers of asylum seekers (p. 79). Because no important groups favored mass immigration to Britain, there was, by definition, no client politics.

Joppke's excursion into the politics of multicultural integration is highly illuminating. Readers may find his treatment of Germany most interesting if only because that case is so commonly misconstrued. Joppke concludes that the ethnonational citizenship model delineated by Rogers Brubaker (*Citizenship and Nationhood in France and Germany*, 1992) and the postnational model espoused by Soysal coexisted in Germany, the latter making the former possible. The ethnocultural mandate to reunify the German people was flexible enough to justify denying immigrants citizenship but at the same time embrace them as equal members of a postnational community. In neither case were immigrants expected

to assimilate; if carried far enough, assimilation would "destroy the ethnocultural texture of the nation" (p. 189).

*Immigration and the Nation-State* is essential reading for scholars in the field, and its clear prose and compelling storyline make it suitable as well for advanced undergraduates. The book perhaps concentrates too single-mindedly on domestic politics, giving only a passing nod to the European Union and presenting no data, as opposed to logic, in arguing against the influence of international courts and regimes on national policies. But Joppke presents a powerful argument for the predominance of national politics that others must confront in exploring the extranational dynamics of immigration policymaking.

**Sovereignty: Organized Hypocrisy.** By Stephen Krasner. Princeton, NJ: Princeton University Press, 1999. 207p. \$69.95 cloth, \$24.95 paper.

Bruce Cronin, *University of Wisconsin-Madison*

Few concepts in international relations have aroused as much debate and emotion among political leaders, activists, and scholars as that of sovereignty. Diplomats continually invoke it, transnational organizations attempt to circumvent it, and scholars debate its meaning and wonder whether globalization is making it obsolete. Yet, most accept the premise that sovereignty is not only the foundation of our international system but also one of the few consequential institutions we have in world politics.

*Sovereignty: Organized Hypocrisy* challenges both assumptions by offering a coherent and powerful analysis of how the theory and practice of sovereignty often diverge. The central thesis is that the characteristics usually associated with sovereignty (territory, autonomy, and recognition) do not provide an accurate description of the actual practices of the units within the international system. Specifically, Krasner argues that since its inception, sovereignty's primary attribute—domestic autonomy—is routinely compromised whenever political leaders believe it to be in their interest to do so.

Although this may sound like a classic neorealist argument, Krasner is not a typical realist, and his book moves well beyond a static structuralist account of state behavior. Although he uses a power-and-interest approach to examine how sovereignty is practiced in international relations, he takes the logic of this analysis farther than most realists have been willing to go. As do most realists, Krasner argues that it is impossible for any institutional arrangement at the international level to become deeply embedded—even one as fundamental as sovereignty.

Yet, whereas a realist may find strong systemic pressures that help maintain domestic autonomy for most states (e.g., the balance of power), Krasner makes no such assumptions. This is because his starting point is not "states" but the political elites who rule in the name of the state. Such individuals are primarily motivated by a desire to maintain their authority within the domestic sphere, so they can adopt a "logic of consequences" that is not consistent with either the norms or structural dynamics of the international system. In this sense Krasner's world is not a Hobbesian state of nature in which states act according to the necessities produced by international anarchy but, rather, a Machiavellian one, in which leaders act to maintain and expand their own power.

Krasner also departs from the assumption shared by realists and nonrealists that the core attributes of sovereignty—international recognition (legal sovereignty), indepen-

dence from outside actors (Westphalian sovereignty), and state supremacy (domestic sovereignty)—are necessarily connected. He argues that a state can have one of these attributes (e.g., international recognition) without enjoying others (e.g., state supremacy). Having made this point, he focuses primarily on Westphalian sovereignty and how leaders compromise this aspect through intervention and invitation.

Krasner argues that the basic principle of Westphalian sovereignty—domestic autonomy—is frequently compromised through coercive intervention by stronger states and by conventions and contracts that domestic political elites voluntarily sign. In each case, external actors influence domestic authority structures, thereby violating the fundamental norms of independence and autonomy. Thus, not only do powerful states routinely compromise the sovereignty of other states, but also domestic elites sometimes compromise their own sovereignty when it suits their interests.

The author supports this thesis through several case studies that cut across time and geographic region. Specifically, he examines the ways states' domestic autonomy is compromised through the conclusion of human rights conventions, minority rights treaties, international financial lending agreements, and conditions placed on the constitution of new states. In some cases states are forced to accept such intervention as the price of international recognition; in other cases they voluntarily agree to accept constraints on their domestic autonomy when such actions strengthen the ruling elite. Taken as a whole, the cases suggest that violations of sovereignty norms have been an enduring practice since the evolution of the nation-state system.

This book is an important addition to the literatures on sovereignty and international organization. Most studies present the institution of sovereignty as either empowering or constraining, and some examine how its meaning is constructed over time. Few are bold enough to suggest that it is irrelevant. Krasner also throws a curveball into the debate between institutionalists and structuralists. Although the two schools differ on the degree to which norms can become institutionalized in international affairs, both assume that, when and if they do, they can have a strong influence on behavior. Krasner offers the provocative argument that some norms do indeed become widely accepted and remain persistent over time, but even when they become institutionalized they are often ignored.

Krasner's study also contributes to the ongoing debate over whether sovereignty is changing or even becoming obsolete in the face of rapid globalization and the spread of transnational issues, such as human rights. Rather than address the influence of these factors on the future of sovereignty (as most studies have done), the author shows that states have never been "sovereign" in the way that many assume. Thus, current trends do not constitute a radical departure from practices that have occurred over the past few hundred years.

The book is powerfully argued and tightly organized, but there are several methodological and conceptual problems. First, the author presents his evidence primarily by chronicling clusters of cases in which international institutions or collectivities of states exercise influence on the domestic politics of other states. Yet, this leads one to ask how these examples compare to the large number of cases in which domestic autonomy is respected. How many violations does it take for one to conclude that they are routine and frequent? On a daily basis, diplomats and political leaders tend to respect one another's sovereignty most of the time. In fact, it could be argued that, given the high level of daily interaction among states, violations as the author describes them are

relatively rare. For example, Krasner discusses how international institutions imposed comprehensive minority rights requirements on new states in the periods around 1878, 1919, and 1992, in each case after a major war or systemic shock. These requirements constituted a significant intervention, but they involved relatively few states under extraordinary circumstances and only occurred three times in the past century.

Second, some scholars may challenge Krasner's interpretation of what constitutes a "violation" of sovereignty norms. The author defines Westphalian sovereignty in terms of domestic autonomy, but few would argue that this means states are unencumbered and free from all external constraints. If sovereignty is understood as a social construct, then the rights and duties contained within the institution are not fixed; rather, they are subject to interpretation and change by the collectivity of states. In fact, many argue that a state's sovereignty is derived from its participation in the international system, and therefore the members of the international community determine what counts as an international as opposed to a domestic issue.

This problem of defining violations is illustrated by the author's inclusion of contracts and conventions among the practices that compromise domestic autonomy. Any form of cooperation or collaboration reduces one's range of options in making policy. Political leaders voluntarily sign agreements in order to attain benefits they otherwise would have to forgo. It is difficult to understand how this violates their sovereignty. One can argue that the ability to enter into international treaties or contracts is actually an expression of sovereignty.

Finally, some may find the argument that sovereignty is not absolute and is often circumscribed by political expediency and the exercise of power is not particularly enlightening. There is virtually no political principle or concept that is either unambiguous or consistently applied in all contexts and under all circumstances at either the international or domestic level. The concepts of "freedom" and "democracy" are examples. Even in the best of circumstances, the application of these principles often has to be harmonized with other (often conflicting) principles. If this constitutes "organized hypocrisy," then all social interaction contains a degree of incongruity.

**Gestures of Conciliation: Factors Contributing to Successful Olive Branches.** By Christopher Mitchell. New York: St. Martin's, 2000. 333p. \$65.00.

Paul D. Senese, *SUNY, Buffalo*

The end game of peace negotiations between international actors has received a great deal of scholarly attention over the years. Much of this effort focuses on the processes by which states, leaders, or diplomats achieve durable solutions to differences previously difficult to resolve. Christopher Mitchell offers a significant contribution to this literature by centering his attention on the observable starting points of peace, those with the potential to trigger a sequence of interactions that eventually produce an important agreement between previously conflicting sides.

Mitchell focuses his analytical energy not on general conflict resolution but on resolution of the most protracted and entrenched conflicts. The most significant contribution of his approach lies in its ability to advance our understanding not only of peace processes but also of peacemakers' adeptness in starting productive interactions between adversaries. Although he is interested in the full process of interactions



associated with peaceful international solutions, Mitchell singles out the importance of initial conciliatory gestures (or olive branches). This core concern allows him to differentiate, theoretically and empirically, between initial gestures that are quickly rejected and those that lead to a series of cooperative actions and reactions between foes. By closely assessing the key factors associated with both successful and unsuccessful conciliatory gestures, Mitchell draws conclusions that can guide academics, consultants, and diplomats in their selection of first-stage peace tactics.

The major focus is an assessment of what factors make it more likely that conciliatory initiatives will achieve any measure of success in bringing staunch international antagonists significantly closer to peace. Exactly what is meant by success? Mitchell, as he does with most aspects of beginning peace processes, devotes significant attention to answering this question (chap. 3). In the end, he suggests the possible utility of an ordinal scale of short- to medium-term success for conciliatory gestures (p. 57). In the abstract, this scale is fine, but operationalizing it would be (as Mitchell does point out) a difficult chore. This points to a general shortcoming evident throughout the book.

Although numerous hypotheses are presented (p. 124 and chap. 14), the book does not go nearly far enough in enabling them to be tested rigorously. Mitchell mentions this a number of times, but it still poses problems. For instance, getting two analysts (from the same or opposite sides of a dispute) to agree on the exact meaning of certain concepts would be almost impossible. Mitchell spends a great deal of time discussing the meaning of his concepts, but he does not offer operational definitions that would eliminate fuzzy interpretations of how the empirical world might support or falsify his hypotheses. Such operational measures are not always easy to provide, but their absence is notable. This is too bad, as this volume's painstaking attention to concept and nuance begs for testable empirical hypotheses.

In the final chapter, Mitchell offers thirty-nine hypotheses. He conceptualizes these as a list of factors that will have a positive effect on the success of conciliatory gestures. There is no mention, however, of their relative importance. Surely, the author must have good reason to believe that some are more integral to success than others. Mitchell is aware of this but suggests (p. 290) that sorting out relative importance is a matter for empirical investigation and testing. Unfortunately, the difficulty of matching operational indicators to his concepts limits our ability to do this. The influence of his book could have been even greater if he had developed a weighted list of these factors or, perhaps, a process model detailing the contingent or necessary status of some factors compared to others.

Much of the theorizing is connected to Mitchell's key empirical launching pad—the relations between Egypt and Israel from 1971 to 1979. The book offers a splendid account of the key interactions between these two states over the period, but much of the grist for Mitchell's hypothesis mill comes from his close consideration of this single case. There is attention to theory in the area, and occasional mention is made of other peace processes (e.g., the United States and North Vietnam, Argentina and Britain), but lessons learned from Israeli-Egyptian interactions (most notably those surrounding Sadat's offer to visit Israel in 1977) strongly influence the development of theoretical expectations. What if Mitchell had focused almost exclusively on another case? Would the hypotheses have been significantly different? This is, of course, the risk one takes basing inductive theory on a small number of cases.

In discussing structural changes that may be conducive to

conciliation (pp. 70–81), Mitchell gives a nod in the direction of quantitative social science by citing a few articles that deal with the “level” of conflict. These works are still worthwhile but are dated and have been surpassed to some degree by more recent efforts. For instance, the link between vital issues (including territorial integrity) and conflict escalation has been examined by such authors as Paul F. Diehl (ed., *A Road Map to War: Territorial Dimensions of International Conflict*, 1999), Paul K. Huth (*Standing Your Ground: Territorial Disputes and International Conflict*, 1996), and John A. Vasquez (*The War Puzzle*, 1993). Furthermore, chapter 10 contains an interesting discussion of signaling and its importance without mentioning prominent contributions by Bruce Bueno de Mesquita and David Lalman (*War and Reason: Domestic and International Imperatives* 1992), James D. Fearon (“Signaling versus the Balance of Power and Interests,” *Journal of Conflict Resolution* 38 [June 1994]: 236–69) and “Domestic Political Audiences and the Escalation of International Disputes,” *American Political Science Review* 88 [September 1994]: 577–92), and Kenneth A. Schultz (“Domestic Opposition and Signaling in International Crises,” *American Political Science Review* 92 [December 1998]: 829–44). Nevertheless, Mitchell should be commended for reaching outside his own methodological orientation in referencing at least a small number of formal and quantitative international relations pieces.

Although *Gestures of Conciliation* has a number of shortcomings, on balance its contribution to the literature is overwhelmingly positive. Mitchell's attention to the importance of factors surrounding initial conciliatory moves in protracted conflicts is very worthwhile. His rich examination of a wide range of variables involved in the contemplation and implementation of such moves deserves to be read by practitioners and students of conflict resolution alike. This book should become a required text in graduate courses on the techniques of successful conflict resolution. Furthermore, its richness of ideas but lack of empirical testing provide fertile ground for future work, including many doctoral theses.

**Taiwan's Informal Diplomacy and Propaganda.** By Gary D. Rawnsley. New York: St. Martin's, 2000. 182p. \$65.00.

John F. Copper, *Rhodes College*

This book assesses how Taiwan, the nation officially known as the Republic of China but called “Taiwan Province” in the People's Republic of China, uses propaganda to pursue foreign policy objectives, attain its national interests, and, even more than that, survive.

Taiwan is a unique case in that its diplomacy is hindered by the fact that Taipei does not have official relations with very many countries (around thirty in recent years), and Beijing pressures the global community to bar Taiwan from international organizations (especially those of a political nature). China wants to isolate Taiwan in order to force its leaders into talks that will lead to its absorption or reunification. All of this means that Taiwan must use propaganda more and diplomacy less.

Rawnsley begins by defining propaganda in a “softer” way than most other scholars: Communications that are intended for all to hear, or essentially publicity, in contrast to diplomatic communications, which may be private or public and are more formal. He maintains that all countries use propaganda, and these publicity efforts should not be seen so much as schemes to control the hearts and minds of people in other nations by using lies and misinformation; rather, this open

information dissemination works together with diplomacy in the normal and legitimate conduct of foreign policy.

Rawnsley begins the Taiwan case with a brief history of diplomacy there and the past use of propaganda. Although only a terse summary, it demonstrates that Taiwan has long been in the propaganda business, extensively so, and for good reason. After the Nationalists fled the mainland in 1949, Taipei had to struggle to win support from the United States. Congress and the American people were critical to this effort. The Cold War advantaged Taiwan in subsequent years, but this was never an easy task. When Beijing won a seat in the United Nations in 1971 and, in 1979, established diplomatic relations with the United States, Taiwan's only important ally, propaganda became an even more important part of its diplomatic campaign. In fact, says Rawnsley, Taiwan's efforts to defend its sovereignty depended upon its ability to convince others, especially Americans, it deserved support.

In chapter 2, the author examines in particular Taiwan's loss of diplomatic relations and notes how propaganda filled a void and became more a functional part of its foreign relations as a result. The terms flexible diplomacy and cultural diplomacy coined at this time reflected the new even more important role of information, Rawnsley argues.

Rawnsley proceeds to assess Taiwan's foreign assistance, its "tailored" use of information, and how propaganda efforts have been changed or updated to fit Taipei's altered view of the world. He also delineates the role of presenting knowledge about Taiwan and Taipei's work to build positive perceptions in other countries. Rawnsley suggests that in a number of cases these efforts had feedback and influenced domestic policies in Taiwan. Democratization was the most important of these, being seen in Taiwan as making the country more attractive abroad and thereby facilitating its diplomatic efforts. Democratization thus became a central theme, which helped Taipei counter Beijing's campaigns to delegitimize Taiwan. A specific example was Taiwan's decision to end martial law in 1987. Much was made of this because it would impress other countries, even though polls indicated most citizens did not object to it and did not want it rescinded. Rawnsley also notes that Taiwan took advantage of the improved global image that resulted from its democratization at a critical time, in 1989, when China's stock plummeted after the Tiananmen Massacre. Taiwan's "information people" later promoted the plenary elections in 1991 and 1992 and the watershed direct presidential election in 1996 (the first in 5,000 years of Chinese history).

The third chapter is devoted to an assessment of the government agencies responsible for making and disseminating information. Rawnsley focuses mainly on the role of the Government Information Office. He discusses at considerable length its links to and relationship with the foreign ministry and other organs of government. He also cites cases of success and failure, drawing on the experiences of foreign offices, especially those in Europe, South Africa, and New Zealand.

Chapter 4 analyzes Taiwan's use of news agencies, newspapers, and radio to present its case abroad. Television is not broadcast abroad very much, and the Internet has only recently become a means of delivering information (although Taiwan has been very quick and adept at using it). The author describes how Taiwan more than other nations emphasizes its relationship with the media. He also notes that Taiwan uses the media to communicate with China. There is, of course, a propaganda war going on between Taipei and Beijing; but clarifying policies and calling for negotiations also comprise efforts to initiate diplomatic talks, he says.

In the concluding chapter, Rawnsley reiterates his conten-

tion that propaganda and diplomacy are closely linked. Taiwan's extensive use of information, he asserts, is evidence for this. Rawnsley also delineates ties between information and business. Again, Taiwan is a good case. But he also notes that Taiwan has been blessed by having money to pay for information dissemination, which is costly. Thus, although Taiwan is a model for other nations to follow, many would have to spend considerable sums to do what Taiwan has accomplished. Finally, Rawnsley connects propaganda to lobbying efforts, especially in the United States.

Rawnsley uses the literature adroitly to prove his points, citing a broad range of works on both diplomacy and propaganda. He applies them well to Taiwan. He also looks broadly and carefully into how Taiwan's efforts have worked, but he might have made more of the successes in the United States. Taiwan is seen in Washington as having very good diplomats, and the reason for that is largely propaganda efforts (or lobbying) with Congress. Taiwan's pseudo-embassy in Washington is said to be one of the three most effective there (along with Japan's and Israel's) and has a reputation for excellence compared to China's embassy and staff.

*Taiwan's Informal Diplomacy and Propaganda* is unique in part because no other scholar has written a book-length study on this topic. But Rawnsley also does an expert job of assessing the bureaucratic system involved, the relationship of propaganda and foreign policymaking, links with the media, and results in the field. He has produced a work that is recommended to scholars interested in Taiwan's foreign relations and its use of information in pursuit of its national interests, as well as to those looking for a good case study of a government that ties together propaganda and diplomacy.

**The Moral Purpose of the State: Culture, Social Identity, and Institutional Rationality in International Relations.** By Christian Reus-Smit. Princeton, NJ: Princeton University Press, 1999. 199p. \$35.00.

Jennifer Sterling-Folker, *University of Connecticut*

The central puzzle motivating this book is why different systems of sovereign states develop different types of systemic institutions. Why did Greek city-states favor arbitration, whereas Italian city-states adopted what the author calls "oratorical diplomacy," the absolutist state preferred "old diplomacy" instead, and the modern nation-state relies on international law and multilateralism?

From the start it is clear that Reus-Smit is not interested in explaining just any interstate interaction but cooperative interaction specifically. He has clear sympathies with a neoliberal perspective on the subject (p. 11), although he is also quick to point out explanatory weaknesses. Reus-Smit makes a strong case that the motivating puzzle has not been answered in a satisfactory manner by any alternative theoretical perspective. Although neorealists can be faulted for failing to recognize that there is a puzzle, neoliberals can be criticized for their dependence on rational choice to address it. And although a constructivist himself, Reus-Smit is willing to take to task such constructivists as John Gerard Ruggie and Alexander Wendt for their exclusive reliance on the theoretically underdetermining attribute of sovereignty as a means to explain systemic institutional evolution.

Alternatively, Reus-Smit argues that such institutions can be traced to "the complex of values that define state identity" (p. 30). That is, their source lies in the meta-moral purposes that justify the existence of the state in any historical period, which states then transplant to the systemic realm as the

institutions that will guide interstate cooperation. “Informed by prevailing beliefs about the moral purpose of the state, the systemic norm of pure procedural justice shapes institutional choice, licensing some institutional solutions over others” (pp. 33–4). The result is different cooperative institutional arrangements under different sovereign state systems. Because the Greek city-states saw public discourse as the means to arrive at what was good and just, this was also the cognitive, institutional template for their dealings with one in the form of arbitration. Similarly, because “the metavalues that define legitimate statehood and rightful state action in modern international society are based on an individualist social ontology,” we find the development of systemic institutions such as international law and multilateralism.

There is much to like in Reus-Smit’s approach. The historical sweep of this study is both absorbing and amply demonstrates the tremendous scope and depth of his approach. The author breathes new life into the interpretation of myriad historical institutions, and along the way he grapples with a number of puzzles specific to particular state systems. There is also something intuitively correct in his argument that systemic institutions derive from the same meta-morals that give the state its purpose in any given period. Thus, it comes as no surprise that modern international law approximates American norms and institutional preferences so closely, although as Reus-Smit points out the foundation for these systemic institutions was laid by historical social practices begun in and shared by the nation-states of Europe as well.

As to whether Reus-Smit’s constructivist perspective manages to address his central puzzle in an entirely satisfactory manner, the answer is mixed. One problem is that it is not always clear what the logical connection is between the value complexes Reus-Smit asserts for the system of sovereign states and the cooperative institutions supposedly produced by them. The link between Italian internal patron-client relationships and the external “oratorical diplomacy” style seems logical enough, but not the link between Greek internal public discourse and external arbitration (are decisions by third-party judges really the same thing as public discourse about which all decide?). The asserted link between the absolutist state and “old diplomacy” is not just undertheorized but contradictory to the main thesis (the value complexes simply licensed war-waging and almost no cooperation). And the connection between the modern state’s meta-moral purposes and international law also has logical difficulties (if the nation-state’s purpose is to protect its own citizens, why would it want mutually binding interstate contracts?).

One reason some of these links may be problematic is because Reus-Smit’s constructivist alternative is only concerned with the production of cooperative institutions. As do most approaches to the phenomenon of interstate cooperation, Reus-Smit’s alternative separates the subject of cooperation from the subject of war as if they were on different causal tracks. His description of the modern period, for example, focuses on the treaties following major wars but makes only a passing reference to the wars themselves, as if they were mere aberrations in the on-going and apparently primary process of cooperative interstate institution-building. But treating the historical record in this way raises an obvious question. If the state’s internal meta-moral purposes produce cooperative institutions at the interstate level, then why does interstate war even occur?

Of course, it seems unfair to expect Reus-Smit to address such a question when his own interests lie with cooperative rather than violent interaction. But in order to illustrate his

argument, Reus-Smit must evoke an historical record that makes elements of his own approach, as well as the standard analytical separation of the two phenomena, increasingly questionable. Just how important can these values be to interstate interaction in general if the meta-moral purposes Reus-Smit has identified cannot also account for violent interaction and hence can only cover half (and sometimes the least interesting) of the interstate historical record?

There is also the possibility that an opportunity is missed. What Reus-Smit provides is the constructivist foundation for a holistic approach that, if meta-moral purposes are appropriately identified and carefully linked to resulting interstate social practices, could explain so much more than cooperation. The same puzzle that informs Reus-Smit’s study of cooperation occurs for war-making as well. Not only do different systems of sovereign states produce different cooperative institutions, but also they wage war differently. Is it not possible, then, that the moral underpinnings of the state could explain institutional variance in both interstate war-waging and cooperation? The chapter on the absolutist state provides ample supporting evidence for such a proposition, but because Reus-Smit is only looking for cooperative systemic institutions, he misses the chance (at least in this book) to account for war and peace under the same constructivist umbrella.

In order to develop such a holistic approach, however, the intimacy between internal moral authority and the power to disseminate it as a basis for interstate interaction would have to be examined more critically. Reus-Smit does not deny that there is a relationship between power and the moral underpinning of interstate cooperation (e.g., pp. 152–4), but he does consistently treat the two as if they were explanatory alternatives to one another. Yet, as many indigenous peoples and human rights activists could tell us, modern international laws (and the multilateral forums where they are propagated) are more often tools for status quo oppression by the powerful than vehicles for the universal protection of individual rights.

Thus, we find a correspondence between the interstate cooperative institutions established by the modern nation-state and the purpose of the modern nation-state to protect what rights its citizenry enjoys against the claims of individuals from other states. This is a correspondence that is fundamentally about power, that is only made possible with power, and that seeks to reinforce the power of haves against the have-nots. Reus-Smit’s excellent if not foundational book points us in the right direction for unlocking the puzzle of cross-systemic institutional difference. Yet, oddly and simultaneously, it underscores the cognitive distances we must still traverse in search of the appropriate keys.

**Causes of War: Power and the Roots of Conflict.** By Stephen Van Evera. Ithaca, NY: Cornell University Press, 1999. 270p. \$35.00.

Allan C. Stam III, *Dartmouth College*

Stephen Van Evera explicitly sets out to accomplish two tasks. The first is to present a set of five hypotheses on the causes of war grounded in “misperceptive fine-grained structural realism” (p. 11). He lists (1) false optimism about the outcome of a future war, (2) perceived first-mover advantages, (3) opening and closing windows of opportunity and vulnerability, (4) cumulativeness of resources, and (5) beliefs about the offense-defense balance. He then develops 23 related hypotheses. The second task is to test some of the major hypotheses (the second, third, and fifth) against a small

set of cases. He succeeds at the first task but is not so successful at the latter. He also briefly speculates on the effects of the “nuclear revolution.”

The major contribution of *Causes of War* lies in Van Evera's demonstration of the similarity of what heretofore have been presented as competing research paradigms: realism and constructivism. Realists traditionally have focused on the material bases of the balance of power and its presumed effects on international relations; constructivists have emphasized the role of ideas and their socially constructed nature. Van Evera expands the explanatory and predictive power of realism by incorporating leaders' beliefs and misperceptions about “fine-grained power” relations between states (p. 7). In doing so, he demonstrates, however unwittingly, the underlying similarities of constructivism and realism. This is a particularly valuable contribution to international relations theory.

As Brooks and Wohlforth point out, “establishing a strong, independent role for ideas will be particularly difficult when material constraints are especially significant and/or when there is relatively little lag between material and policy changes” (Stephen Brooks and William C. Wohlforth, “Power, Globalization and the End of the Cold War: Reevaluating a Benchmark Case for Ideas,” *International Security* 25 [Winter 2000–2001]). Van Evera manages this well, within the inferential constraints of his method. By noting that “realism thus is most powerful—if we repair it by shifting its focus . . . from power itself to national perceptions of power” (p. 9), he blurs the distinction between realists and constructivists, whose key contribution has been to note the explanatory power of socially constructed national beliefs, or ideas about power and relations thereof (e.g., Alexander Wendt, *Social Theory of International Politics*, 1999).

There is much to like about the book, but there are problems. From an empirical perspective, several weaknesses stand out. Van Evera uses a method perhaps best termed historical constructivism, an empirical analog to the theorized social construction of ideas at the heart of the paradigm. The hypothesis tests consist of clever historical constructions based on carefully selected facts and recollections through which the author demonstrates that beliefs (also referred to as ideas or misperceptions) lie at the core of his microfoundational realist agenda. Van Evera commits four inferential sins that further obscure the eroding distinction between his brand of realism and social constructivism to the extent that, empirically, any remaining differences might be considered superfluous.

First, the observations from which the hypotheses are inferred are the same ones used to test them. For example, when testing his “jumping the gun” hypothesis (p. 63), Van Evera presents the historical evidence in support of the test in footnote 107. Rather than reveal an historical insight, however, this note simply refers the reader to the evidence presented in note 55 (p. 49) and notes 70 and 71 (p. 52), found in the section where the hypothesis was developed. Van Evera is well aware of this and other inferential problems but seems blissfully unconcerned. “The orthodox methodology creed . . . requires that cases not be selected on the dependent variable. . . . It warns against testing theories with cases from which the theory was inferred. . . . It warns against selecting atypical cases loaded with the causal phenomenon. . . . I have never found these rules useful, and my case studies break them all. Readers can judge if my recalcitrance did any harm” (p. 12). What do we lose by adopting this historical constructivist method? Foremost is the ability to distinguish between cause and effect and the ability to falsify judgments thereof. From a methodological perspective, this

is not Wendt's positive social constructivism but historical constructivism in the best tradition of Foucault at his most cynical (see Michel Foucault, *Archaeology of Knowledge*, 1982).

Second, both the tests and hypotheses rely completely on a review of the secondary literature on the origins of World War I and others. We become captive to the judgment of the historians Van Evera carefully selects to construct his argument. On World War I, for example, a more balanced review of the historiography reveals that the Russians' intentions and degree of military optimism were not as clear as Van Evera claims they were. As Goemans points out, “on the one hand, the secondary literature often asserts that like France and Germany, Russia expected the war to be short and victorious. On the other hand, closer study reveals that well-informed decision-makers were often much less sanguine about Russia's relative strength and the duration of a war” (Hein Goemans, *War and Punishment: The Causes of War Termination and the First World War*, 2000, p. 127, in particular note 9).

Third, Van Evera does not randomly sample the facts he cites from the population of states that might go to war. Rather, it appears the evidence has been selected both on the value of the dependent variable and also to bolster the author's argument, not necessarily to get to the truth of the matter (e.g., note 118, p. 66). Since we cannot identify the general population of crises at risk of escalation to major war, it becomes difficult to have confidence in the few policy recommendations based on the findings.

Fourth, the evidence typically consists of individuals' observations, but the stories being tested are state-level arguments. Why is this of concern? Early on, Van Evera makes clear what his brand of realism is all about: citing Robert Keohane, he summarizes: “1. States are the most important actors in world politics, 2. States are unitary rational actors, and 3. States seek power . . . and they calculate their interests in terms of power” (note 11, p. 7). If states are the unitary rational actors Van Evera assumes them to be, then we should search for evidence at the state level or in observations about state characteristics. Instead, most of the evidence is individuals' recollections of how the tide of myopic decisions made by others drove events forward. We could just as easily construct a story using Van Evera's evidence that is more consistent with the psychological explanations of dispute escalation found in Richard Lebow's *Between Peace and War* (1984).

The historical constructivism in *Causes of War* will prove seductive to many because of the polished prose that leads the reader though what appear to be powerful tests of myriad hypotheses. Although Van Evera's construction of fine-grained realism succeeds on this level, it fails on a more rigorous epistemological level of positive social science. In the end, readers will likely take from the book what they bring to it. Realists may find the emphasis on ideas somewhat puzzling, and positivists will be deeply disappointed by the outright flaunting of scientific rules of inference and in the end will find little of systematic substance to take with them.

**Globalizing Concern for Women's Human Rights: The Failure of the American Model.** By Diana G. Zoelle. New York: St. Martin's, 2000. 169p. \$49.95.

Fiona Robinson, *Carleton University*

It is difficult to argue with Diana Zoelle's claim that liberal democracy, as conceived and developed in the United States, is a problematic model in globalizing concern for women's

human rights. Moreover, when she suggests that U.S. ratification of the International Convention on the Elimination of All Forms of Discrimination against Women (CEDAW), although not a panacea for the attainment of full equality, would constitute an important step toward alleviating women's oppression, she is probably correct. Finally, although her claim that the potential currently exists to accord human rights to all people in a world community that is less torn apart by bipolar enmity, less subverted by ideological tensions, and less compromised by the economic priorities of multinational corporations is probably not correct, one cannot help wishing that it were.

It is difficult to disagree with most of Zoelle's arguments, but one is left with the feeling that a great deal more needs to be said. Indeed, the subject of her book—the failure of liberalism, especially in the United States, to deliver women's human rights—is of great importance and has tremendous potential. Yet, too many pages are devoted to long appendices and annotated articles of CEDAW; much of what remains is undertheorized in the extreme. Not until the last chapter is the reader introduced, in a most cursory way, to the central conceptual debates that bear heavily on the topic of this book, such as the relationship between liberal democracy and capitalism; the postmodern ideas of identity/difference, self/other; and the feminist debates about the relevance of “rights talk” to the moral and social lives of women. Moreover, the book puts forward a potentially radical and even original thesis, but the end result is neither. Zoelle is ostensibly critical of liberal democracy, yet she seems to find the whole idea of universal human rights beyond question. A more thorough and profound interrogation of the notion of rights as a moral concept, as well as of the political relationship between rights and power, could perhaps have made this interesting and important book a magnificent one.

*Globalizing Concern for Women's Human Rights* is organized into five chapters, and includes the full text of CEDAW as an appendix. Zoelle begins by arguing that U.S. civil rights legislation has failed in its task of correcting past discrimination against women and racial minorities. She claims that civil rights are different from and secondary to human rights; the latter are universal and incontrovertible, whereas the former are augmentary, supplemental, and subject to the vagaries of politics. The United States has limited itself to an emphasis on civil rights but has failed to take seriously the human rights of its own citizens, while simultaneously choosing to police the behavior of states that have ratified international treaties.

Zoelle raises some important points in this chapter; indeed, perhaps her most insightful claim here is that liberal democracy, as it exists in the United States, is structured around exclusion and oppression. “Further, because the practice of exclusion is not simply civil and legal, but is also sociopolitical, economic, and cultural, more is required to alleviate these problems than civil remedies” (p. 15). This is certainly a crucial argument, but Zoelle's distinction between civil and human rights is perhaps too strongly made. When she states that “civil rights discourse . . . disregards social and economic preconditions for unequal treatment and civil laws are certainly no substitute for inherent human rights” (p. 16), she seems unaware that this argument can and has been leveled at human rights discourse as well, by both feminist and nonfeminist critics. (See chapters by Tony Evans, Spike Peterson and Laura Parisi, Caroline Thomas, and Anthony McGrew and John Galtung in Tony Evans, ed., *Human Rights Fifty Years On: A Reappraisal*, 1998).

Chapter 2 examines the U.S. record on women's human rights and argues that CEDAW is the best means available to articulate a commitment to the rights of women in particular. Zoelle rehearses now familiar (yet still important) feminist arguments regarding the public/private dichotomy in liberalism and state complicity in systematic human rights violations against women. Chapter 3 discusses the ongoing debate regarding difference among women and the problem of constructing a coherent voice in the expression of “women's” concerns. The arguments are clearly stated, and the key issues are highlighted, but Zoelle's position lacks rigorous theoretical support. When she claims that “it is not difference that must be eradicated but the attendant asymmetry in power arrangements” (p. 64), the reader is left unsure of the conceptual starting points that lead her to this conclusion. Indeed, when Zoelle makes the bold, and somewhat paradoxical, claim that “diversity is a fundamental truth” (p. 64), she seems to be appealing at once to poststructuralist approaches as well as to a kind of moral and epistemological absolutism.

Chapter 4 takes a slight turn to examine U.S. foreign policy vis-à-vis human rights. Zoelle describes as “perplexing” the record of the United States on arms sales, economic assistance to oppressive regimes, and support of U.S.-based multinational corporations in highly repressive nations. Again, it is certainly to her credit that she makes these arguments, yet it is disappointing that she has not drawn on the bounteous literature that offers some credible explanations for these “perplexing” activities. (See Tony Evans, *U.S. Hegemony and the Project of Universal Human Rights*, 1996, and Noam Chomsky and Edward S. Herman, *The Political Economy of Human Rights*, 1979).

The final chapter is in many ways the best. Zoelle begins to engage with theoretical and conceptual arguments and introduces some important debates within feminism, liberalism, and human rights theory. The relationship between democracy and capitalism is finally mentioned, but the tone is more assertive than analytical: “Liberal democracy in conjunction with a market economy fails to address the situation of those within the state who have been, historically and globally, denied full access to the institutions and instruments that regulate their lives” (p. 106).

The spread of capitalism and liberal democracy worldwide seems to contradict Zoelle's earlier claim that “the potential exists to accord human rights to all people in a world that is . . . less compromised by the economic priorities of multinational corporations” (p. 4). Curiously, her brief discussion of global capitalism and the exclusion for which it is responsible is followed almost immediately by a reference to the poststructuralist arguments of David Campbell, who regards exclusion, by contrast, in discursive terms: “identity is achieved through the inscription of boundaries that serve to demarcate an inside from an outside, a self from an other, a domestic from a foreign” (quoted in Zoelle, p. 107). Some theoretical consideration of materialist versus discursive analysis is surely needed here. This is also true of the hugely important question raised but not explored on page 111: “Can a rights discourse serve as a vehicle for expression of women's claims?” Indeed, that this question is even worth posing demonstrates the potential fragility of the premise on which Zoelle's entire project is based. Even if the author would answer the question in the affirmative, as I believe she would, by engaging with the feminist criticisms of rights-based approaches she would have strengthened the arguments of her ambitious and important book.