

BOOK REVIEWS

Political Theory

The Future of Teledemocracy. By Ted Becker and Christa Slaton. Westport, CT: Praeger, 2000. 248p. \$65.00 cloth, \$24.95 paper.

Darin Barney, *University of Ottawa*

As Ted Becker and Christa Slaton affirm in their introduction to *The Future of Teledemocracy*, this is not “just another book” of scholarly reflection but, rather, an account of “a way of life” (p. xii) in which they have played a central role. Consequently, instead of a detached, critical investigation of the dynamic encounter among technology, communication, and democratic politics, or even a dispassionate appraisal of the implications of a particular aspect of this encounter, what we receive in this book is an intimate digest of Becker and Slaton’s 23-year “odyssey” on behalf of their own vision of teledemocracy—complete with “true blue allies” and powerful enemies “who opposed our ideology and had the means to halt our experiments” (p. xi)—framed by a futurist manifesto that unfortunately diminishes the contribution made by the chronicle of the authors’ crusade.

The argument driving the book is relatively straightforward: The dawn of the third millennium has brought with it an array of “uniquely menacing dilemmas” (p. 7)—civil war, poverty, environmental degradation, Third World debt, disaffected youth, etc.—with which representative democracy, dominated by “tiny cliques of economically powerful and well-organized interests who are, by and large, sexist, racist and Social Darwinists at heart” (p. 6), is ill equipped to deal. The solution to these “threats to human viability” is teledemocracy: a “purer, future democracy” (p. 7) that makes liberal use of direct democratic instruments and new information and communication technologies, preferably on the model developed by Becker and Slaton in the course of more than two decades of experimentation. In their view, this “New Democratic Paradigm” is imminent, especially in America, the progressive center of the “one continent on this globe generating a series of impulses that contain the best way for humankind to work together, live together, grow together and govern together” (p. 8). This “wave of the future” (p. 9) is the political analog of the quantum revolution in physics (highlighting randomness, uncertainty, and unpredictability), its progress hindered only by political, economic, and media elites whose interests are wedded to the current representative system and the outmoded “Newtonian paradigm” (emphasizing reason, causality and hierarchy) upon which it is based and legitimated. The realization of teledemocracy requires a leap to “quantum politics” (p. 36), to which Becker and Slaton see their work as making a contribution.

The book is divided into three parts. Part I traces the lineage of teledemocracy in the ideas of a selection of “democratic visionaries of the 20th century” (p. 11), including Buckminster Fuller, Erich Fromm, and Abbie Hoffman. It also presents the case linking the failure of contemporary representative institutions to the Newtonian thinking which informed their founding, and for how a “political-scientific quantum correction” (p. 28) will “reinform our political thinking and processes in order to ameliorate some of the most debilitating and most lethal crises all modern governments face” (p. 21). Part II recounts the history of Becker and Slaton’s experiments in “quantum politics”: Deliberative polling and televoting; electronic town meetings (ETMs) mediated by television, telephone, and computer technology; and the integration of Internet utilities into these models. This part also

includes an interesting discussion of the authors’ experience developing “a commercial enterprise devoted to producing ETMs for a profit” (p. 119). Part III imagines the future of teledemocracy in the quantum age. Drawing on Benjamin Barber’s idea of strong democracy and the futurism of Alvin and Heidi Toffler, Becker and Slaton forecast the proliferation of a global direct democracy movement, expansive implementation of technologically mediated direct democracy initiatives, the establishment of mediation as the core of “non-hierarchical, quantum-style conflict resolution” (p. 191), and the transformation of political organizations and institutions by the Internet. In other words, the future of teledemocracy will bear a marked resemblance to precisely what Becker and Slaton have been doing over the past 20 years.

There are sections of this book that are informative. The authors’ recollection of their journey of social experimentation aimed at transformation holds lessons for those embarking on a similar trajectory, and their expertise undoubtedly will help identify best practices in the execution of teledemocracy exercises. Additionally, the section on the global direct democracy movement (pp. 158–78) provides a useful, if selective and uncritical, catalog of electronically mediated democratic activism around the world. Becker and Slaton do well to point out that political and economic elites who continue to benefit from the current configuration of power and technology have little to gain from real democratization and will use their existing advantage to preserve the status quo. Unfortunately, they assert rather than argue this point, typically at a level of rhetorical excess that undermines its persuasiveness. To their credit, they also seem aware that the combination of direct democratic and technological instruments is open to manipulation by these same elites in ways that undermine efforts at fundamental democratization. However—with the exception of a brief critique of Ross Perot’s presidential campaign, apparently inspired by the candidate’s rejection of the authors’ offer of expertise in designing ETMs (p. 103)—this possibility is not adequately explored.

Chief among the book’s shortcomings as a piece of research writing is its parochial stance relative to the broader scholarly discourses in which it might otherwise have been profitably situated. While Becker and Slaton do refer to fellow travelers on the experimental road to teledemocracy, with the exception of brief references to Barber, Amitai Etzioni, and James Fishkin’s work on deliberative polling, there is little engagement with the theoretical or social science literatures that examine a number of issues with which Becker and Slaton ought to be concerned. Thus, the book suffers from a lack of sustained conversation with prominent contemporary theories of democracy, participation, deliberation, representation, technology, or citizenship and from minimal reference to the extensive mainstream social science research on direct democracy, the political economy of media, or the emerging political dynamics of digital technology. The result is that complex critical concerns about the potential pathologies of direct democracy and the Internet are glossed over, possibly because these instruments are inextricable from the “quantum democracy” to which the authors declare themselves committed on a personal level. How else are we to explain, in the age of Microsoft and Time–Warner/AOL, the authors’ claim that the “New Internet” is a system of mass telecommunications with “no owners,” “no gatekeepers,” and under which “all Web sites are equal” (pp. 136–7)? Becker and Slaton refer extensively to their own previous work, and to that of their close circle of colleagues. However, more systematic attention to issues raised by theorists and scholars outside the fraternity of “friends of the ETM process” (p. 106) may have made the authors’ case in favor of electronically

mediated direct democracy more persuasive, and less exhortative.

Becker and Slaton are correct in their conviction that American democracy bears a name it does not deserve, and there is no reason to doubt their commitment to radical democratization or to dismiss out of hand the contribution teledemocracy might make to its realization. It is entirely plausible that televotes, technologically mediated deliberative polling, and electronic town meetings will become a more prominent element of public life, and it is certain that activists will struggle creatively to ensure that these processes reflect the best, rather than the worst, face of democracy. The counsel of intellectuals such as Becker and Slaton will contribute mightily to this struggle. That being said, this book disappoints as a revolutionary manifesto, primarily because it is utopian in precisely the sense in which Marx and Engels understood that word. There is nothing wrong with imagining and working toward a better, more democratic future. However, to assume that its achievement awaits "a correction of the underlying theory of all our social, economic and political structures" (p. 3) rather than an upheaval in the material conditions which support prevailing configurations of power serves only to misdirect the energies of genuine change. To paraphrase Marx on wage labor, we cannot do away with domination and inequality simply by eliminating the thought of them. To suggest the persistence of "Newtonian thinking" is the chief obstacle to a substantial democratization of liberal capitalism, or that "a quantum correction to the Newtonian political thought" (p. 153) will achieve this, is ultimately disempowering. Equally utopian are the authors' concluding projections that teledemocracy will reverse "the forces of rampant, market-based globalization," yield "a fairer allotment of wealth and social services... reverse the severe degradation to the planet's ecology in the 20th century... [and] work some positive healing of the human psyche and spirit..." (pp. 211-2). It remains to be seen what the broad impact of technologically mediated direct democracy will be. Becker and Slaton succeed neither in establishing that the most important variable determining this outcome will be the prevailing theory of the nature of the cosmos, nor in adequately investigating what some of the other, more significant variables might be.

Sexual Identities, Queer Politics. Edited by Mark Blasius.

Princeton, NJ: Princeton University Press, 2001. 382p. \$60.00 cloth, \$19.95 paper.

Sexual Strangers: Gays, Lesbians, and the Dilemmas of Citizenship. By Shane Phelan. Philadelphia: Temple University Press, 2001. 232p. \$59.50 cloth, \$18.95 paper.

Anna Marie Smith, *Cornell University*

Shane Phelan and Mark Blasius have played a leading role in the development of a lesbian, bisexual, gay, and transgender (LGBT) literature within political science, and their most recent texts enrich both this specialized field and the study of politics as a whole. Phelan's investigation of citizenship begins with the claim that LGBT people are "strangers" in American society. According to Zygmunt Bauman (Zygmunt Bauman, *Modernity and Ambivalence*, 1991), the Jews in European history were strangers. Neither friend nor enemy, they were treated as an ambiguous and shifting figure of otherness that marked the boundary between us and them, but at the same time calling into question the very possibility of such a clear exclusionary division. Critical readings of anti-Semitic discourse can be quite fruitful for the study of homophobia because the demonized subject in both cases is an enemy within who is simultaneously excessively present but

invisible; powerful and threatening but harmless and effeminate; physically close but spiritually remote; embodying the highest intellectual and aesthetic values but expressing modern urban decadence and decay, circulating in "mainstream" society and seductively corrupting the innocent, but shunning "mainstream" society and remaining an unassimilated difference within its own separatist cultural enclave. Phelan puts Bauman's stranger trope to good use as she explores the complex positioning of LGBT people in American official discourse and culture.

Drawing from both the liberal republican and feminist traditions, Phelan defines citizenship in terms of legal status, meaningful participation, and recognition. Progressive liberals might be content to argue solely for the extension of basic civil and political rights to LGBT people, but Phelan complicates matters by pointing to the symbolic and discursive dimensions of participation. To have one's voice heard in a public debate, there must be some shared understanding that one deserves respect as a valued member of the community. Working in a similar vein, Nancy Fraser has explored the importance of combining redistribution and pluralistic recognition strategies in her work. Phelan's specific contribution consists in her discussion of the complex ambiguities that homophobia presents for democratic citizenship theory. LGBT people are not singled out for apartheid-style treatment—access to the vote, public office, public accommodation, and residential property ownership is not organized according to sexual classes in a systematic manner. Yet, *Bowers v. Hardwick* establishes that gays do not have a right to privacy; the "don't ask, don't tell" military policy bans self-affirming lesbians and gays from the military; the criminal justice system usually fails to address antigay violence; and laws protecting LGBT people from discrimination are weak, few, and far between. Phelan contends that although LGBT people do have access to many fundamental rights on a formal basis, the denial of other rights and symbolic recognition tends to diminish the political effectiveness of our citizenship.

Phelan concludes that democratic activists must work to introduce an antihomophobia and/or proqueer ethos of solidarity into American society. From this perspective, she urges conservative homosexual leaders to abandon their assimilatory attempts to depict lesbians and gays as just another middle-class suburban ethnic group. She contends that their tactics have become particularly offensive insofar as they have borrowed a typical ethnic group normalization tactic and have positioned "normal" homosexuals as "mainstream" citizens by attacking the marginal figures in our own community—the transsexual, the gender nonconformist, and the bisexual. This mainstreaming approach will inevitably fall short and backfire. Even if the conservative homosexuals secure small reforms, unless the deeply engrained systems of antigay bigotry and discrimination are addressed, many LGBT people will remain in the closet and will not exercise their rights. Furthermore, heterosexuals will not be pressed to examine their assumptions about sexuality, sex roles, the family, and the community, and the support from opinion-sensitive politicians and judges will remain so shallow that many of them will abandon the reforms as soon as they are pressured by the religious Right.

Although Phelan is entirely convincing on these points, she could have engaged more substantially with radical queer theory, which has much to offer to any critique of assimilation. Phelan's analogy between the exclusion of LGBT people and racism and ethnic discrimination is quite interesting, but she also could have given more consideration to the ways in which homophobia remains somewhat unique. The expulsion of LGBT people from their birth families, for example, is a

distinct feature of homophobia. Although families in some fundamentalist religious communities systematically expel their children if they reject their faith, this sort of deep-seated intrafamily antagonism is generally quite rare within racial and ethnic groups.

The Blasius anthology deals with the politics of sexuality. The chapters span the entire political science spectrum in terms of research paradigms. Some of the essays were first presented as papers at a 1996 conference, and the high quality of the work is such that even the oldest pieces have stood the test of time. Blasius nicely situates the various chapters so that interesting contrasts emerge. Rayside's overview of LBGT social movement politics in the institutional contexts of the United Kingdom, Canada, and the United States sets up Duyvendak's treatment of gay organizing in the French republican political culture and centralized political system, as well as Diaz-Cotto's analysis of Latin American lesbian-feminist activists' negotiations of the uneven organizing opportunities created by a series of feminist conferences.

The comparative politics chapter by Altman addresses international cultural politics. He demonstrates that in some Southeast Asian gay male communities, Western LBGT ways of thinking about sexuality have been enthusiastically taken up by new generations of bar queens, but Western-style activism has generally not taken root in the difficult economic and political conditions of the developing world. Petchesky, by contrast, emphasizes deliberate coalition-building tactics that have brought feminists together from both the north and the south. Her chapter discusses the progress that feminists have made in institutionalizing sexual rights within official international human rights discourse.

Blasius, Currah, and Bailey call into question popular approaches to LBGT subjectivity. Blasius argues that the effect of an LBGT presence should be theorized in terms of our community-based production of a specific form of emergent subjectivity—the dialogical process of self-constitution or “coming out”—rather than social movement or interest group analyses. Currah argues that lesbian and gay advocates ought to abandon the conservative tactic of constructing sexual orientation discrimination as simply a type of sex discrimination (e.g., Mitchell *Y* would have been able to marry Juan *X* if Juan had been a woman) but should instead struggle against any attempt by the state to regulate or punish those who deviate from traditional gender roles, inhabit transsexual bodies, or express unconventional sexualities. Bailey's analysis of lesbian and gay households in urban settings suggests that the LBGT presence should not be depicted as easily defined and homogeneous neighborhoods—the “gay ghettos”—but as highly differentiated, transitional, and shifting urban spaces in which LBGT people from all economic brackets coexist with seniors, female-headed households, and racial/ethnic minorities.

Several authors explore questions related to political strategy. The Cook and Hartnett analysis of television news coverage traces the ambiguous representations of lesbians and gays as worthy individuals and as a dangerous collective movement, thereby revealing the unevenness of strategic media opportunities. Harris deploys a key LBGT studies tactic by championing the work of a neglected gay African-American philosopher, Alain Locke. Yang rejects an elitist approach to mass opinion change, which suggests that “experts” always play a leading role in changing popular attitudes, and demonstrates that the privileging of “experts” depends on the precise issue in question. LBGT activists influenced the American Psychiatric Association's decision to remove homosexuality from its list of pathological disorders but were unable to shape the military's “don't ask, don't tell” policy.

Echoing Foucault, Yang points out that democratic struggles always involve struggles for the authority to determine truth and morality. Salokar urges LBGT activists to maximize the opportunities presented by the federal and multibranch nature of American government by working at all levels and in all areas of politics, while Badgett underlines the importance of LBGT think tanks.

The Blasius collection includes one of the most important pieces published in the entire LBGT literature during the 1990s (given the size, breadth, and quality of this body of texts, that achievement is no small feat), namely, Cohen's critique of radical queer theory. Whereas queer theory has constructed the political as a single struggle—LBGT people versus heterosexuals—Cohen points out that such a construction fails to capture the complex nature of the sexuality-oriented relations of domination that are central to contemporary American politics. Poor women of color have been attacked as sexual deviants and subjected to increasingly punitive welfare programs precisely because of their heterosexual fertility. Cohen affirms that homophobia remains a serious obstacle to progressive social change, but she urges LBGT activists to abandon the single-issue approach to politics. She concludes that the radical promise of apparently transgressive queer activism will only bear fruit to the extent that we grasp the intersections among homophobia, institutional racism, class exploitation, and the patriarchy and situate a radical, antiassimilationist struggle for LBGT rights within a broadly based leftist, feminist, and antiracist coalition.

It can be argued that both books are significant because they transcend the “narrowness” of LBGT politics and address much broader and universal questions of ethnic group politics, democracy, and citizenship. These texts are in fact significant, but we should not have to judge their importance in this manner. It is true that Phelan moves deftly back and forth across LBGT, feminist, multicultural, and democratic theory literatures with ease and to great effect, and the Blasius collection features first-rate works drawn from every corner of the entire political science discipline. These texts should find appreciative audiences not only in LBGT studies but also in the fields of feminist theory, social and political theory, and political science.

But such an assessment would implicitly accept the premise that LBGT politics is an inherently specialized and narrow field of inquiry, and that mainstream scholars should only value LBGT studies when we translate our work into mainstream terms. Literature departments, for the most part, have demonstrated that they welcome the very best humanistic scholarship, and the fact that some of it addresses problems related to gender, sexuality, and race has not interfered with their assessments. If our humanities colleagues have been more open to LBGT studies, our social science colleagues have, for the most part, failed to value this field of inquiry. The latter have often used the “particularism”/“universalism” code to conceal their exclusionary attitudes. Ultimately, the claim that a given knowledge project is particularistic instead of universal can only be defended in political terms; indeed, the charge of particularistic and narrow against LBGT studies is nothing more than the latest version of homophobic pathologization and erasure.

As Phelan and contributors to the Blasius book demonstrate so well, LBGT politics is not a narrow, special interest group, minority concern; it is a field of struggle that brings the very foundational architecture of American citizenship to light. In the words of Salokar:

To study the LGBT movement, scholars will need to move beyond a narrow definition of politics and entertain the notion of “political” as encompassing the social, cultural,

ethical, and moral issues of society. What is sought by many LGBT activists is not simply political equality, equal protection of the laws, or the legal recognition of their relationships, but a wholesale examination and retooling of the most basic power relationships among individuals, social institutions (e.g., the family), and the regulatory capabilities of the state in order to shape a more just society. Ultimately, the LGBT movement is seeking broad-based change that is "political" in all sense of the word (*Sexual Identities*, p. 260).

At the Heart of Freedom: Feminism, Sex, and Equality.

By Drucilla Cornell. Princeton, NJ: Princeton University Press, 1998. 254p. \$14.95 paper.

Gayle Binion, *University of California, Santa Barbara*

Drucilla Cornell has two goals: Pinpoint equal freedom as the core of sexual equality and make the case for the equal rights of gays and lesbians. Interwoven within these themes is a case for sexual freedom itself, for men and women. With erudite references to a wide multidisciplinary swath of literature, she succeeds in hammering home these concerns and in demanding that the sociolegal order reform its policies affecting sexuality, reproduction, and definitions of family. In these respects, this is a valuable study of how the United States specifically and other societies referentially fall short of what Christine Littleton calls making sex "cost free." Cornell's book, which in the subjects and issues it analyzes covers very familiar territory, is intriguing for a very different reason. It is one of a very few works in radical feminist thought that is fundamentally about employing the tenets of classical liberalism, if not libertarianism, in the service of progressive social change. In contrast with the paradigms of modal feminism, which address social structures and connectedness, and which are concerned predominantly with equality, this work unabashedly focuses on the individual and stresses the freedom of each as a sexual being.

Despite Cornell's creativity, extensive research, and erudite references, the book is not entirely satisfactory. Most immediately, it builds broad general theory from stated but largely unproven premises: (1) One's "sexuate being," defining and representing one's sexual self, is a/the critical foundation of personhood, and (2) the state is largely responsible for our exclusionary views of normalcy in the "imaginary domain." Each premise is repeated throughout and functions as the framework within which Cornell offers legal analyses and suggestions for reform of public policy concerning primarily pregnancy and parenting, but they are not effectively explored as to their centrality, and some readers will remain unpersuaded.

Although it is nearly axiomatic that feminist theory, of which Cornell is self-consciously a major source, serves to challenge sex role orthodoxy, it is rather less clear that the enterprise of feminist jurisprudence is advanced by defining the essence of individuals as inherent in their sexuate natures. By this I mean to separate and applaud the very critical case that must be made, and is made by Cornell, for full freedom and equality whatever one's sexual orientation. It might be noted that a fair application of strict scrutiny analysis to sexual orientation would render unconstitutional all forms of governmental discrimination on the basis of this criterion. Few classifications better fit a test designed to address characteristics that are immutable, historically the basis of arbitrary exclusion, and unrelated to one's ability to contribute to society. But is this case against what is, in my mind, purely and simply bigotry necessarily advanced by an argument that one's sexuate being is the essence of the person from which

principles of freedom, generally, do themselves flow? Perhaps this is but a quibble as to how critical sexuality is to freedom in social and political spheres of life, which is arguably a case apart from equal acceptance and integration of people of all sexual orientations into the fabric of society. The former case is not made particularly effectively by Cornell.

I have more problems with the excessive reliance on the proposition that the state is the source of the exclusionary social norms that all but heterosexual men and heterosexual women who are deferential to men are understood to experience. There is a general axiom in law and society research that, "awaiting contrary proof," public policy is more often than not presumed to be a dependent variable, a phenomenon to be explained by other social institutions and forces. Law is seen not as the source of social norms but as a reflection of these values, buttressing and reinforcing them. Cornell's analysis of patriarchy demonstrates the weakness of viewing the state as the problem. "For my purposes, the word *patriarchy* indicates the manner in which a woman's legal identity remains bound up with her duties to the state as wife and mother within the traditional heterosexual family. Our feminist demand must be for the full release of women from this legal identity that is wholly inconsistent with the recognition of each of us as a free and equal person" (pp. 101-2).

Arguably, patriarchy, which long predates the development of formal institutions of government and law, would continue to exist even if there were no state policies enforcing its strictures. It is, therefore, difficult to understand not only which policy changes will undo patriarchal institutions but also how any changes in contemporary law would necessarily effect positive social change in this arena. The case that is seriously in need of being made is how and under what circumstances the state can effectively intervene and undo the conditions of disempowerment and exclusion that are endemic in the social order. This may be a Herculean task, if not a fool's errand. Carol Smart, in *Feminism and the Power of Law* (1989), suggests that the law is unreformably patriarchal and that women must look elsewhere to further their interests. The danger, Smart (p. 3) notes, is to buy into the law's "overinflated view of itself."

Cornell's work would have benefited from more effective copy editing. Some statements, such as that Congress "overturn[ed]" *Geduldig v. Aiello* (1974) in the Pregnancy Discrimination Act of 1978 (p. 66), are not accurate. (By statutory law, Congress could not undo the constitutional doctrine of the case denying that discrimination on the basis of pregnancy is sex discrimination under the fourteenth Amendment.) Other debatable observations would have been aided by documentation, for example: "There is widespread agreement among psychologists and psychoanalysts that addiction is inseparable from a blocked longing to be a person" (p. 81).

Cultural Studies and Political Theory. Edited by Jodi Dean.

Ithaca, NY: Cornell University Press, 2000. 362p. \$45.00 cloth, \$19.95 paper.

Jill Locke, *Gustavus Adolphus College*

Film, painting, disease, nationalism, mass media, literature, family values, capital punishment, music lyrics, theater, UFOs, the Statue of Liberty, and Appalachian hollers are some of the cultural texts subject to political analysis in this impressive volume of sixteen essays. Neither are these cultural sites presumed to be transparent nor are their investments fully known. They are problematized, pluralized, specified, and contextualized in terms of such political categories as liberalism, communitarianism, privacy, civility, nationhood,

citizenship, community, the will, responsibility, the common, revolution, public sphere(s), and the political itself (p. 19). This approach, which Jodi Dean terms an “interface” between cultural studies and political theory, captures how cultural forms such as film and literature cannot be decoupled from their political moorings, just as a political concept like citizenship is always embedded with cultural meanings.

All the essays unpack and rethink the political entanglements, investments, and effects of cultural artifacts, narratives, and sentiments. Some contributors approach this interface expressly; others take its power and validity as givens and proceed by example. In the first mode, Judith Grant and Paul Apostolidis establish criteria for determining whether cultural work has political purchase. Grant examines the turn toward art and culture in the tradition of critical theory as a positive expansion of politics and meaning-making spaces that turns away from economics. But Grant also warns that, without consciousness, cultural work is not emancipating, and its commentary is not political action. Apostolidis argues that criticism on its own is not political without a commitment to democratic leadership, which he defines as “the task of forging alliances between political intellectuals and groups of ordinary people” (p. 144).

The majority of contributors fall in the second mode, sitting more easily with the cultural studies/political theory interface. Mark Reinhardt concludes that even the most generous incarnations of communitarian thought (represented in his essay by Daniel Bell) still fail to conceptualize and therefore obfuscate myriad forms of “cultural struggle” (p. 108). Priscilla Wald reads “carrier narratives” from *Typhoid Mary* to Robin Cook’s *Invasion* to show how communicative disease renders us “all related” and therefore consolidates and decimates community (p. 201). Linda Zerilli examines the U.S. investment in seeing itself as the “real” democracy vis-à-vis national narratives about the Statue of the Liberty as the referent for the Goddess of Democracy held up in Tiananmen Square. She chronicles the history of the statue from its beginnings, as the “gift that nobody wanted,” to the miniatures produced for tourist consumption today. Dean’s concluding essay celebrates conspiracy theory, typically debunked as paranoid fanaticism, as part of the American tradition of political resistance present in the Declaration of Independence, whose conspiratorial tone unsettled the certainty of who held legitimate power.

Because these essays embrace, without regret, the pluralization of politics, they are meaningful contributions to debates about the expanding or shrinking public and political. Such debates are often played out in terms of a geography of public and private wherein certain concerns are considered essentially public, others indubitably private, artistic, cultural, economic, and so on. Hannah Arendt (*On Revolution*, 1963; *The Human Condition*, 1958), Sheldon Wolin (“Political Theory as a Vocation,” *American Political Science Review* 63:4 [December 1969]: 1062–1082 and “What Time is It?” *Theory & Event* 1 [No. 1, 1997]), Michael Walzer (*Spheres of Justice*, 1993), Jean Bethke Elshtain (*Democracy on Trial*, 1995), their commentators, and others have been engaged in debates about this kind of political mapping. Even contemporary feminist scholarship has become uncertain about its stake in making the personal and cultural political.

These essays neither police what counts as political nor make everything so. They detail the workings of power that proliferate beyond the so-called public sphere (and are often constitutive of it) and affect our politics and our culture, and they show us the political failures in working to keep the two distinct. Thomas Dumm discusses the ostensibly mundane act of a group of people in a bar singing the lyrics to “Wild Thing.” He examines the political meaning of how the audience (all

but Jean Baudrillard, whom Dumm explains was quite baffled by the singing delirium) knew that the opening chords signaled “Wild Thing” and not its chordic sibling, “Louie, Louie.” Michael Shapiro juxtaposes the story of his grandfather with the movie *Dead Man*, an approach well outside the domain of mainstream political theory, as he invokes not only film but also family history and personal experience to show the “radical contingency of the family” (p. 270, emphasis in original). It is not that everything is now political in this interface of political theory and cultural studies, but nothing is essentially not political either. To equate the political with everything would have a paradoxically depoliticizing effect. But to show how political meaning is configured and reconfigured through cultural phenomena is to work in the spirit of the essays collected here.

This book challenges political theorists to be more capacious in their treatment of the political, more attuned to cultural work that unties their precious categories. It calls on cultural studies scholars to be more deliberate about the political potential of their insights into the subtle cultural feedback that they so convincingly deconstruct. Given that global capitalism lurks as a persona non grata throughout the text, its entailments could be illuminated more directly. Specific attention to the flow of global capital, examination of the economic and cultural imperatives that fashion the global citizen as global consumer, and closer engagement with non-American culture on its own terms would have expanded this volume’s already inspiring effort to problematize, pluralize, specify, and contextualize the production, meaning, and circulation of politics. As individual essays and as a collection, however, this book will reach far into the bookshelves and classrooms of critical legal theory, democratic theory, American studies, feminist theory, critical race theory, and other untapped resources for political/cultural imagination.

A Politics of the Ordinary. By Thomas L. Dumm. New York: New York University Press, 1999. 240p. \$55.00 cloth, \$18.50 paper.

Fred M. Frohock, *Syracuse University*

Exploring the ordinary is a reasonable and fun way to get through the day. Thomas Dumm takes the exploration along a cart path toward democratic politics, dramatizing the intersections and reciprocal influences of everyday life and political events and the forces of conformity and normalcy that shackle the ordinary. The working technique is juxtaposition, the kind of display that one finds in the store windows of, well, ordinary life in towns and cities. The pantheon of familiar figures and texts includes Emerson, Thoreau, Nixon, Disney, alien depictions, Lowi, Wolin, Cavell, the King’s Two Bodies, Baudrillard, and many more, all offered as showcase for the book’s main claim that the ordinary is the primary source of the democratic imagination.

There is an early gesture toward methods. Dumm acknowledges a distinction between proof as “an illustration of form” and a “showing to be true” (p. 7). The former is a proof through exemplification; the latter, an effort to fix a definite meaning through placement. It should come as no surprise that Dumm embraces illustration and example and, in doing so, tries to preserve the ordinary with an approach traceable to classical philosophy. The book that follows this method is a presentation of the forms of ordinary life, an inventory of events, anecdotes, narratives, film, and fiction, in general a set of portraits that represents the private, common, personal stuff that we recognize as ordinary life. For Dumm this tentative and spontaneous life world expresses and mediates politics “understood as a capacity and a yearning” (p. 5).

Theory, understood as an explicit attention to the rival forms of understanding, explanation, argument, rules of evidence, and inference used to explain human experience, is no part of this book. Instead we have a collection of insights that draws upon the actual texts at hand and elsewhere to provide smart observations on unexpected connections and meanings. Dumm confesses that he finds theorists such as Habermas and Rawls, and debates about items such as liberalism and communitarianism, of little use as inspiration, in helping us to breathe, preferring instead the leads found in popular culture. The result is a book that reads like a collection of film reviews, biting, funny, enlightening in concrete ways, and leaving us with a wish to have a beer and a conversation with the author.

But so much is missing and missed in a work like this. Saul Bellow once said that he could not bear to read "The Adventures of Augie March" because he saw too many missed opportunities. Here are some opportunities and distinctions that fly away when Dumm puts pen to paper (or fingers on the computer keyboard). First, Dumm often writes as if the ordinary is in some earth chamber under siege, sealed off from flights of speculation and conjecture. But there is no required hostility between ordinary life and the larger forces of hegemony, codification, and controlling power that Dumm sets against the ordinary as both concept and event. The most ordinary event in the world is the slide from event to abstraction, from the quotidian to the generalizable account, from ambiguity to provisional certainty. The child's repetitive *Why?* question, as ordinary as any item in human experience, leads to infinite regresses and halting games at wonderful levels of pure abstraction, a domain of the ordinary that can easily examine (and dismember) the heavy theories of mass thinking. Remember that the search for timeless, permanent, transcendent truths is the most commonplace occurrence in history. Some of the best political arguments are street discussions of justice and the public good. Or, the counterplots of the ordinary have their own benign origins in the ordinary. Only a very few hints of these curious mergers are found in Dumm's presentations of ordinary life. Explore them and demarcations between the ordinary and its various antitheses may vanish.

Second, much of contemporary social theory is indeed turgid, unreadable and unthinkable, and (I would add) intellectually dishonest. But current failures of thought and expression are not indictments of abstract thought. They are just poor instances of theory. Good theory is usually elegant and beautiful. Read again Watson and Crick on discovering the double helix as the form of DNA. Third, without some work in theory, explorations even of the ordinary have as little use as last year's film reviews. There are rewarding ways to employ theory and avoid the ghastly trappings of bad social theory. Look at Roy Rappaport's magisterial *Ritual and Religion in the Making of Humanity* (1999), a work that combines rigorous anthropological research into types of ordinary life with analytic philosophy, to the benefit of both.

The oddest thing about Dumm's book is that he follows the lead of most sociologists today in rendering experiences from the outside, never discussing their internal meanings or truths, and Dumm himself seems always in a state of readiness to identify patterns and symbols that actors and authors almost never intend or recognize. For example, he presents the film *Independence Day* as a "reflection on the crisis of American national politics that is precipitated by the decline in confidence in our system of representation generally..." (p. 146). The 1978 remake of *Invasion of the Body Snatchers*, Dumm asserts, "was directed at Jimmy Carter" (p. 152). *The Exorcist* is "a Watergate scandal film" (p. 153). And so on, with one thick imposed connection following another in a book that celebrates the ordinary.

One of my colleagues once asked the director Stephen Frears what he meant by the high shot of the action at a crucial point in the film *My Beautiful Launderette*. Frears said that on the day of the shot another crew had left a crane on the set so, what the hell, let's use it, and the famous scene was shot. Accidental to the core. Conceded, the author's intentions are not privileged in understanding a text, and if something is there it doesn't matter a lot whether or why the author put it there. The question is whether any bridge theory is privileged in accounts of the ordinary. I finished reading Dumm's book the day after the director in charge of *Eastenders*, the most popular program on English television, spoke to my London seminar. In response to questions about symbolic space he said what I have heard so often: that kind of stuff is the business of the literary critics. What I do, he said, is just present popular culture: wise, because, as Dumm recognizes, the ordinary is indeterminant at its center, which is one source of its life blood. But then one cannot just do bad and misleading literary theory, imposing patterns from the outside in place of fieldwork into ordinary life and the popular culture in which it is represented.

In the last chapter ("Wild Things") Dumm raises the question at the center of an intersection between the practical sides of ordinary life and philosophy: Why exist? He is right that contemporary theory is not much help in negotiating this primordial inquiry. But then consider Dumm as guide on the welcome contingency of language: "It requires a particular valorization of ordinary experiences, an attempt to emphasize the powers of experience while avoiding the temptation to make experience into a force for normalizing the ordinary" (p. 168). Thank you, but I prefer the less trendy languages of good anthropology and the insights that sound research can provide on ordinary life.

The Cunning of Unreason: Making Sense of Politics. By John Dunn. New York: Basic Books, 2000. 401p. \$20.00.

Nadia Urbinati, *Columbia University*

If I had to talk about this book to my students, I would begin by describing its cover. The image of the upside down Capitol with the cupola hanging over the key word of the title (*Cunning*) invites the reader to peruse this fascinating and melancholic book as a species of inverted Hegelianism. This is a book about regret. It regrets what seems out of reach—politics as "something... uniquely courageous, direct, and even potentially effective in its assault on the misery and injustice of the great bulk of collective human life" (pp. ix–x). Its perspective is realistic but contains a glimmer of hope. Dunn's realism is ateleological and open to a moderate voluntarism; it conveys a disillusioned pessimism along with the belief that our politics can be less corrupt and cynical. Hope counterbalances the inexorable fatalism of reason. Its source is acceptance of the limits of human rationality, an endogenous fact unamendable to History's infallibility. Hope is the daughter of judgmental fallibility, the hunch that there is "room for maneuver" (pp. 347–8).

Written for a cultured public as well as for political scientists and theorists, this is a book about what politics were, are, and could be. Dunn makes politics an object of knowledge (the first question he asks is: "What exactly is politics?") (p. ix) because he believes that knowledge can make human agency "potentially effective" and "a locus of value" (p. 111). In order to understand modern politics, we must analyze how the classical political categories have changed. In order to solve the problem of contemporary political science, we must keep the general norm and the specificity of the actual context together. Thus, although he acknowledges, realistically, that

interests are powerful factors in human agency and a limit to politics, Dunn wants to preserve the leading role of politics. The goal of modern representative democracy is to keep “a polity in good order” by “the selection as rulers of those with the intelligence, moral commitment and strength of character to rule as they should” (p. 268). To attain this goal, politics needs the “great imaginative force” of the ideal of impartiality to rectify the partial in political judgment stemming from economic interests (p. 357).

The Cunning of Unreason is divided into three parts and combines historical, empirical, and analytical approaches. The first part discusses the transformation of the classical political categories. Paraphrasing Benedetto Croce, Dunn asks: “What is dead and what is alive of Aristotle’s vision?” His conclusion leaves no room for doubt: “Whatever we may in the end choose to agree with Aristotle, we can hardly hope to see eye to eye with him” on many ideas (pp. 16–7). Aristotle’s conception is no longer viable because it demands that the city be “virtually self-sufficient” and makes women, slaves, and manual workers into pariahs. We live in a globalized economy and have a more comprehensive view of political inclusion. To be sure, Aristotle’s world was also global, albeit in a limited way. Greek city-states were engaged in a network of commercial relations with other Mediterranean populations and were familiar with imperialism. Aristotle conceived of “self-sufficiency” as political autonomy, the basic condition for public decisions. Perhaps this general criterion can still be useful, at least to evaluate the character of our democracies, whose scope of action, as Dunn notes, is on many occasions defined by a nonpolitical (thus, free from political control) domain such as the market.

Nonetheless, Dunn is right to trace modern politics back to the less noble and more instrumental view of politics that sprang from Christianity. Starting with the original sin meant to give politics both a coercive and a salvationist role, Christianity promoted both realistic and eschatological perspectives. Predictably, Dunn endorses the former: Hobbes not Marx. Hobbes, Dunn says, was right to treat cruelty and vainglory as “ordinary vices” reason can control (but not eradicate) by transforming them into the cornerstones of a secure social environment. Hobbes’ legacy is to have conceptualized the content of the kind of politics we deal with today: interests, instrumental rationality, and the individual potential for good and evil. These are the foundations of the main characteristics of modern politics: the juridification of state authority and the depersonalization of economic relations.

The second part of the book is a critical analysis of an empirical case of modern politics: the “structure of domination” of the Thatcher regime, which was a unique mix of “free economy” “protected by and in the last instance enforced by a ‘strong state’” and “belligerent chauvinism” (p. 144). What can possibly explain the longevity and popular support of a political program that was clearly not in the interests of that majority? As an Italian compelled to obey Silvio Berlusconi’s government, I sympathize instinctively with Dunn’s skepticism about the voters’ ability to make rational decisions. Yet, citizen “incompetence” is an artifact that begs other questions about how private interests control and manipulate information as well as about the role of ideology, rhetoric, and passion in politics.

In the third part of the book, Dunn outlines the theoretical bases of his reasonable hope. He singles out two possible explanations for the “unimpressive” performance of modern democracy: the Platonist (which “attributes the misdirection essentially to incomprehension” of both ordinary citizens and career politicians) and the sociological (which “attributes the performance of politicians, above all, to the cognitive facili-

ties and distortions which result from, and the more or less perverse incentives which are offered by, the institutional settings and social milieu”) (pp. 300–1). Dunn declares his preference for the latter and proposes a vision of democratic politics that is anti-Habermasian, realistic, and contextual in its value. This is a Shumpeterian view of politics, consistent with Dunn’s previous choice of Hobbes over Aristotle. Electoral competition is vulnerable to corruption, creates a “gap” between “enfranchised citizens and career politicians,” and suffers “the sheer difficulty of forming and implementing rational strategies” (p. 285). Yet, it is all we have if we are to make government democratic and “do the best we now can to modify the consequences of our action and inaction for the better” (p. 341).

Dunn endorses Benjamin Constant’s paradigm: The moderns have to cope with the constraints that private liberty imposes on participation. Representative democracy, like capitalism, is our only choice, but dropping the Enlightenment’s grand vision does not imply abandoning “the strategy of rectification” modern rationalism has perfected (p. 360). Modern politics is a complex concoction of “fatalism” and “voluntarism,” “routine” and “crisis,” realism and hope. It demands that reality be accepted, not glorified or demonized.

The Moral of the Story: Literature and Public Ethics. Edited by Henry T. Edmondson III. Lanham, MD: Lexington Books, 2000. 272p. \$75.00 cloth, \$24.95 paper.

Shakespeare’s Political Realism: The English History Plays. By Tim Spiekerman. Albany: State University of New York Press, 2001. 208p. \$16.95 paper.

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As their scholarship indicates, Henry Edmondson and Tim Spiekerman share two basic assumptions: There exist certain enduring issues of politics, such as the nature of social justice and the legitimacy of power, and authors of fiction, drama, and poetry who write with knowledge and sensitivity about the human condition often will have something significant to say about them.

Spiekerman searches the English history plays of Shakespeare, which involve King John, Richard II, Henry IV, and Henry V, for the Bard’s treatment of these enduring issues. In doing so, Spiekerman rejects historicist claims by influential contemporary literary critics that his search is implausible because the era in which Shakespeare wrote was so different from our own. Along the way Spiekerman offers some provocative insights. Among them is that Shakespeare does not completely accept or reject Machiavellianism. On the one hand, the author argues, Shakespeare shares Machiavelli’s position that the thirst for power is more central to politics than the desire for justice. He points out that none of the historical English kings treated by Shakespeare comes close to being a genuinely moral person. At the same time Spiekerman contends that, unlike Machiavelli, Shakespeare feels there are important moral aspects to politics as well. According to Spiekerman, the subjects of these historical monarchs exhibit a deeply held passion to be governed by morally superior rulers, even if such rulers are rarely found.

Less illuminating is the analogy chosen by Spiekerman to explain the relationship of subjects to rulers who claim that their power and legitimacy derive from divine right. He favorably compares that relationship to the mystical bond connecting children to their biological father. Spiekerman apparently wants readers to understand that the question of political legitimacy sometimes incorporates extrarational qualities. Yet, one can only wonder why he lends any credence whatsoever

to the paternalistic mentality that tyrants habitually exploit to sanction their oppressive regimes.

In his anthology of eighteen generally thoughtful essays, Edmondson describes Shakespeare's Henry V as a virtuous leader, a statesman, and the ideal Christian king. By Edmondson's estimation, it is during his famous soliloquy on the eve of Agincourt that Shakespeare most clearly portrays the depths of Henry's ethical character. In contrast, Spiekerman considers the soliloquy to be unremarkable ethically because it fails to include a moral justification for the invasion of France, which is a patriotic ruse to support his family's controversial right to rule.

Another essay in *The Moral of the Story*, by Alan Levine, investigates the novels of Chinua Achebe in an attempt to understand the personal and political consequences of moral idealism. In Achebe's *Things Fall Apart* (1958) and *No Longer at Ease* (1960), Levine discovers persuasive evidence that expecting persons, cultures, and governments to be too rational and moral can be just as destructive as a more cynical view. Politics has the potential for executing some important public functions, but it certainly is not a panacea for our ills, Levine interprets Achebe as saying. The most profound success we can achieve in combating ignorance, superstition, and injustice must come from education and from within ourselves, if it is to come at all.

Also in the Edmondson volume is an essay by Michael Platt, whose otherwise excellent treatment of prudential leadership in Joseph Conrad's "The Secret Sharer" employs an unfortunate illustration. Platt notes the crucial role of vision in prudence and argues that certain self-proclaimed freedom-loving Texas officials acted imprudently when they balked at providing local hospitality for injured Afghan Mujahadeem they had helped transport to Dallas hospitals in the early 1980s. Platt wonders why the officials hesitated to support those then engaged in the active struggle against Soviet totalitarianism. Yet, even if we recoil with Platt from their hypocrisy, is it not acutely shortsighted of the author to advocate unconditional acceptance of a group that twenty years later is guilty of providing financial and military support for international terrorism?

Despite such relatively minor difficulties, these volumes represent noteworthy additions to the growing literature on the relationship between art and politics. By depicting human beings as complex, three-dimensional figures, Spiekerman and Edmondson suggest, great artists can remind political scientists to avoid reductionism in their empirical research. By providing compelling examples of how abstract philosophical concepts influence the daily lives of citizens, the authors contend, great artists can enhance our understanding of political theory. In his contributions to the Edmondson anthology, Gregory R. Johnson thus consults the novels of Jane Austen and Flannery O'Connor in order to gain a deeper appreciation of Aristotle's ideal of magnanimity or greatness of soul.

The Next Religious Establishment: National Identity and Political Theology in Post-Protestant America. By Eldon J. Eisenach. New York: Rowman and Littlefield, 2000. 167 p. \$75.00 cloth, \$22.95 paper.

Peter Dennis Bathory, *Rutgers University*

Animated by an "American identity crisis," Eldon Eisenach seeks to comprehend a "deep disjunction in political culture and values between elite national institutions and the aggregate of local cultures and values." National elites, in particular, many in the "American university," offer America-in-crisis "a highly abstract democratic Universalism" which "denies American nationality." At the same time national

representatives of "local cultures and values" attempt to "ground national identity and national policy in a 'conservative Restoration' by bringing back to prominence and honor the value-sustaining institutions of civil society" (p. 99). Both "Universalists" and "Restorationists" fail in their respective tasks, and American identity, Eisenach argues, is threatened in the process. While "Universalism," often couched in the "neutral" language of "juridical democracy," is in some ways built upon the New Deal "liberal establishment," it has, he insists, not been able to articulate policies that speak to this "older, ruling coalition" and so poses "above the battle" (pp. 102-3). Restorationists suffer from another malady. Though winning elections, they have proved incapable of establishing "national authority" and so equally incapable of reestablishing an American national identity.

Eisenach joins the fray. Sharing with both Universalists and Restorationists a sense of impending disorder, he develops the case for a new "National Political Theology," another in a series which, he insists, has identified and dominated American political life from its beginnings. A new "Religious Establishment," however, requires something that neither of the alternative formulations he explores can provide. Universalists, seeking to be "free from nationalism, racism, sexism (and every other contingent boundary they happen to reject) that stands in the way of full equality" (p. 101), developed a rhetoric that was increasingly abstract "while the concrete interests and groups they in fact represented appeared increasingly fragmented, particularistic, and demanding." They fail to understand that their "standards of equality are historically articulated" and, in the process, prove incapable of developing the "narrative" of American life necessary to the establishment of a new American identity. Though much of this provided an easy target for conservative Restorationists, they also have failed, Eisenach argues, to take their complete case to the American people. Successful in implementing economic and tax policies, with their emphases on deregulation and cost benefit analysis, efficiency, and social cost measures, Restorationists have had far less success "in the areas of social values, intellectual culture and education" (p. 103). Insightfully, Eisenach reminds us that "economic conservatives, insofar as they adhere to neo-classical or libertarian values, do not share with moral conservatives an inner imperative to reform culture" (p. 104). But, the intramural conflict does not end here, as "some moral conservatives fear that the positive Restorationist projects led by the national government (e.g., national school standards, media censorship or control, the funding of religiously committed academics) require the active assistance of the federal bureaucracy and other national institutions dominated by their opponents" (p. 104).

For Eisenach, however, all is not lost. The moral vacuum left by these multiply-contending forces has produced "Third Way" alternatives, both "Liberal Nationalism" and "Pluralist Communitarian" in substance. From Richard Rorty and E. J. Dionne, Jr., on the Liberal Nationalist front to Charles Taylor and Michael Sandel among the pluralist communitarians, he finds the beginnings of a more satisfactory encounter with our contemporary dilemmas—an unwitting reconstruction of a national political theology. However, in the absence of "a common faith," even these alternatives will founder in contemporary America. Though he finds the bases of that faith, in a "common democratic faith" of Liberal Nationalists and Communitarians, Eisenach seeks to anchor that "faith" in the American political tradition, in Western political thought beyond America, and to recover in the process an American "experiential foundationalism" (p. 130).

Looking to the Progressive Era, Eisenach echoes Herbert Croly's "promise of American life" and its "national cohesion

... dependent . . . upon fidelity to democratic principle" (p. 8). While cautious not to elide intervening American history, Eisenach finds much to recommend in this era and its "pragmatism." Indeed, the writings of John Dewey serve not only to define an earlier "religious establishment" (pp. 89–92) but to mark boundaries of the new regime of "civility and citizenship" that Eisenach propounds in his conclusion (p. 148). Likewise, "the shared German or Hegelian or historicist spirit [that], he claims, pervaded the American university well before the Civil War" (p. 86) is revived in Eisenach's call for "a new American narrative" (p. 138). Closing with a plea for toleration, civility, and citizenship embedded in a new American political story, Eisenach's own story ends with a reshaping of "the legacy of American pragmatism into the beginnings of . . . a political theology for the next American establishment" (p. 149).

In its critical spirit, Eisenach's argument is reminiscent of Elstain's *Democracy on Trial* (1995), but his self-proclaimed "prophecy" places a faith in the American university past and present that is more in the spirit of Dewey. He trumpets "the success of the American university as a powerful and unified set of national institutions independent of both church and state" and explores the possibility that it "might be understood as the authentic national church of voluntary religious establishments" (p. 75). At the same time he understands the dangers of abstraction, distance, and arrogance: The antidemocratic temptations of the academy. Much as Tocqueville did, he attempts to comprehend the essence of politics as theory and its relation to political action. He knows, as Tocqueville taught us, that "the political sciences form a sort of intellectual atmosphere breathed by both governors and governed in society, and [that] both unwittingly derive from it the principles of their actions" (See Alexis de Tocqueville, 1852, "The Art and Science of Politics," *Encounter* XXXVI, 6 [January 1971]: 27–35).

Still, there is something puzzling about his reliance on American intellectuals and the university. He seems to understand with Tocqueville that the political sciences (Tocqueville's use of the term is closer to our understanding of political theory) are not always clearly understood, that the "intellectual atmosphere" may become polluted, leading to unintended consequences, even to the contradiction of stated goals. Tocqueville's warnings here about the intellectuals in politics may be more apposite than Dewey's principles, for though Tocqueville admired the pragmatic spirit of Americans, he knew first-hand of both the importance and the difficulties of integrating "political art" and political theory. "Authors," he thought, had been responsible for the self-consuming ideology of the French revolution. Democracy was fragile for Tocqueville, requiring attention to both theory and practice and the endlessly complex relation between them. Eisenach's turn to Dewey, Rorty, Taylor, and others suggests less than a full appreciation of the complexities of the very problem he addresses. It may be that more attention to Tocqueville on both France and America would have helped shape this argument.

Eisenach's reliance on the university, furthermore, confronts dangers beyond those of which Tocqueville could imagine. While our interest in "service learning," for example, seems to harken back to Dewey, our fascination with "distance learning," in particular, as a source of revenue, should give us pause. Within our discipline the spirit of "economism," alive and well despite the contrary signals of the "perestroika" exchanges, suggests yet another problem for Eisenach's "prophetic" message. The reestablishment of a "national political theology" may be responsive to his twin demons, but it is important to recognize that there are aspects of the "atmosphere" he hopes to freshen, quite apart from

Universalism and Restorationism, in need of careful attention as well. This said, Eisenach's jeremiad is both provocative and useful for our time and may well stir the fruitful controversy he admittedly seeks.

Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again. By Bent Flyvbjerg. Cambridge: Cambridge University Press, 2001. 201p. \$54.95 cloth, \$19.95 paper.

Stephen K. White, *University of Virginia*

The 2001 American Political Science Association meeting was alive with the Perestroika controversy. It has been more than a quarter-century since the profession last saw such ferment. If one compares the two episodes, there is strong similarity in the level of organizational activity; but when it comes to methodological issues, the differences are pronounced. The earlier period was characterized by a rich discussion; today there is comparatively little. And this relative silence has been the norm for a good number of years.

What explains this state of affairs? The decline of methodological talk may have reflected a general exhaustion or a dampening of enthusiasm for positivism and the strong behavioralist program of the 1960s. Many political scientists have been content in the last couple of decades to settle down quietly under labels such as postpositivism, postempiricism, and pragmatism, without much sustained analysis as to what such labels actually amount to. And political theorists, with few exceptions, have seemed uninterested in probing these questions.

Perhaps such quiescence is to be expected when there exists widespread consensus about the nature of political inquiry. But with the emergence of Perestroika, no one can pretend that this is the case. Perestroikans charge that certain methodological approaches have established a "hegemonic" position. If that is true, then what becomes most striking about the current state of political science is the stark contrast between the force of this expansionist strategy and the paucity of sustained methodological reflection bearing on its intellectual respectability.

What might we expect from a reinvigoration of methodological talk? One thing we shouldn't expect is any quick fix. Instant therapies were already making their appearance at the 2001 meeting. But such proposals are likely to be perceived either as new power grabs or as too amorphous to have much real purchase on our problems. An example of the former was David Laitin's conference paper that asserted categorically that we already have "impressive intellectual coherence" in our discipline; the real problem is institutional "anarchy" and "indiscipline" that just needs to be cleaned up ("The Political Science Discipline," p. 3). An example of the latter was Elinor Ostrom's well-intended proposal to gather all political inquiry under a big-tent description: the systematic study of "all human activities guided by norms" (Roundtable on "Shaking Things Up? Future Directions in Political Science"). Upon hearing this proposal, a person in the audience behind me mumbled, "I'll study bass fishing."

Given this situation, Bent Flyvbjerg's *Making Social Science Matter* is a welcome intervention. Flyvbjerg is a Danish social scientist who recently published an excellent case study of the city of Aalborg—essentially what used to be called a "community power" analysis—only this one persuasively incorporated a Foucaultian dimension to its understanding of power. The author's new book projects a general vision of how social science should reimagine itself. A key issue in this regard remains, of course, what one thinks about the unity of science thesis. My guess is that there are relatively

few political scientists today who believe that natural and social science are exactly alike; but many still think that some kind of progress on a naturalistic model is possible, and thus the profession should adhere to a clear-cut research program like rational choice. But, despite the many real insights and limited-range generalizations generated by that program, the fact remains that there is no general consensus that political science over the last quarter-century looks like an emerging science on the naturalistic model. Critics of such a research program increasingly find its adherents to be like die-hard Marxists: absolutely convinced the revolution will come and full of an unending supply of reasons why it has failed so far.

Flyvbjerg shares this familiar skepticism about the naturalistic model. A useful way to engage his arguments is to compare and contrast them with ones that were developed in the earlier period of ferment in political science. From this angle, one can see better both what is distinctive and persuasive in Flyvbjerg and what is more problematic. Like earlier critics, he contends that the naturalistic model does not adequately account for the irreducible centrality of "context and judgment" in human action (p. 4). But Flyvbjerg differs from many critics in his construal of the grounds for such centrality. The more familiar approach runs something like this: Because judgment and behavior in political life reflect actors' interpretations of common meanings embedded in particular contexts, and because these meanings and contexts cannot be grasped by abstract, universalistic models of behavior, we cannot expect such models to do very well in predicting political action. This line of argument is perhaps most closely associated today with Charles Taylor. Flyvbjerg finds this kind of hermeneutic account to be inadequate. The problem with this approach is that it amounts only to "a more complex form of determinism"; in effect, if you know the rules governing common meanings, you can determine which actions will occur. But, Flyvbjerg contends, this is surely wrong: "The rules for chess are not chess," and the rules governing common meanings are not a determinate map of expected action (p. 43). Flyvbjerg is, of course, right about this point. The problem is that he never indicates who actually ever believed that rules strictly determine the playing of a game. Wittgenstein certainly didn't; and Taylor doesn't either. Strangely, Flyvbjerg does not include any reference whatsoever to Taylor.

Flyvbjerg contends that we must turn to the work of Hubert Dreyfus as our main resource for showing why action is generally too open-ended to be successfully modeled. Dreyfus's research has shown that the knowledge and skill of a competent actor—whether a paramedic or a politician—is too intuitive, holistic, and context-dependent to be "externalized into rules and explanations" (p. 21). Flyvbjerg's discussion of Dreyfus is generally quite good in drawing out the latter's significance for social scientists. But I do have some reservations. It strikes me as unwarranted to make the case for a nonnaturalistic model depend primarily on Dreyfus's particular theory of human skill and judgment. Given Flyvbjerg's denigration of the hermeneutic account, he forces us to place all our bets on Dreyfus. I would rather reverse the weighting, with the heaviest bets put on a (correctly understood) Taylor-like account that can be combined (easily, I think) with Dreyfus-like insights. Moreover, we would do well to remember that Dreyfus's work actually fits into an existing tradition of reflection on the character of political inquiry. In his much discussed essay of 1969, Sheldon Wolin characterized political insight as involving "tacit political knowledge" ("The Vocation of Political Theory," *American Political Science Review* 63:4 [December 1969]: 1070–4).

Flyvbjerg's heavy reliance on Dreyfus is related to the Aristotelian orientation he envisions for social science. Aristotle's *phronesis*, with its focus on the quality of judgment and

practical wisdom exhibited by political actors, is the key concept. Flyvbjerg aims to "restore social science to its classical position as a practical, intellectual activity aimed at clarifying the problems, risks and possibilities" that are faced by "the communities in which we live" (pp. 4, 166). To his credit, Flyvbjerg is quite concrete in spelling out methodologically what this vision implies. Thus, for example, he includes a chapter on the importance of case studies (which tend to be denigrated somewhat within the naturalistic model) and a chapter elucidating his own research on Aalborg. Flyvbjerg's "phronetic social science" does make one sharp departure from Aristotle, in that it self-consciously attends to how conflict and power are constitutive of political life and inquiry (pp. 3, 129). Although much of what Flyvbjerg says about Foucault on this theme is persuasive, some familiar questions about the latter's perspective on power in general, as well as its relation to domination, remain unanswered. For example, Flyvbjerg unpacks Foucault's notion of power as "a multiplicity of force relations" and "the strategies in which the force relations obtain effects" (pp. 116, 120). But what, then, are "force relations"? Similarly, one thing a contemporary phronetic science is supposed to do is unmask relations of domination; but the normative criteria for picking out instances of domination remain unclear (p. 104). Flyvbjerg, like Foucault, wants to avoid using universalistic normative frames for addressing this issue. He contends that we need only appeal to the normative frames of "the communities in which we live." Without further explanation, however, this would seem to move us toward a position like Richard Rorty's, wherein political inquiries aim at clarifying the implications of authoritative traditions (p. 140). Flyvbjerg seems unaware of the potential tension here: Rorty castigates inquiry that busies itself with just the sort of systematic unmasking operations a phronetic social science undertakes.

Although I have difficulties with some of Flyvbjerg's claims, the book does an excellent job of bringing a whole range of timely methodological issues onto the table and of projecting a thoughtful vision of social science that runs counter to the hegemonic one. Proponents of the latter owe the discipline careful responses to proposals such as Flyvbjerg's. Failure to do so merely feeds the growing suspicion that what is at issue is not a justifiable hegemony but, rather, simply a coercive occupation of intellectual terrain.

The Vocations of Political Theory. Edited by Jason A. Frank and John Tambornino. Minneapolis: University of Minnesota Press, 2000. 400p. \$57.95 cloth, \$22.95 paper.

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A generation ago, Sheldon Wolin evoked an image of the vocation of political theory as an alternative to the behavioral program of theory and scientific inquiry that had come to dominate political science. His call also summoned those who believed that, in the midst of the political turmoil of the 1960s, the mainstream of the discipline had become politically quiescent and, at least by its inaction, even implicated in the political crises of the time. Intellectual and ideological choices were, indeed, involved, but Wolin was implicitly also giving voice to a professional identity for a large segment of the academic subfield of political theory that had been evolving for at least three decades. His articulation of the vocation was, however, as mythical as the method of science to which much of political science had subscribed, and these hegemonic legitimating myths ultimately could neither withstand critical scrutiny nor suppress the latent differences within each.

The editors of this volume ask what has become of this vocation, and they are sometimes ambivalent about whether

they are referring to Wolin's epic image or to the character and condition of political theory as a professional university activity. Whatever their assumptions about the relationship between the two, their principal focus is on the latter. Although the editors state that the concerns "animating" the volume are "the character and status of contemporary political theory, its place in the academy and its role in public life," (p. x) these matters are not explored very fully, either analytically or empirically, and the essays have difficulty staying on message.

With respect to the issue of the relationship between political theory and political science, the editors characterize their approach as "primarily theoretical and metatheoretical." (p. xxv) They claim that political theory should be relevant, but they point to the value of detachment and "question the assumption that political theory should avoid straying from direct engagement with current events." (p. xv) They stress the existence of "vocations" of political theory as well as the pluralistic and changing character of the field, which they believe warrants yet another assessment of its current state. This pluralism is attributed in part to the nature of the field but particularly to the contemporary diversity and flux within the domain of politics and to the proliferation of the political. Although the editors warily celebrate how eclecticism has enlivened political theory and contributed to its flourishing, they worry about dissolution of identity and about "our ability to speak of a vocation at all" (p. xiii–xiv). As with most credos of pluralism, there is an attempt to indicate some sense of unity and continuity, but they claim little more than an unspecified "family resemblance" among the many modes of theorizing.

The lead essay is by Wolin and focuses on this loss of identity. It also notes that the conference from which this volume derived was devoted to future political theorists and to the future of political theory and was a response to the "perennial uncertainty and controversy about political theory's relationship to political and social science, to philosophy, to history, as well as its relationship, if any, to the 'real' political world." The essay, which Wolin labels an "invocation" or "a response to a certain kind of loss," is not optimistic. (p. 5) He claims there is a danger that the vocation is about either "to lapse into dilettantism" or "to harden into professionalism" (p. 618). He viewed his original essay as partly a response to a political "crisis" and to a condition of undertheorization and conformism, whereas we now face a situation marked by "overtheorization."

According to Wolin, in a world in which "both theory and politics are ubiquitous and indeterminate" and "freed of the constraints of an overarching political," we have a "politics of multiplicity" and a concomitant "proliferation of theory" and diverse identities for "disjointed theorists" (pp. 9,11). As a consequence, political theory makes reference to real-world controversies, but "its engagement is with the conditions, or the politics, of the theoretical that it seeks to settle rather than with the political that is being contested. . . . It is postpolitical" (p. 15). Although the situation seems to be a classic case of overproduction and underconsumption, Wolin attributes these problems, as he has in earlier essays, less to the faults and failings of theorists than to the chaos and speed of modern life, to a "society without a paradigm," to a "utopian" era that paradoxically depends on the "perpetuation of dystopia" for the many. Can an invocation of the vocation of political theory aid us? Wolin suggests, "besides being fatuous, that call may be too late in the day." In the space and time between his two essays, "the academic intellectual has undergone a dizzying series of intellectual permutations." Varieties of political theory have "replicated the pace of technological change" and become a segment of the "brainy classes," who ultimately

perpetuate and benefit from the present and undemocratic condition of society (p. 21).

Wolin's image of the vocation was the theme on which the conference pivoted, but the other twelve essays—half by establishment scholars and half by ABDs and recent Ph.Ds—seldom directly engage his account. There is much discussion of theory, but it is often very abstract and reflects the very diversity of issues and perspectives that the editors and Wolin consider problematical.

Wendy Brown discusses Derrida and Benjamin while addressing the relationship between political thinking and forms of historical consciousness. J. Peter Euben, picking up on the more general dimension of Wolin's essay, speaks about the relationship between political theory and the sense of loss. Thomas Dumm explores the significance of loss in a more private or ordinary sense. David Mandell considers the relevance of the study of the history of political theory for thinking about contemporary issues. Linda Zerilli criticizes, from a Wittgensteinian perspective, the craving of feminist thought for generality and its neglect of the ordinary. Mark Brown examines the often contradictory ways in which political theorists have viewed natural science. Lon Troyer urges an ethos of political theory based less on any particular approach than on an attitude of provocation. Shane Gunster argues that political theory must aim at achieving the status of Gramscian "organic intellectuals." Samantha Frost discusses Hobbes as a vehicle for engaging contemporary issues regarding the embodied subject. Jill Locke attempts to bring political theory to bear on the public policy issue of using chain gangs. William Connolly seeks to extract democratic insight from the work of Nietzsche.

These are all lucid and credible essays, but the volume as a whole demonstrates that political theorists live lives of persistent and sometimes desperate cognitive dissonance as they seek to relate to both political science and politics and to come to grips with the disparity between their actual professional position and extravagant images of their vocation.

Public Reason: Mediated Authority in the Liberal State. By Fred M. Frohock. Ithaca, NY: Cornell University Press, 1999. 234p. \$35.00.

Albert W. Dzur, *Western Michigan University*

The hope of ordering collective purposes through rational speech stretches back to the earliest historical attempts at theorizing political practice. Public reason has been a particularly prominent theme in political theory for the last decade and is central to discussions of deliberative democracy. Recent work on public reason is motivated by the belief that contemporary political procedures lack adequate norms and spaces for public engagement in intelligent, informative civic debate sensitive to deep differences in values. Fred Frohock's thought-provoking book covers much familiar ground, yet it also contributes a number of key ideas and a dose of realism to a research program now in a stage of self-criticism.

John Rawls's *Political Liberalism* (1993) is in the background throughout this book as the best available theory of the liberal state and as the natural starting point for any critique and reconstruction of contemporary liberalism. Like Rawls, Frohock believes that liberal states must face up to the demands of pluralism and accommodate, in their "governing language," the multiple interpretations of the good that thrive in liberal societies. Unlike Rawls, Frohock rejects procedural solutions to the problem of pluralism. "Merit" forms of public reason that attempt to pare away from deliberation everything but what can be considered the "merit" of the particular political issue under debate fail because political

disputes under conditions of pluralism are indeterminant. Some disputes, such as those between prochoice and pro-life in the abortion controversy, stem from different ways of legitimately interpreting key terms such as "person," "life," and "harm." Even advocates on both sides who wish to reach an agreement and who share the same moral and political language differ irreconcilably on their interpretations of these terms because of their different life experiences. Deliberative breakdowns are even more dramatic in cases where advocates do not even share the same conceptual language, as in the dispute between health-care professionals committed to biomedical models of thought and Christian Scientists who reject that model in favor of religious healing. Neither debate will be resolved by carefully sifting evidence and weighing the merits of competing arguments.

One of Frohock's targets is Rawls's attempt to distinguish, for the purposes of state action, comprehensive moral arguments from political claims. Like Gutmann and Thompson, whose very closely related work, *Democracy and Disagreement* (1996), he unfortunately does not engage, Frohock believes that moral disagreement is a legitimate feature of politics. In the abortion and religious healing cases there is no feasible way to distinguish the moral from the political, so suffused are the political issues in the moral experience of the disputants. Frohock is right to remark, "It is hard to see where the moral and political can be demarcated as these matters enter public space. Bracketing moral beliefs in these cases seems to remove the political dimensions also" (pp. 58–9). And he is right to point out the historically close connections between the languages of law, politics, and morality. It is probably necessary to admit, as Frohock encourages, that the state is "clearly a kind of moral undertaking" (p. 57). Harder to reach, at least without more argument, is Frohock's conclusion that when the state steps in to mandate lifesaving medical treatment for a child of Christian Scientist parents, it is rejecting the idea that prayer is a form of healing, and that when it permits the parents to decline such treatment the state is endorsing prayer as healing. Because officials and citizens in liberal states recognize the complicated trade-offs in cases like these, policy settlements simply do not place the state in one moral corner or another.

Under what conditions, then, can we expect legitimate resolution, if only temporary, for the long-term moral disagreements that mark our sharpest political debates? Frohock believes that closer attention to the narratives that shape individual lives is part of the answer. "Narrative conceptions of the self can also provide stories that summon common human experiences, and these stories may mediate the strong pluralism of liberal settings" (p. 100). Stories help unify selves and link people together through overarching themes. Frohock points out that the "great narratives of political experience can forge unity in describing and identifying the roles of persons in a universal political culture," even though they often "relax literal truth in favor of metaphor, symbols of various types, myths even" (p. 105). This emphasis on narratives, and how they integrate the social and the highly personal, is an important addition to the literature on deliberative democracy, which tends to overemphasize the power of rational discourse in forging agreement between persons with divergent interests or values. Frohock leaves open many important questions about how, exactly, narratives challenge our "understandings of evidence, inference and public space" (p. 106). The ideal of public reason seeks the "peculiar merger of self and process" found in a good conversation and hopes to absorb individuals "into the talk that uses and crafts collective arrangements" (p. 113). This formulation is suggestive and warrants closer analysis of the constraints on and the risks of such an ideal under conditions of normal politics, where some narratives have

been historically marginalized and others given unchosen and unreflective dominance.

Another solution to the problem of moral disagreement is mediation: "Connections among persons and outcomes in deliberative procedures often require third parties, figures who are to some degree outside of the relationships among persons seeking a collective outcome but are vital to a successful outcome" (p. 203). Mediators can "move the conversation to a deeper inquiry" that may motivate personal changes and help resolve disputes (p. 203). Mediation, therefore, can be "a powerful source of collective experience" (p. 204). The ideal of context-oriented, nonadversarial, minimally hierarchical mediation is the book's strongest positive contribution to research on public deliberation, which has much to learn from the role mediators play in actual contexts of conflict resolution. Frohock exemplifies mediation with the bioethics deliberations of hospital ethics committees, which seek to resolve disputes over treatment plans between patients and health-care workers. This well-chosen example shows how complex mediation is, however, and how vulnerable to the institutional context in which it is embedded. Frohock notes that bioethics mediation is limited by the traditional purposes of medicine—keeping people alive—and by legal constraints, but he pays less attention to the fact that this mediation is also limited by an institutional context that privileges the biomedical model above other "narratives" and grants the physician far more discretionary power than any other participant in mediation. Further, Frohock might have gone farther in discussing the great demands placed on mediators. In the case of medicine the bioethics consultant must be familiar with the language of medicine and the pluralistic languages of patients, must be attentive to the highly personal elements of patient care as well as their social dimensions, and must be aware of legal constraints on patient and physician decision. Such demands are not unique to mediators in medicine. Very similar demands are faced by mediators in the criminal justice domain, such as those who work in victim–offender mediation. Such complexity suggests that mediation may best be seen not as an individual role, as Frohock implies, but rather as something that requires a good deal of structural support.

A methodological merit of this work is its attention to ground-level cases of moral disagreement. Especially attractive is the use of examples drawn from the medical domain, with which Frohock is exceptionally well acquainted. This domain has had a flourishing normative discourse for more than three decades. Political theorists can gain much from paying more attention to the role of ethics consultation and the many political aspects of bioethics discourse in hospitals, clinics, and wider public arenas. Methodological merits and suggestive discussions of narrative and mediation aside, however, the book is difficult to recommend to someone with only a general interest in political theory because Frohock begins *in media res* with intricate discussions that demand a strong background in contemporary debates over liberalism. The book offers more to those with particular interests in liberalism and in public deliberation, though the latter audience will miss the wide and deep engagement with both critics and advocates of deliberative democracy that is needed at this stage of the research program.

The Enemy with a Thousand Faces: The Tradition of the Other in Western Political Thought and History. By Vilho Harle. Westport, CT: Praeger, 2000. 232p. \$59.95.

David Boucher, *Cardiff University*

This book is part of a much larger collaborative project devoted to "Otherness, Identity, and Politics." It explores an

aspect of identity theory, about which the author makes two uncontentious claims: first, that identity is socially and politically constituted and, second, that identity politics predate 1989. By delimiting a theme in Western political thought and history that constructs the “I” and the “thou” in terms of good and evil, the book identifies and delimits a tendency to portray the Other as an enemy, evil incarnate, and dehumanized by a combination of religious and political ideas. The tradition of understanding the Self and the Other as the vehicles of good and evil is reproduced in thought, speech, and action and constitutes a continuous tradition from ancient Iranian Zoroastrianism, through Judaism, Christianity, and Islam.

The book is a work of synthesis, constructed largely from a wide variety of secondary sources, with little evidence of a familiarity with the primary works, with the notable exception of the chapter on Carl Schmitt. The assumption throughout is that we know more about a particular thought or action by pointing out that similar thoughts and actions happen elsewhere. It is a construction based on resemblances at the level of conclusions, without any serious attempt, and deliberately excludes any serious attempt, to explore the reasons for arriving at the conclusions, in order to avoid the “mere description of the unique case” (p. 10). It is interesting, for example, to know that Cicero did not justify war, especially on religious grounds, unlike ancient Hebrew thought and Zoroastrianism. Politics, and not war, was central in his idea of national identity. Why Cicero held these views is left unexplored, and unless you already know, it remains a mystery.

As the author presents it, he has constituted a tradition based upon coincidences, rather than substantive connections. The continuity of the tradition is not demonstrated, but assumed from the soundings taken in different historical periods, considerable distances apart in both space and time. The Middle Ages gets particularly cursory treatment on the eccentric and idiosyncratic grounds that it is an elusive period between the ancient and the contemporary worlds, stretching from the early Christians to Edmund Burke, as the first “modern” representative of the tradition. There is a great body of literature on what constitutes a tradition, and the criteria that have to be met before one can talk in terms of authors belonging to such traditions (see John Gunnell, *Political Theory: Tradition and Interpretation*, 1979; C. Condren, *The Status and Appraisal of Classic Texts*, 1985). For Harle, the existence of a tradition is unproblematic. A resemblance between the thought of one period and that of another, or the thought in one geographical location and that in another, is sufficient evidence of the “use” or “application” (p. 53) of the tradition, or of the influence of one set of ideas upon another. Is, for example, self-consciousness, or self-awareness, a criterion of belonging to a tradition? If so, authors have to be shown to be, at least in principle, conscious of their use of the vocabulary and concepts. Even in making such connections, to follow Ricoeur, are we doing no more than constructing a quasi-world of texts, distanced from their authors and the contexts in which they were produced? Indeed, Harle deliberately distances the ideas he reiterates by presenting them at a high level of generality, in order to highlight their similarities. Take just one example as illustrative of what is lost in dealing with generalities abstracted from their contexts. The author suggests that the rhetoric surrounding the conquest of the American Indians “reproduces the tradition” he is discussing (p. 72). What is important for Harle is the dehumanisation of the Indians, the representation of the Other as alien and inferior. What is of significance, however, is how a complete adjustment of the European cosmology had to be changed as a result of the discovery of the Americas, and how it generated modern theories of property, such as those of Grotius and Locke, which justified the appropriation of land in the

Americas. First, the discovery of a fourth race of people was contrary to the teachings of the Bible, which taught that Africans, Asians, and Europeans were the descendants of the three sons of Noah. The question of whether the Indians were human was a real theological dilemma. Given that their lands were being conquered, to constitute a legitimate claim to appropriation, and to have that claim respected in international law, or the law of nation, the question of land ownership became paramount. On this question it was claimed that Indians were not human, or that they were children, heathen, or insane, all of which would disqualify them from land ownership. The Spanish theologian Vitoria denied all of these grounds for the appropriation of Indian land but allowed appropriation on the grounds of just war. Practically, this had the same effect as allowing appropriation on the other grounds. To prevent free passage through one’s land was a violation of the law of nature and constituted an act of war, which allows combatants to seize lands. It is true, then, that the Indians as Other were constituted as the enemy, but we could hardly claim to have understood why without knowing that it was in order to justify the appropriation of their lands.

Harle’s book is not without illuminating insights, and interesting pieces of information that give an indication of different ways of thinking from the West, such as that of Islam. At a time when *Jihad* has become so thoroughly part of the *lingua franca* of the age, it is important to be reminded that it does not simply mean holy war. The great *Jihad* in Islam represents the religious quest for righteousness and goodness in all aspects of life. The small *Jihad* is the duty to defend Islam with weapons and is related to the idea of just war. War between Islamic countries (*harb*) is regulated by strict ethical considerations, whereas between an Islamic state and a nonbeliever state (*razzia*) ethical rules are suspended and any methods for defeating the enemy are permissible.

As an interesting catalog of ideas on the ways in which evil is represented in a variety of cultures, as manifest in the enemy, or Other, the book serves a useful purpose and is a valuable resource. It is, however, methodologically and theoretically naïve.

Natural Law Theories in the Early Enlightenment. By T. J. Hochstrasser. Cambridge: Cambridge University Press, 2000. 246p. \$54.95.

Michael J. Seidler, *Western Kentucky University*

This detailed historical study focuses on Protestant natural law theories in the early German Enlightenment (explicitly excluding the French and British sectors) and traces their influence, or fate, through Kant. Despite its title, it is more than a specialist tome devoted to an historically isolable development, and it is not merely a subsidiary, underlaborer’s attempt to recount the prehistory of Kant’s achievement. Rather, by tracing several important background currents through the period concerned, Hochstrasser illuminates the odd historical fact that German enlighteners at the end of this span knew or thought so little of those at its beginning. The central topics are eclecticism; the so-called “histories of morality” that were part of its self-conscious legitimization method; the rationalism-voluntarism split in early modern natural law; and the associated distinction among moral philosophy (ethics), natural (positive) law, and international law (*ius gentium*) that developed out of these debates.

Some of these topics have been treated before, but others remain nearly unknown, at least in the Anglo-American realm. There is a growing literature on so-called early modern natural law theory, but eclecticism as a philosophical method has received little attention in English. Moreover, the many

histories of morality that appeared (mainly in Latin) during the late Seventeenth and early Eighteenth centuries are even less familiar, both individually and as a pervasive intellectual phenomenon *sui generis*. Hochstrasser is the first to tackle this body of work and to expound its historical and philosophical significance.

The book contains six chapters whose rich detail might have been better reflected in an expanded table of contents. Chapter 1 establishes the connections among the main themes: natural law, eclecticism, history and theory, and the influence of debates about these topics on university reform and the politics of enlightened absolutism. It ends with an appendix listing the main “histories of morality” consulted. Chapter 2 examines Pufendorf’s intellectual method and intentions, particularly his *Specimen controversiarum* (1678), which was the first and, indeed, paradigmatic instance of moral historiography in the service of philosophical argument. The third chapter compares Leibniz and Pufendorf, not merely their famous disagreement about the nature of obligation but also the underlying moral epistemologies that determined their respective intellectualist and voluntarist approaches.

In chapter 4, Hochstrasser details the metamorphosis of Pufendorfian jurisprudence in Christian Thomasius and his school, including the formal commitment to eclecticism as a self-critical intellectual method most suited for practical reforms. Chapter 5 shows how Wolff and his followers (including Vattel) transmuted voluntarist natural law into rationalist, metaphysical forms directly opposed by its founders. It also describes how the melding of rationalist and voluntarist traditions in Brucker’s influential history of philosophy began, already, to obscure the important role of Pufendorf and Thomasius. The final chapter traces eclecticism into the *Popularphilosophie* of Meiners and Garve, explores Kant’s understanding of the history of philosophy, and clarifies the inhospitality of pure reason to natural law as such. Moreover, it iterates the continuing linkage between these abstract discussions and more worldly contestation over academic disciplinary status and, indirectly, social policy. The chapter ends with a look at the Kantianized histories of moral philosophy by Stäudlin, Buhle, and Tennemann, in which natural lawyers and eclectics are barely noted and, ironically, their work is mistaken for the neoscholastic, quasitheological orthodoxies that they opposed.

The sheer range of this book and its ability to fold such a variety of intellectual developments into a coherent account are its chief recommendation. Several features deserve specific mention as notable contributions to the scholarly literature. One is Hochstrasser’s careful analysis of the so-called Boineburg correspondence. This important epistolary exchange in the early 1660s—among the German constitutional lawyer Hermann Conring, Baron von Boineburg (Leibniz’s patron at Mainz), the Grotian expositor J. H. Böcler, and Pufendorf—resulted in the latter’s abandonment of a formal, quasimathematical method for one argumentatively more engaged with historical predecessors. There is nothing as detailed and informative in the English literature, and Hochstrasser’s treatment sets a high standard for such background studies, which are so necessary for understanding the evolution of ideas.

Another feature is Hochstrasser’s excellent articulation of Pufendorf’s sociality principle in terms of his empirical account of language acquisition and the associated development of human rationality through a socialized *cultura animi*. This, too, is an unusual and powerful focus that shows Pufendorf to be about twenty years ahead of Locke (in his opposition to innatism and his analysis of complex, abstract ideas), and it rightly exhibits the contingency at the heart of Pufendorfian natural law that so irritated his theological,

rationalist, and (later) even transcendental opponents (i.e., Kant), who were all absolutists in their own way. By noting the dependence of (even) language and rationality upon a kind of double contract (consistency, veracity) similar to that grounding the state (association, submission), it exhibits well the experience-based conventionalism, or controlled voluntarism, that elaborates the meaning of sociality at different levels of Pufendorf’s natural law framework.

Some of Hochstrasser’s concepts and distinctions still need sharpening. The notion of “imposition” in Alberti and Pufendorf, and in the divine and human instances (even among humans), is not quite the same. The claimed divergence of ethics from natural law and international law—a result of these debates—also depends on particular interpretive commitments whose potential divergence is not always noted. Even the central notion of voluntarism seems philosophically underdetermined at times, as Hochstrasser moves from the voluntarism “excluded” (p. 104) from Pufendorf’s moral epistemology to Kant’s “frontal assault” (p. 198) on the voluntarist moral histories in which Pufendorf was central. Ironically, this problem arises because Hochstrasser, unlike the histories he canvasses, may be too historical and insufficiently philosophical. That is, he sometimes takes thinkers or ideas too much on their own or their respective expositors’ terms. Yet, without a stronger interpretative or philosophical preference, also reflected in a consistent terminology, the narrative gets redefined too often, and the reader winds up conceptually stranded, without a general vantage point.

The problem is occasional, however, and does not undermine the intelligibility brought by this excellent study to an historical period still lacking in plausible panoptic perspectives. The work rests on a wide and deep scholarship versant with the primary and secondary literature in many languages. It is well written and carefully produced, and it will become an important tool in the field.

Encountering Tragedy: Rousseau and the Project of Democratic Order. By Steven Johnston. Ithaca, NY: Cornell University Press, 1999. 189p. \$29.95.

Lori Jo Marso, *Union College*

Rousseau is known as both exalted democrat and father of totalitarianism. Steven Johnston’s book demonstrates that the two interpretations cannot be separated. Johnston evaluates Rousseau’s texts hoping that the “costs of Rousseau’s virtuous republic, many of them hidden or muted, can be reckoned alongside its accomplishments” (p. x.). Not surprisingly, Rousseau’s accomplishments are called into question. This elegantly written and forcefully argued book claims that order is the essential ingredient in Rousseau’s recipe for democracy. Developing the argument via Nietzsche’s tragic perspective, Johnston wonders aloud whether all attempts at democracy will collapse under the weight of their internal contradictions.

Resisting the urge to simply “upbraid Rousseau,” Johnston promises to “rethink and radicalize certain aspects of his thought” (p. 3). His focus highlights tragic elements inherent in Rousseau’s theorization of the state of nature, the politics of the founding, the role of government, and the existence of enmity. Describing the “phantasmic life” tragedy enjoys in the “interstices” of Rousseau’s work, Johnston claims that tragedy manifests itself at “untimely moments” (p. ix). Marginalization ensues from promises of freedom, domination springs from equality, difference rears its ugly head in the quest for identity, arbitrariness haunts legitimacy, cruelty accompanies virtue, and on these dilemmas continue (p. ix). To characterize this sequence as loss, failure, or unfulfilled promise, however, misses Johnston’s point. He argues that tragedy is “inescapable, inevitable, inexorable, and—what is

more—commonplace, even banal. . . . Tragedy lies in wait, an inevitable concomitant to human exertions and projects” (p. 9). If the tragic is all too human, as it were, Rousseau could not do otherwise than to manifest tragic elements in his thought. How, then, does Johnston, radicalize Rousseau? Can he, as he claims, “enlist Rousseau as an ally against himself?” (p. 3).

Johnston’s chapter “On Government” aptly illustrates his method. Johnston does well to employ *Letter to D’Alembert*, *Political Economy*, *Considerations on the Government of Poland*, and *Constitutional Project for Corsica* to “think about the political dicta of *The Social Contract* in terms of the principles and practices of governmentality that permeate these works” (p. 76). Here he moves from Rousseau’s specific examples to consider again his general theoretical exposition. Johnston claims that this approach will “tame the seductive rhetorical powers of *The Social Contract*” (p. 77). Indeed it does. The way Johnston constructs his argument, with help from a 1978 lecture by Foucault titled “Governmentality,” one might think that Rousseau’s dictates on sovereignty and the art and role of government were written expressly to illustrate the dangers governing oneself poses in this newly exposed Foucaultian universe. As Johnston puts it, “Rousseauian practices of government presuppose and engender relations of reciprocal subjugation more than autonomous determination” (p. 89).

How could this happen? Isn’t government, in Rousseau’s formulation, intrinsically dangerous, mandated to operate merely at the behest and in the service of sovereignty? Were we to read only *The Social Contract*, we might conclude that Rousseau recognizes the dangers of government and the constant power struggle between the forces of government and sovereignty. Yet even within these pages Rousseau hints at an “algebra of order” in claiming that the “modern age, with its humongous states, does not lend itself to the pursuit of republicanism” (p. 83). The government must be “relatively stronger in proportion as the people is more numerous” (*The Social Contract*, p. 80; cited by Johnston, p. 84). Political activity in such large states fails to mold citizen behavior and government steps in to fill the void. Johnston’s attention to the “specific, the contemporary, the local, the empirical” (p. 100), as Rousseau explores in the essays on Poland and Corsica and *Letter to D’Alembert*, reveals that the citizen is not formed through the political activity of enacting and engaging in sovereignty. Instead, the citizen is produced “through disciplinary processes centering on the regulation of bodies in public space” (p. 98). Military discipline takes the lead in *Considerations on the Government of Poland*; in the essay on Corsica, citizens are created through service in the corvée; *Letter to D’Alembert* illustrates the “panoptical architecture” of the marriage festival (p. 103). “Here the subject-citizen is disclosed as the contrivance of power—an artifice to be constructed more than an essence to be realized. . . . The art of government rather than the exercise of sovereignty is responsible for its manufacture” (p. 87).

Johnston’s argument that Rousseau accomplishes what he appears to least desire is convincing. In the chapter “On Enmity” we discover that the enemy is always within; the “republic’s enemies seem to be proliferating at an alarming rate” (p. 139). Thus while Rousseau rightfully fears the encroaching arm of government, he reproduces its tactics in creating and encouraging the art of government to form his citizens. Though he desires a virtuous republic, the battle each citizen must fight within himself (against individual will), against women (effeminacy), and against all signifiers of difference yields an uncompromising totality where freedom requires total submission.

But must the analysis end here? Johnston hints that a “full-bodied democratic politics” may be “newly capable of finesse-

ing” the “tragedies of social and political life” (p. 24). Yet the reader is never offered as much as a nod in the direction toward creating this new politics. In fact, we are told the contrary: Any political community is “an arbitrary, contingent artifact;” we are not meant “to live one way rather than another, ruled by this set of values rather than that one” (p. 5). We have learned here that violence, cruelty, and enmity are at the heart of democratic politics. How, then, and why might we be moved to take any political action whatsoever? The intellectual exercise performed by Johnston is a treat to read. Yet this reader wonders where we go from here politically. Rousseau, above all else, insists that we demand attention and commitment to the ways in which a more democratic politics might be theorized and the spaces in which it might be enacted. Despite the valuable insights into Rousseau that Johnston reveals, the limitations of this form of theorizing also lie exposed. A *Social Contract* completely purged of its seductive power doesn’t leave us any space for imagining and enacting new forms of community.

The Loss of Happiness in Market Democracies. By Robert E. Lane. New Haven, CT: Yale University Press, 2000. 466p. \$42.00 cloth, \$19.00 paper.

Conrad P. Waligorski, *University of Arkansas*

Happiness, unhappiness, and depression are not the usual foci of political science or economics, but Robert Lane demonstrates their importance. *The Loss of Happiness in Market Democracies* is a worthy companion to and extension of Lane’s earlier work, especially *The Market Experience* (1991). Lane employs psychology, genetics, evolutionary theory, and medical research to convince economists and democratic theorists that biological and psychological research can enrich their often unrealistic assumptions about well-being and behavior. He argues that in affluent societies there is growing unhappiness, growing depression, and declining marginal utility of income to produce happiness. These are accompanied by mistrust and increasing political negativity, which further undermine happiness. The dominant Western image of individualism ignores that people often do not know what makes them happy, which undermines prevailing market and democratic premises (p. 284).

What has caused this? Lane’s discussion of genetics and evolved needs is speculative but plausible, in part because the author is careful to ascribe no more than 40% to 60% of behavior to genetics. He emphasizes instead the mutual influence of endowment and environment. “We have violated evolutionary instructions and we have devalued our children” (p. 37). Affluent, market-democratic humanity has subordinated the need for companionship to acquisition, and the result is insufficient and declining companionship and a “social, occupational, and internally generated” (p. 47) stress response with which we cannot cope. Money and economic growth cannot buy happiness, are not substitutes for intimacy, and do not relieve loneliness. In the process, we have subjected children to increased depression and unhappiness.

Lane effectively argues that neither an ever more compulsive pursuit of market rewards nor, unfortunately, what he calls market (parliamentary) democracy provide an answer. Rather, as presently constituted, they exacerbate our difficulties. Markets make people unhappy (p. 139, pp. 159–92). Democracy is difficult and often painful (pp. 220–45), although some of its pains are true of all political systems, and it does not provide effective self-determination or the close companionship needed to deal with unhappiness and depression. Beyond the [ever shifting?] poverty level, satisfaction with one’s family life and interpersonal relations contributes more to subjective well-being than added income,

the market, or democracy. Democracy, however, is still relevant to individuals. It can support the behavior that generates companionship, intimacy, and a sense of control over one's life. In itself, this is a great deal.

Lane's "purpose . . . is to diagnose the problem and not to offer remedies" (p. 335), but his many suggestions lead one to hope that his next book will examine remedies. Lane believes that the "decline in happiness . . . is likely to be ephemeral" (p. 321). Several times he analyzes "Companionship or Income" and felicity versus income (pp. 77, 125), arguing people must trade some income to create conditions conducive to happiness, but he does not discuss the institutional and ideological changes necessary to support this trade-off. His goal is a "companionable society" (p. 332) in which people realize that there are multiple goods, all of which contribute to democracy and happiness. Democratic government cannot create companionship but can do other things that support such a society, including the creation of "lifeframes" that sustain the "microworlds" of experience, perhaps something like the traditional liberal distinction between state and society.

Although strongly recommend this book, it would have been strengthened if Lane had explored alternatives to current market democracy, had more fully examined the relation between macrophenomena and the "microworlds" of daily experience, and had expanded his analysis of market thinking as ideology and influence.

Perhaps democracies directly contribute little to subjective well-being because the dominant model is limited and often perceived as unresponsive. Although it is not his purpose, beyond a few comments Lane does not address the participatory current in democratic theory—including Aristotle, Jefferson, Mill, Dewey, and Benjamin Barber—that claims democracy can educate and train the citizen. Lane agrees that education is a primary government duty and contribution to happiness, but he slights the participatory heritage and the possibility that small group democracy, even if all will not participate, can expand one's sense of control and companionship.

Lane contends that the microworlds of experience are the locus in which we experience satisfaction. Yet, in numerous places he examines large-scale phenomena that also have an effect. These include unemployment and especially its effect on children, the Great Depression (p. 52), severe economic reversals (p. 62), economic insecurity (pp. 71, 112), "anxiety about the future" (p. 108), and "individualistic doctrine" (pp. 283, 111). Thus, Lane recognizes the larger environment, but he could fully incorporate it into his analysis by examining in detail the interaction between macro and micro levels. This is crucial because, despite popular claims that citizen and consumer are sovereign, everyone is immersed—born, educated, work, and die—in large-scale institutions that individuals are more or less powerless to affect.

Lane is fully cognizant of the often detrimental influence of economic thinking on political analysis, and he criticizes the economicistic fallacy that happiness is "proportionate to income" (p. 64), but he does not systematically examine the market ideology that dominates how many people perceive and seek to shape the world. This theory assumes an acquisitive human nature; justifies distribution and differential rewards; encourages self-interest; claims individuals are powerful; defines freedom, equality, and democracy; shapes what we perceive as problems and acceptable solutions to them; and frequently denigrates government and politics. Its popular form teaches joy through consumption: This car or toothpaste bring sexual fulfillment; your children will be happy and love you if you take them to that hamburger place. As Lane repeatedly states, this is not so; more income or goods do not bring happiness or relieve depression. Yet, constant messages

that consumption brings happiness may affect satisfaction, depression, and the intimate relations that Lane emphasizes. To paraphrase John Kenneth Galbraith (*Affluent Society*, 1958, 1998), there is no systematic propaganda for friendship, companionship, or trading income for time to promote closeness.

This is a balanced, nuanced book that blends normative and empirical theory. It leaves much room for active government and interventionist policy, and it reminds us that political and economic ideas are not autonomous. This imaginative, multidisciplinary contribution questions almost everyone's presuppositions about a decent life.

The Multiculturalism of Fear. By Jacob T. Levy. Oxford: Oxford University Press, 2000. 268p. \$29.95.

Jeff Spinner-Halev, *University of Nebraska*

Jacob Levy argues that the multiculturalism of fear is meant to supplement, not displace, the multiculturalism of rights. Running against many recent celebrations of ethnic identity, Levy is wary of the effects that ethnic (which includes national) identity can have. Too often ethnic politics are cruel and conflictual. Levy is skeptical that a world where everyone's ethnic identity is politically recognized can be peaceful and harmonious. Yet neither can we simply wish ethnic identity away. While cultural identities are socially constructed, they are very much part of our world and so they must be dealt with. "The multiculturalism of fear," Levy writes, "does see ethnic communities as morally important and distinctive, not because of what they provide for individuals, but because of what they risk doing to common social and political life" (p. 33).

Ethnic groups are dangerous, yet they are a part of our world. The result is that we should try to minimize the harm they do. A multiculturalism of fear, directly inspired by the late Judith Shklar's liberalism of fear, focuses our attention on the ways in which groups are victims of cruelty and humiliation because of their ethnicity. For example, Levy argues that when India changed the name of Bombay to Mumbai, it was done to reassert the city's Hindu identity in a way that was meant to taunt the city's many non-Hindu residents. A multiculturalism of rights may have little to say about name changes like this, since this change of names did not violate anyone's rights. The multiculturalism of fear, however, is attentive to the tensions between ethnic groups and can tell us why seemingly innocuous name changes should be resisted. This is an important insight on the usefulness of a multiculturalism of fear.

Related to the multiculturalism of fear (though exactly how is never spelled out) is Levy's negative consequentialist argument. Levy cites Montesquieu as teaching us that laws are often a bad way to change customs; that rewards and incentives are better ways to do so. Outlawing a long-standing custom may have worse consequences than allowing it unaltered or perhaps in a modified form. For example, some immigrant women from Africa recently asked their doctors in the Seattle area to perform a mild version of female circumcision on their daughters. Levy argues that the doctors were right to agree to do so (though they changed their minds under public pressure), because this incision would not have harmed sexual intercourse, sexual pleasure, or childbirth. In a perfect world the question of such an incision would never arise. But we must, Levy warns us, deal with the cultural and religious realities as they do exist. The alternative to the incision performed in the Seattle hospital was, for many of these families, to return to their country of origin to have the cutting under the control of their traditional grandmothers.

Surely it is better to have the cutting performed by the Seattle doctors.

This is an insightful argument, but it is underdeveloped. Levy is not willing to allow all cultural practices to exist. When the rights of women are violated, then direct intervention is called for. This leaves the reader wanting to hear more. What if stopping these rights violations has terrible consequences? Or if we can successfully stop the violations of women's rights in certain cultures, why not also directly stop patriarchal norms?

Just as the negative consequentialist argument is not spelled out enough, so too the contours of the multiculturalism of fear are left unfilled. Levy's argument on name changes is one of few examples of how the multiculturalism of fear works. In fact, Levy's fear argument is prominent only in the first two chapters and then a little in the last one; it is rarely mentioned in the five chapters in between. While the theoretical framework tying the book together is uneven, this book has considerable insights into multicultural arguments. Levy is an incisive critic, with an extraordinary command of cases from throughout the world and a firm grasp on the many arguments for nationalism and cultural rights. Levy is a fox, not a hedgehog. The reader will not learn one big thing from this book, but she will learn many smaller (and worthwhile) things. Levy's book travels through arguments against liberal nationalism and liberal arguments for cultural rights, land, indigenous law, and official apologies.

Levy is particularly insightful when he dissects the categories used in a variety of cultural rights arguments. His description of the different kinds of indigenous laws, and their uses and abuses, and his discussion of the several possible approaches to ethnic symbolism are first rate; so too is the powerful chapter on classifying cultural rights. This chapter, already well known in a previously published form, is a grand tour through the different kinds of cultural rights available and can be used a basic starting point in the cultural rights debates. Yet the normative implications of this chapter are too briefly discussed, and its relationship to the multiculturalism of fear is left unclear.

The relationship between theories of rights and theories of fear is also underexplored. In Levy's well-argued chapters against liberal nationalism and cultural rights, he appears to be against the multiculturalism of rights. Levy is willing to allow group rights only when the alternative is worse; he does not think that cultural membership in itself has much moral worth. Yet sometimes it seem like the liberalism of rights trumps the multiculturalism of fear. Levy gently argues that giving territorial self-government rights to indigenous peoples causes a problem with local minorities, raises concerns about the individual rights of tribal members, creates boundaries problems, and restricts the mobility of nonmembers (which Levy counts as a liberal right). While Levy briefly invokes the multiculturalism of fear argument to justify this position, one might think that the fear argument works the other way: That it is cruel and humiliating to take away or withhold territorial self-government from indigenous peoples. And there is, of course, substantial evidence that doing so has negative consequences for indigenous peoples.

Nonetheless, Levy's chapter on indigenous law, like his chapter on land, pushes political theorists in directions in which we normally do not go. Land, in particular, is a difficult subject that few political theorists have tackled, and Levy's argument provides an excellent juxtaposition of liberal and nationalist views on territory. Here and elsewhere, Levy's willingness to take on a broad range of subjects and arguments will provide readers with many occasions to pause and ponder as they read this richly rewarding book.

Community, Solidarity, and Belonging: Levels of Community and Their Normative Significance. By Andrew Mason. Cambridge: Cambridge University Press, 2000. 246p. \$59.95 cloth, \$21.95 paper.

Russell Muirhead, *Harvard University*

Communitarianism has long suffered from vagueness: what is community? What about it, if anything, is necessary or good? What are the boundaries of morally relevant communities? Often those who attend to the claims of community tender these questions with too little rigor. Not so Andrew Mason. His new book has three main purposes: to map the meaning of community, to assess the claims of political community in relation to communities below the level of the state, and to evaluate the claims of political communities with respect to the global community. In addition to a number of rigorous and thoughtful clarifications and a comprehensive overview of recent debates between liberalism and its critics, the book offers arguments of its own which are often more bold than its judicious manner suggests. It deserves careful attention by all who have attempted to sort through the tensions between liberalism and community.

As those who are familiar with his work know, Mason wields a sharp analytic knife (*Explaining Political Disagreement*, 1993; *Ideals of Equality*, 1998). In the first part of *Community, Solidarity, and Belonging*, he uses this to advantage by clarifying the distinction between *ordinary* and *moralized* conceptions of community. Ordinary community refers to a group of individuals who share some values, possess a way of life in common, identify with the group, and recognize each other as members of the group. The moralized concept contains all of these but adds solidarity or mutual concern. In a moral community, individuals "give each other's interests non-instrumental weight" (p. 27). Mason also distinguishes among different levels of community, including those below the level of the state (such as minority cultures), those at the level of the state (political communities), and those at a global level (a community of states). These inform the questions at the center of the book: What community should government encourage at the level of the state? and How should political community accommodate communities below and above it?

Aiming to balance the protection of rights with a sense of belonging among citizens, Mason advocates what he calls the "inclusive political community." This is meant to be a practical model, not an impossible ideal. It squarely faces the practical difficulty that arises when liberal states contain communities within them that endorse illiberal views. Perhaps it would be ideal if citizens converged on one (liberal) concept of justice, if their togetherness came from something like moral reasoning. But a workable model of political community must take people much as they are—divided, not only by interest but also by cultural affiliation, and therefore unlikely to agree about either principles of right or conceptions of the good. These divisions are the real matter of politics.

Amid such disagreement, Mason aims not for principles of justice that all can accept but, rather, for a policy of accommodating communities below the level of the state. He claims that recognizing and including groups, often on their own terms, would cultivate a widespread sense of belonging. The policy Mason has in mind comes into sharp focus in matters of education. In a tempered endorsement of multicultural education, Mason recommends a "pluralist" school system where schools are permitted to endorse particular cultures. He says, "the *pluralist* model maintains that it is legitimate for teachers to evaluate the ideas of different cultures from some particular perspective, find them wanting and dismiss them" (p. 151). While subject to some constraint (such as a bar on propagating racist or sexist notions in the classroom), Mason

would go a long way to accommodate specific cultural groups. “*In certain circumstances*,” Mason writes, “there may be a strong case for stopping schools from teaching that the holocaust never happened because this would undermine a sense of belonging, but it is less clear that there could be similar reasons for preventing schools from teaching that creationism is true” (p. 161; italics added). Yet the standard for education should not be a sense belonging, but truth. Against Mason, one might insist that in no circumstances should schools teach what is obviously false—and that those, for instance, who cannot abide by the fact that the Holocaust never happened do not merit a sense of belonging.

Indeed, the extent of accommodation Mason recommends gives a fragile cast to his liberal commitment to protect rights. He argues, for instance, that the content and meaning of rights should come from citizens, not lawyers and judges (pp. 81–2, 90). Fair enough, politics has its place, one which philosophy and jurisprudence are often too ready to usurp. Yet in practice this can be a slippery distinction. Although Mason has in mind a kind of dialogue (not the tussle of interest groups), real politics rarely resembles the openness or reflection of nice discussions. To render rights the prey of political dialogue may denude rights of their symbolic and institutional force. It’s an open question whether the broad “liberal perspective” Mason affirms can survive the degree of accommodation that he calls for and continue to count as a liberal doctrine (p. 9). A more direct argument on behalf of maintaining a sense of belonging is needed if we are to fully grasp why, when rights and the claims of cultural minorities conflict, we should relax a commitment to enforcing rights.

Why should liberals care whether nonliberal minorities “feel at home” in the liberal state? One reason might be strategic: The demands of holding polities together might require some condescension from liberal principles. Yet the hand of necessity is not always so near, and in fact Mason’s argument is motivated not only by empirical concerns about political unity, but also by a principled commitment to inclusion. This, in turn, would seem to reflect a particular interpretation of moral equality. Since the consequences of these commitments are profound, their justification needs to be made more visible.

The scope of accommodation, for instance, will depend on the way inclusion is justified. If the argument for accommodation is simply strategic, it might apply only where liberalism has a real fight on its hands and thus needs to accommodate. In other places, where liberalism is dominant, liberals may have the luxury of indulging illiberal groups in their midst. Yet to persuade them to grant such indulgences—or further, to persuade readers that protecting rights can be an act of cultural imperialism (pp. 85, 90)—will take a more principled argument about rights and justice than Mason offers. Indeed, Mason raises vital doubts about “the notion of public justifiability” on which many liberals lean so hard (p. 69). Following these doubts through to the full arguments that underlie them would lend stronger support to the book’s provocative recommendations. As it stands, this work subtly nudges liberalism’s self-image while reminding readers that the politics of protecting liberal rights while accommodating nonliberal groups remains a vexing mix.

Justice and Punishment: The Rationale of Coercion. By Matt Matravers. Oxford: Oxford University Press, 2000. 286p. \$70.00.

Jean Bethke Elshtain, *University of Chicago*

Justice and Punishment begins promisingly. Matt Matravers notes that the question—“Why and by what right, do some

people punish others?”—is “not a new question. The problem of punishment is one of the most enduring in political theory” (p. 1). But over the years, punishment theory has been separated from moral and political philosophy more generally. The upshot is that both punishment theory and moral and political philosophy have suffered. To put things right, Matravers avers, any adequate theory of punishment “must be rooted in a broader moral theory, and that broader moral theory will be . . . constructivist” (p. 1). It is the task of his book to explore why and how this is so.

It is undeniably the case, as Matravers insists, that normative political philosophy has been dominated for three decades or more by theories of justice. But these theories, including John Rawls’s signal achievement, have concentrated on distributive justice and questions of retributive justice have been excluded. Why is this the case? Matravers has an answer: The separation occurred because “contemporary theories of justice cannot explain the relationship of justice and morality more broadly conceived” (p. 2). This seems a way to restate the problem rather than to offer an answer. But the problem itself is stated clearly: Absent an overall moral context within which to situate both distributive and retributive accounts, such accounts become less connected to, or intelligible to, one another. Going their own separate ways impoverishes each.

Matravers builds his case by exploring, first, what is at stake in the debate between consequentialism and retributivism—the dominant ways of construing the punishment task as an autonomous enterprise. But must one choose one or the other? Are these approaches mutually exclusively? Matravers argues that, no, they are not and that each captures insights that any comprehensive theory must take into consideration. Indeed, second, any account, to be compelling, must incorporate what he calls a “correct understanding” of what is due to responsible moral agents. Is there something like what Simone Weil would call a right to punishment? (Surprisingly, Matravers does not reference Weil, this despite the fact that punishment features centrally in her major treatise, *The Need for Roots*.)

Contemporary punishment theory is a failure because, no matter how the question is refracted, the need to integrate and to justify an account of punishment within a larger overall theory is ignored. Matravers’ first five chapters are devoted to the critical task of showing the internal weaknesses and flaws in current accounts, whether consequentialist or retributivist; whether justice is construed as impartial or mutually self-interested. Chapters 6 through 9 offer his constructivist theory of moral norms, going on to locate coercion and punishment within the framework of what he calls a comprehensive account of the moral community.

This is a very tall order and Matravers hasn’t quite met his goal. His meticulous unpacking of the accounts he finds wanting is carefully done. But his arguments suffer from a fair bit of repetition as he states and restates the central dilemma and his assessment of that dilemma. Moving to what he calls “constructivism” rather than “contractualism,” and it is less clear than it ought to be precisely what hangs on his rather idiosyncratic use of the term “constructivism.” Matravers builds up a moral context within which punishment theory can be located. The elements of such an account must be complex and multiple. Any such account must not only take seriously questions of reciprocity, fair play, and mutual advantage, but also incorporate matters of “sufficient security” and “justified coercion,” else punishment theory will wither on the vine.

Especially important here is Matravers’ explicit recognition of “the educative function of punishment” whereby a community “expresses its abhorrence at the offender’s

action" and reaffirms the choice all persons must make between the requirements of membership and the yearning for some hypothetically independent existence. Despite the importance and complexity of its many parts, *Justice and Punishment* doesn't add up to the compelling, comprehensive moral theory within which Matravers claims, rightly, any account of punishment must be imbedded. His conclusions restate the problem, this time as a claimed accomplishment. But a few quite essential ingredients are missing—including a clearer articulation of precisely what Matravers means by "the moral community" in light of the fact that the conditions of contemporary pluralistic societies are such that we find ourselves members of a number of overlapping moral communities whose requirements for membership in good standing and standards of what counts as an infraction and what as acceptable punishment may conflict with, rather than reinforce, one another.

One minor note: In his determination to right the wrongs of male pronouns, "she" becomes the new universal in Matravers book. The upshot is that all murderers are "shes." This is a bit unsettling.

Suffering and Moral Responsibility. By Jamie Mayerfeld. Oxford: Oxford University Press, 1999. 237p. \$45.00.

Joan C. Tronto, *Hunter College, CUNY*

Jamie Mayerfeld has written a wise and morally sensitive book that he hopes will compel readers to take seriously their "prima facie duty to relieve suffering" (p. 9). Insofar as "attention to suffering has been a casualty of a long series of attacks on hedonistic utilitarianism" (p. 3), Mayerfeld offers a thorough account of the nature of suffering and argues for the view that its badness imposes a universal prima facie duty for people to try to avoid suffering. Since the purpose of moral inquiry is "to identify wrong kinds of behavior so that we can avoid them" (p. 7), Mayerfeld, not himself a utilitarian, follows a catholic approach and skillfully draws upon arguments from utilitarians, deontologists, Aristotelians, hedonists, psychologists, and philosophers to support his moral intuitions.

The book begins with a detailed account of the nature of suffering and how to measure it. In the fifth chapter, Mayerfeld provides his justification for the prima facie duty to relieve suffering: "because suffering is bad and ought not to occur.... Its occurrence makes the world that much worse" (p. 111). In the last several chapters he discusses limits to the prima facie duty to end suffering, but he concludes in the end that, "at a modest sacrifice, fortunate individuals can prevent an enormous amount of suffering.... Failure to do this is unjustifiable" (p. 224).

Mayerfeld's treatment leaves a number of interesting features of this topic unexplored. The book is not so much about political theory as it is about identifying a moral duty. The political questions that arise from recognizing the existence of such a duty remain unconsidered.

First, we might inquire about the origins of suffering, a topic that is not much discussed here. Mayerfeld is eager that we not focus too much upon evil doing as the source of suffering lest we will think that if we have not committed the evil, then we are not responsible for the suffering or its prevention. Mayerfeld suggests that our failure to follow the precept to prevent and reduce suffering "results largely from a mixture of inattention and indifference. Partly, it results from ignorance or confusion about the meaning and moral significance of suffering" (p. 224). He does not care to pursue the natural, economic, personal, or political causes of suffering any farther. It does not matter to him why there is suffering; the fact

that it exists is enough for us to try to prevent or reduce it. As do so many liberal theorists, Mayerfeld seems to think that ignorance alone is responsible for what is wrong in the world; that there might be evil doers who are responsible for this suffering is not the issue. That people obviously also find reasons for inflicting suffering on others, or for ignoring it, cannot be accounted for. Mayerfeld also does not imagine that human actions, even those designed to eliminate suffering, can have harmful unintended consequences. Postmodernists might see in his approach a return to an Enlightenment optimism about the possibility of the mitigation of suffering.

Second, Mayerfeld is so eager for us to recognize suffering per se that a reticence to make judgments about the topic creeps into his writing. The discussion is fairly abstract, and his examples (ranging from slamming one's finger in a door to torture) are not richly described. Such distance is undoubtedly calculated, as it avoids the harm of using others' suffering for exoticism or titillation. Mayerfeld provides ways to compare different kinds and amounts of suffering, but he refrains from making any broad judgments. He stresses the importance of torture, but he is willing to grant that each of us will decide what form of suffering matters the most to us, that for each of us, relieving suffering "will soon take on the character of a *personal project*" (p. 208). This overall view seems to be that if enough people (all people?) are willing to accept their duty to relieve suffering, then the burden will not be too onerous for any one person in particular (pp. 206-7). Mayerfeld allows that "we would need some mechanism to insure that the appropriate division of labor was actually observed, but I assume that in a world where every one was committed to the prevention of the worst cumulative suffering, this would not be an insurmountable problem" (p. 206). He makes this claim as if this political process could be taken for granted.

Mayerfeld's argument begins from moral intuitions and ends up being able to do little else except try to convince each, individually, to accept this moral starting point for how we might live our lives. Yet, Mayerfeld also shows that two complications make it unlikely that, short of political action, there is any way to achieve this result. First, it is almost impossible for people to perceive the suffering of others and remain focused on it. Second, the duty to relieve suffering is "not as transparent as, say, the prohibition against killing," so that the duty to relieve suffering requires that "we need to calculate what would actually be required to prevent the worst possible suffering" (p. 120). For both of these tasks, to keep focused on suffering and to make calculations about what is required to prevent it, some forms of political institutions and processes are necessary, but Mayerfeld does not want to consider such questions here.

Mayerfeld has done a great service by drawing our attention to suffering. His arguments are careful and thoughtful, and this book should become the standard work on the topic. If only the problem of human suffering were as susceptible to human reason as Mayerfeld thinks it is.

Bodies of Meaning: Studies on Language, Labor, and Liberation. By David McNally. Albany: State University of New York Press, 2000. 277p. \$72.50 cloth, \$23.95 paper.

Nicholas Xenos, *University of Massachusetts at Amherst*

David McNally styles this book as beginning in a polemic and ending in a "materialist approach to language" much indebted to the German critic Walter Benjamin. The charge is that "postmodernist theory, whether it calls itself poststructuralism, deconstruction or post-Marxism, is constituted by a radical attempt to banish the real human body—the sensate,

biocultural, laboring body—from the sphere of language and social life" (p. 1). By treating language as an abstraction, McNally argues, postmodernism constitutes a form of idealism. More than that, it succumbs to and perpetuates the fetishism of commodities disclosed by Marx insofar as it treats the products of human laboring bodies as entities independently of them. Clearly irritated by the claims to radicalism made by those he labels postmodern, McNally thinks he has found their Achilles' heel: "The extra-discursive body, the body that exceeds language and discourse, is the 'other' of the new idealism, the entity it seeks to efface in order to bestow absolute sovereignty on language. To acknowledge the centrality of the sensate body to language and society is thus to threaten the whole edifice of postmodernist theory" (p. 2).

Famously, and somewhat ambiguously, Marx dedicated *Capital* to Charles Darwin. Darwin figures prominently for McNally, too, and he starts out with a chapter on Nietzsche's relationship to him. McNally finds it curious that there is little contemporary discussion of that relationship: "For here I detect something of a symptomatic silence, an avoidance that hints at a resistance. Read Michel Foucault, Gilles Deleuze, Jacques Derrida, and their followers when they discuss Nietzsche. The near-total absence of Darwin's name is striking" (p. 15). McNally argues that while Darwin provided the inspiration for Nietzsche's attack on the transcendental philosophy of Kant, he later rebelled against the egalitarian implications of Darwinism and produced an idealist reaction against his own early "materialist impulse." This reaction is entailed in the imperative of self-overcoming, the desire to free oneself of the physical and psychic limitations of the body. It also entails a struggle with language, since for Nietzsche language encapsulates the consciousness of the herd in its universalizing concepts. Thus for true experience to be revealed, it must somehow find expression through the language that conceals it. This antidemocratic, disembodied Nietzsche, he claims, is the inspiration to Heidegger and the darling of postmodernism (p. 38).

McNally's approach runs the risk of forcing his concerns onto the vaguely defined "school" he seeks to undermine rather than engaging with the substance of any particular theorist. For example, McNally notes that Deleuze does mention Darwin in his *Nietzsche and Philosophy*, but only to point out Nietzsche's preference for Lamarck's more active, transformative notion of evolution. He chides Deleuze for not pursuing the significance of Lamarckianism for Nietzsche and claims that it is to be found in *Beyond Good and Evil*, where McNally sees an effort to create an "aristocratic biology" (p. 27). However, McNally ignores the main focus of Deleuze's book, which is on Nietzsche's critique of nihilism and of dialectical philosophy. Since McNally's project is to validate some form of dialectical thinking, dealing with Deleuze's reading of Nietzsche at that level would seem more productive than projecting an overriding concern with language onto his text.

One theorist McNally does engage in a fairly systematic, albeit brief, way is Derrida. McNally argues that Derrida takes over the worst elements of the theory of language of Ferdinand de Saussure; namely, the notion that linguistic meaning is a product of differentiation within language systems, that meaning is constituted out of the difference between signs within a system. For McNally, this notion of language is an exact replica of the functioning of commodities within capitalism, wherein the value of a thing is determined by its relationship to other things rather than in relation to some intrinsic value or usefulness. Saussure's theory of language is modeled on an economic system, but that system is taken as timeless and universal, whereas in truth it is historically specific. If anything, Derrida is more consis-

tent than Saussure in following this model, McNally claims, a consistency that is best illustrated in Derrida's *Given Time: I. Counterfeit Money*. There, Derrida is said to construct a notion of the subject as capitalist, as a calculator of identity through debt and exchange, "[a]nd since subjects are constituted in and through language for Derrida, language too must be a system of calculation, a capitalist system" (p. 61). In such a system, there is no discerning between the real and the counterfeit, between cash and credit. McNally then links Derrida to Jean Baudrillard's writings on simulacra to establish the point that postmodernists have reproduced the contemporary world of financial markets and consumer sovereignty, and thus fallen prey to the phantasms of a commodified society.

The phantasm theme and a notion of language that originates in a prelinguistic material world is the principal subject of McNally's largest chapter, on Walter Benjamin. But before he gets there, McNally turns first to contemporary work in evolutionary biology, then to the literary studies of the Russian writers Valentin Voloshinov and Mikhail Bakhtin. Neither are necessary. The survey of Darwinism, paleontology, and anthropology is there to prove a few things: that bodies "speak" through gesture and mimesis, that the capacity to think and to cooperate therefore precedes language, and that the body is historical. All of this may be true, perhaps even more than trivially true, but McNally's confidence will not be shared by everyone, since he is selectively synthesizing a wide variety of theories that support his polemical purposes. His effort here might be reminiscent of Rousseau's use of the materials available in his time to construct a hypothetical evolution of human beings and society in his *Discourse on the Origins of Inequality*, but McNally lacks Rousseau's sense of irony.

Voloshinov is enlisted to provide a theory of language that is attentive to a "diverse range of socially structured contexts in which people live their lives," which are in turn products of specific relations of production (p. 117). However, McNally finds that Voloshinov cannot transcend such undialectical oppositions as between the natural and the social and the biological and the historical. Bakhtin has the merit of bringing the body back into language and McNally emphasizes his interpretation of carnival as an occasion not only for the inversion of dominant notions of the body in the carnivalesque celebration of the body's "lower stratum" (genitals, bellies, excretory functions, etc.), but also for the expression of unrealized communal aspirations. But McNally also criticizes Bakhtin's interpretation for failing to place the carnival within the context of developing capitalist markets and to see the role carnivals also played in legitimating various social hierarchies through the exclusion or parodying of oppressed groups.

McNally's interpretation of Benjamin's theory of language emphasizes the notion of mimesis, "the act of imaginatively entering into material things" (p. 185). This concept figures in Benjamin's understanding of childhood play as an entering into the physical world. Happiness is this oneness with the world, a relationship that is occluded in adulthood, and the mimetic faculty "takes refuge in language." Thus there emerges "a sort of *linguistic unconscious*" that is the repository of our sensuous relationship to the world of things (p. 187). McNally rightly emphasizes that Benjamin increasingly came to identify the relationship between people and between people and things in terms of the fetishism of commodities. The task, politically and intellectually, thus becomes to liberate the happiness that is repressed in language and to realize a nonfetishistic relationship to the world of things. McNally notes that Benjamin's notion mirrors Freud's concept of the unconscious and that Benjamin tried to work out

on a collective basis Freud's idea of shock. Dialectical images played this role for Benjamin, images that would disrupt the fetishized world of experience and release the repressed memory of happiness. The key move here is to reproduce Freud's notion of the individual unconscious as a collective unconscious, individual memory as collective memory, which is particularly prevalent in Benjamin's unfinished *Arcades Project*. This is a dubious move in Benjamin's theory, at best, but McNally reads Benjamin as maintaining that, in the era of mass consumption, "[b]ourgeois society produces ... a tremendous uniformity of desires and wishes, its inhabitants really do dream the same collective dreams" (p. 199). This notion is never demonstrated by McNally with any concreteness, and its general character effaces the language of class and gender he is otherwise at pains to deploy.

McNally's polemical intentions thus undermine the parts out of which this book is fashioned. He is determined to find commonalities among theorists as divergent as Deleuze, Derrida, and Baudrillard and begs the question by labeling them all postmodernist. The specific texture and concerns of these writers and others are thus lost to view, and the very real challenges they pose, in turn, to McNally's "materialist" categories are ignored or glossed over. The chapter on Benjamin, in many ways the most interesting, comes up short when Benjamin's concepts prove to be less orthodox than McNally's. It is also noteworthy that in attempting to reclaim Benjamin from those whom he classes as postmodernists, McNally ignores the engagement with Benjamin's notion of language by Derrida or Paul De Man. McNally's book thus not only begins but also ends in a polemic that leaves everything standing as it was before.

Citizenship and National Identity. By David Miller. Cambridge, MA: Polity, 2000. 216p. \$29.95.

Jacob T. Levy, *University of Chicago*

From the title one might expect a sequel of sorts to the author's highly regarded *On Nationality* (1995). The volume is both less and more than that, although mostly more. David Miller has long been critical of the Anglo-American liberal approach to political theory and has advanced his criticism along a number of fronts. To oversimplify, Miller is not a liberal, he is a civic republican; he is not a universalist liberal, he is a nationalist; he is not a liberal democratic, he is a deliberative democrat; he is not an economic liberal, he is a social democrat.

Some North American critics of liberalism eventually become concerned to show that, really, they are liberals, too, and they are only concerned to debunk certain metaphysical or rhetorical moves made by certain liberal theorists. Miller does not go in for that sort of backsliding. (The now common references to the theory of *On Nationality* as a species of the genus "liberal nationalism" therefore does him something of an injustice.) He advances serious, careful, civil criticisms of what he takes to be the standard liberal approaches to fundamental questions of political order. He does not blur the distinctions between liberalism and his own views. Liberalism seems to him too individualistic and too universalistic, insufficiently attuned to the collective political, economic, and social projects that take place and ought to take place at the level of the nation-state. In this book he draws those themes together. This makes for a work that, instead of developing much farther the nationalist argument in *On Nationality*, shows the links between that work and the theories Miller has elsewhere (e.g. *Market, State, and Community*, 1989; *Principles of Social Justice*, 2000) developed regarding social justice and citizenship.

Eight of the ten chapters have been previously and separately published, but they hang together quite well. The oldest, written nine years ago, is a piece on deliberative democracy and social choice theory that does feel somewhat dated. It advances what has become the standard view: Deliberation about the common good diminishes the force of social choice theory concerns regarding the coherence of democratic outcomes. But it sets the stage for subsequent chapters in which Miller argues for the close tie between an ideal of deliberative democracy and his understandings of republicanism and nationalism. The balance of the book is largely concerned with demonstrating the value of citizenship in a democratic-republican nation-state and defending it against both more local and more global claims.

Miller argues that "nationality answers one of the most pressing needs of the modern world, namely, how to maintain solidarity among the populations of states that are large and anonymous, such that their citizens cannot possibly enjoy the kind of community that relies on kinship or face-to-face interaction" (pp. 31–2). His is a nationalism stripped of much of the pretense that national sentiment is somehow an extension of the desire for a homogeneous, local, immediate community. Indeed, Miller is acutely concerned to defend the extended community of the nation-state against the claims of ethnic and cultural communities that might more closely approximate a face-to-face ideal. Roughly half the book consists of arguments against various forms of multiculturalism, the politics of difference, and pluralism that might shift citizens' primary loyalties downward from the nation-state.

Miller suggests that liberal and libertarian understandings of citizenship and pluralism are too fragmentary, and a robust republican conception of citizenship is necessary to overcome the centrifugal forces of modern pluralism. He thinks there are genuine national identities attached to the British, Spanish, and Canadian states, and not only to the Scottish, Catalan, and Québécois subunits; and where nationality takes this nested form, secession by the national minority is not only imprudent but also unfair and actually contrary to the principle of national self-determination. (The British nation has to have a voice; the decision cannot be made by the Scottish nation alone.) He defends the shared project of deliberative democracy against those who say that it disadvantages minorities.

Some parts of this argument are more persuasive than others. In the chapter on liberal, libertarian, and republican responses to pluralism, Miller makes the familiar point that Rawlsian liberal citizenship is supposed to be justified in terms of respect for pluralism, but it excludes those whose religious or other primary identities are incompatible with the liberal polity. He argues that republicanism is more inclusive because it "places no limits on what sort of demand may be put forward in the political forum," and the success of any particular claim depends only on "how far it can be expressed in terms that are close to, or distant from, the general political ethos of the community" (p. 57). But the apparently greater inclusiveness at the level of political argument is bought at the very high price of a shared commitment to a unified political project, regardless of its outcomes. The (in Rawls's terms) unreasonable believers may be brought into common political life, but they must agree to make the common political project more important than their private religious lives. Miller thinks liberal and republican citizenship will converge in practice, for instance, because the republican polity will adopt the usual liberal rights. But it is by no means clear that the subordination in principle of private rights to public outcomes ought to be more preferable to religious or other minorities than the exclusions in principle of liberal public reason.

In addition to arguing against giving priority to subnational groups and identities, Miller engages with partisans of supranational politics. Pieties about cosmopolitan citizenship may jeopardize the reality and real importance of "bounded" citizenship within a finite state; we should not interpret the demands of global justice so broadly as to forget that social justice is primarily an attribute of political communities with shared histories and social understandings. "Global justice" sometimes seems to be one of those topics that attracts writers only after they have a fairly expansive understanding of it. Miller's intervention in this literature is welcome, and I hope he will develop his arguments on this front further.

In so few pages Miller cannot and does not try to restate the separate positive cases for his theories of nationality, social justice, and republicanism. Instead, the book makes the case that those theories mesh, and the combined whole is able to respond to a variety of criticisms. The reader looking for refinements to Miller's previously stated views may be frustrated; other than the (quite important) chapters on secession and nested nationalities, there is little that decisively adds to *On Nationality* or *Principles of Social Justice*. To those unfamiliar with Miller's earlier works, this volume may seem like a collection of skeptical review essays on multiculturalism, social choice theory, global justice, communitarianism, and so on, with not much argument for Miller's own preferred alternatives. Moreover, the author's ethical position in general is founded on a Humean deference to received sentiment that, even when fully developed and laid out, can be difficult to engage with. Critical argumentative steps are filled by the appeal to sentiment. Liberal citizenship provides insufficient unity for the pursuit of social justice in the nation-state. Multiculturalism undermines that pursuit; appeals to cosmopolitanism weaken it; and so forth. But the defenses of both nationality and social democracy depends on the existence of sentiments in their favor. Those whose sentiments, or principles, do not incline them toward the social democratic nation-state will sometimes find no point of intellectual entry.

All of that said, this is a valuable addition to Miller's valuable corpus. His complex but generally unified theory of politics has genuine differences with most others on the academic scene. In *Citizenship and National Identity* Miller makes the case for the theory's unity and politely but vigorously engages with its rivals. Both aspects of the collection help clarify the shape and scope of his intellectual project.

Hannah Arendt and the Politics of Tragedy. By Robert C. Pirro. DeKalb: Northern Illinois University Press, 2000. 224p. \$38.00.

Annabel Herzog, *University of Haifa*

Hannah Arendt's so-called nostalgia for the Greek *polis* stands at the core of most readings of her work, especially in debates between proponents of her concept of action as agonistic and interpreters of this concept as associational or communicative. Many feminist theorists, participatory democrats, and liberals share an aversion to Arendt's philhellenism and criticize her machismo, her apparent neglect of Athenian injustice, and her "republicanism," with its potential for endangering individual autonomy. Similarly, Arendt's emphasis on the political relevance of stories and her self-acknowledged storytelling have also given rise to extensive interpretations. Arendt scholars, in line with many contemporary political theorists, reject the totalizing and universalizing power of theory and argue that human plurality is better expressed in stories than in abstract homogeneous theory. According to them, by exemplifying or illuminating general intuitions and propositions, storytelling concretizes the understanding

of politics. They suggest that stories allow the political thinker to be critical and situated. Moreover, stories take into account forgotten parts of history, or forgotten parts of the political sphere, often denied in theories that cannot accept difference and contingency.

As Robert Pirro rightly argues, no one has really studied the link between these two sides of Arendt's thought, that is, her use of the Greek tradition and her storytelling. In his carefully argued and well-documented book, Pirro attempts to "compensate for this neglect by indicating how an unacknowledged theory of Greek tragedy... shaped her understanding of storytelling" (p. 9). He patiently analyzes the distinct foundations and intuitions of Arendt's thought and brings them together toward the discovery of a "theory of tragedy," which, by analyzing the relation between her narrative method and her admiration for Greek political culture, sheds new light on her work. In so doing, Pirro affirms the relevance of such a theory in promoting contemporary democratic participation, challenging opponents to Arendt's philhellenism, and revealing the task of the political theorist.

The book starts nicely with an analysis of the themes and narrative strategy of Arendt's early article "We Refugees," which, as Pirro demonstrates, already sets "the basic terms she would use to theorize for the next thirty years about the nature and meaning of politics in the modern world" (p. 5). "We Refugees" reveals two major issues of Arendt's thought. The first is her "struggle both to be a part of and to stand apart from a group of people with whom one identifies," which is aimed at promoting a "form of public- and critical-minded democratic citizenship" (p. 7). The second is her elaboration of a narrative "that both conveys the shocking novelty of the experiences it recounts and asserts a claim for their exemplary significance" (p. 6).

In chapters 3 and 4 we are shown how Arendt's focus on tragedy follows from these issues. Pirro emphasizes that, according to Arendt, action and freedom need to be limited, or shaped by a stable framework. In other words, contingency appears only in a predetermined structure (pp. 52-3), the lack of which characterizes our modern times. He shows that Arendt distinguishes among three historical periods: a Greek period, during which the stable structures of political freedom were provided by tragedy; a period starting with pagan Rome through to the nineteenth century, defined by authority; and postauthority, contemporary times. In our world, so long as a substitute for authority is lacking, there can be no real practice of political freedom. In ancient tragedy, Arendt finds examples of a political life with a stable framework but without authority, which help her elaborate a postauthority alternative.

Indeed, Pirro's opposition to the widespread view that Arendt is nostalgic for authority is a major quality of his book. Yet, to say that tragedy offers an image of a nonauthoritarian public life does not necessarily imply that the polis was managed in such a nonauthoritarian way. Moreover, Pirro suggests not only that tragedy was, for Arendt, a faithful image of the Athenian polis way of life (p. 75) but also that it promoted such a way of life, that is, a life without authority (pp. 78-9). He is therefore led to the conclusion that Arendt was a pure product of a "predominantly German tradition of thought, which contends explicitly that Greek tragedy acted as the foremost institution of political education" (p. 78). This would be only of historical relevance were not Pirro to suggest that Arendt recommended some "equivalent of tragedy as a contemporary replacement for... authority" (p. 79). This equivalent is storytelling, which serves "to promote political freedom as a mode of being that is accessible, continuous and durable" (p. 76).

To understand how storytelling achieves such a purpose, Pirro turns (in chap. 4) to Arendt's notion of judgment and

its corollary, the political spectator. He shows that, again, Arendt finds her examples in Greece, namely, in the Athenian tragic spectator (p. 99). Thus, what seems to interest Arendt in tragedy is not only its educative political content but also a way of listening and looking at the world that tragedy induces in its spectators (p. 139). Pirro then formulates the theory of tragedy implicit in Arendt's work, a theory of "reconciliation and initiative-taking" (p. 142) based on the identification of the audience "with the hero in his twofold role as immortal and as member of the dead" (p. 141).

This formulation of Arendt's understanding of Greek tragedy is highly plausible, as is Pirro's suggestion that her storytelling was intended to replace authority in modern times. Some other claims are less persuasive. Pirro provides many examples of Arendt's analysis of Greek tragedy but almost no examples of her own method of writing. This lack has two main consequences. First, one may still feel entitled to wonder about the modern political relevance of a theory of Ancient Greek tragedy. In other words, Pirro does not evoke the original and new features of Arendt's storytelling and portrays her as focused on preauthority times. Hence, we are still given a "nostalgic Arendt" and find it difficult to understand her contribution to contemporary times.

Second, Pirro provides a lengthy description of the significance for Arendt of tragedy in politics but virtually ignores the tragic features of her own narrative. In other words, he shows what Arendt has to say about tragedy but not what she told tragically. This results in a contradiction. On the one hand, Pirro's emphasis on the importance of storytelling as against or complementary to theory leaves us with a convincing theory of tragedy but fails to provide an account of the political function of storytelling in our times. On the other hand, his exhaustive account of how tragic storytelling worked on Athenian citizens fails to show how a theory of tragedy works on modern citizens. Hence, Pirro fails to contribute to our understanding of how Arendt's theory of tragedy might help resolve modern political problems.

Total Freedom: Toward A Dialectical Libertarianism. By Chris Matthew Sciabarra. University Park: Pennsylvania State University Press, 2000. 390p. \$65.00 cloth, \$24.50 paper.

Peter G. Stillman, *Vassar College*

Sciabarra's book attempts to conjoin dialectics with libertarianism to produce total freedom. He is led to this seemingly odd conjunction by a concatenation of concerns. He sees dialectics as the logic or method most attentive to contexts and libertarianism as a radical political ideology of freedom. He sees the opportunity to free dialectics of its totalitarian (including Marxist) overtones and libertarianism of its apparent irrelevance, which is the more galling now that once-popular Marxism has failed as radical social theory. He wishes to combine his own academic appreciation of the dialectical elements of Marx's method with his long-standing love of libertarian ideas. Primarily, he hopes to expand libertarian thought from a narrow concentration on economic self-interest and the state as repressive to a broader concern with the cultural, social, and historical preconditions of freedom, and he sees dialectics, with its emphasis on contexts, dynamism, and relations, as a method that can be appropriated by libertarians to realize these broader concerns and to propound a comprehensive and radical social theory. No longer need libertarian thought be seen as atomic individualism struggling for freedom against state violence; building on dialectical thinking shorn of its Marxist content, libertarians can embrace whole individuals living in rich social environments that can carry

out, without violence, the social powers that the state has illegitimately appropriated.

The result is a long book divided into two almost-equal parts: "Dialectics: History and Meaning" and "Libertarian Crossroads: The Case of Murray Rothbard." The book ends with a brief chapter surveying recent exemplars of dialectical libertarianism and a briefer epilogue expressing the hope of continuing this new way of thinking. Sciabarra's approach in both parts verges on the encyclopedic: He presents a topic or thinker by encompassing a wide range of secondary sources or alternative positions (or both). Whether the topic is Marx on the dialectic or Rothbard on the origins of the state, he wishes to report almost every possible opinion that might be germane, and his bibliography (48 pages) shows it. He responds to critics of his past two books on points relevant to this one; he appends explanatory or exploratory footnotes that mention interesting, significant, or tangential issues; and he includes many footnotes thanking colleagues for helpful comments or communications, implying a community of scholars and interested parties working together on a novel but important political undertaking. No one can doubt the amount of his scholarship, his commitment to his topic, his generosity of spirit, and his desire to encompass as many opinions as possible.

The result is a hefty book. But I kept wishing that the book were twice as long, so that Sciabarra could have made informative arguments about every issue he mentions, or—and better—half as long, so that he could have focused on the central points and developed them with careful analysis and examples. I also kept thinking that Sciabarra has written two books—one scholarly, the other hoping to influence libertarian ideas—that are melded uneasily into one volume.

In Part One, his first three chapters present a history of dialectical thinkers and his fourth ends with a definition of dialectics (p. 173) and a 15-page "unpacking" of the definition; the chapters are a treasure trove of statements about dialectical methodology. How readers evaluate these chapters will probably vary widely; I do not find them illuminating. In his treatment of Hegel, for instance, Sciabarra uses all sorts of secondary sources—late Victorian Hegel scholars, contemporary ones, those who use Hegel for their own ends such as Fukuyama, and economic historians such as Heilbroner—treating their remarks with equal weight. On the other hand, he does not touch on the masterful discussions of Hegelian dialectic in Michael Forster's *Hegel and Scepticism* (1989), Charles Taylor's *Hegel* (1975), or George Armstrong Kelly's *Hegel's Retreat from Eleusis* (1978). Moreover, most of what Sciabarra says derives from what others say about Hegel's dialectic, not from analysis of what Hegel himself says; and when Sciabarra does refer to Hegel's famous dialectics of master-slave (pp. 71–2) and the opening triad of the Logic [being, nothing, becoming (pp. 67–8)], his brevity means that he necessarily omits what many would see as crucial: He mentions only work and satisfaction in the master-slave relation and, oddly, labels "becoming" as a "halfway-house" between being and nothing. Hegel's stress on the dialectic as concrete, his emphasis on negation and, especially, "determinate negation" (mentioned in passing on p. 64, note 26), his refusal to speculate about the future and his argument that freedom can only be comprehended as existent in the present (not posited in some future)—all these are almost completely ignored. As with Hegel, so throughout Part One: many sources, used (or ignored) without apparent discrimination, few references to (and little analysis of) original texts, and even fewer examples of the dialectic at work. The result, however, is mixed: While I find the resulting definition disconnected from the history and too general and vague (so that many thinkers are dialecticians

by Sciabarra's definition without knowing it), some might find the sheer volume of information useful, and Sciabarra himself might well reply that he wishes to present a workable sense of dialectics for libertarian thought and so gives statements that lead to defining it in terms that can be readily understood and easily applied, even if unsatisfactory for scholars.

I have the same types of problems with Part Two. Sciabarra's wide-ranging and extensive presentation of Rothbard's libertarianism is doubtless the best secondary treatment of the subject, including both sympathetic understanding and severe critique—though much of the critique focuses on Rothbard's (nondialectical) dualism. Reading Sciabarra raised for me a further set of questions. Can a philosophy of origins, like Rothbard's, which presents a hypothetical original condition containing central continuing norms, ever be effectively dialectical (because the central norms are historically unchanging from the original condition)? Can a philosophy built on a Manichean dualism between the good market and the evil state be dialectical? Following Hegel's insights (*Philosophy of Right*, Sections 242–6), does the libertarian tendency to talk about a free market without mentioning wage labor or poverty involve ignoring essential contexts or aspects of the market? Of these three queries, Sciabarra does not address the first; he spells out dualism (pp. 344–6) and asserts that for Rothbard the state will be replaced by voluntary cooperation, but does not show the dynamics or logic for this change (see pp. 346–7); and he mentions and buries wage labor in a two-sentence footnote (p. 259, note 34). Since especially my last query addresses dialectics as "context-keeping," Sciabarra's informal core definition of dialectics, his refusal to address the issue is as disconcerting as his willingness to mention it.

In short, this book is not a scholarly or analytic discussion of dialectics or a complete dialectical critique of Rothbard's libertarianism. But perhaps it was meant to be not a scholarly work but a book dedicated to reshaping libertarian theory and ideology. *Total Freedom* suggests that relational, contextual, and dynamic (i.e., dialectical) methods might strengthen some of the weak points of libertarian theory and might overcome some of the unpopular gaps in libertarian ideology. So it should be read by libertarians and by those who study Rothbard's thought. Even beyond the project of dialectical libertarianism, Sciabarra's book might provoke some thinkers to look again at dialectics—but they will have to go beyond his book if they wish to make sense of dialectical thinking.

Cinematic Political Thought: Narrating Race, Nation, and Gender. By Michael J. Shapiro. New York: New York University Press, 1999. 176p. \$55.00 cloth, \$18.50 paper.

John Seery, Pomona College

Writing on the outskirts of Hollywood, where my college campus episodically turns into a set location for the filming of *West Wing* or, most recently, *Pearl Harbor*, I have picked up a few pointers about the *biz* just by keeping my ears perked. "Talk to me, babe! Luv ya. My agent will get back to you on that. What's my motivation?" And so on. Clichés and formulas do matter, however, in such a large-scale collaborative enterprise because often they provide the underlying rules of the game. The first rule of screenwriting, any insider knows, is that you *must* hook your reader on page one. If you do not grab your audience right from the start, even before the opening credits roll, the rest of the spec script will surely get tossed, and the movie will never get made.

Michael Shapiro begins his *Cinematic Political Thought: Narrating Race, Nation and Gender* thus:

This book is both a series of investigations into aspects of contemporary politics and a more general attempt to articulate a critical philosophical perspective with politically disposed treatments of contemporary cinema.... What I offer is a politics of critique, which, in a Kantian spirit, specifies an attitude rather than a particular destination. My investigations treat various aspects of the present, but they are anti-diegetic and non-hermeneutic; I attribute no clear historical direction to the temporal differences with which I deal and I do not seek to attract a particular interpretation of contemporary life-worlds.

The second stock rule of screenwriting is that plot and dialogue should adhere to Aristotle's *Poetics*. Rewrite coaches often cite Aristotle's line: "Character and thought are merely obscured by a diction that is overbrilliant" (*Poetics* 25). Applying that advice to the above passage, I would have blue-lined at least *anti-diegetic*.

To be fair, perhaps Shapiro's text should not be assessed as a potential script, but he invites such a direct comparison by stating explicitly that he is attempting to write much of the book cinematically (pp. 6–7). Gilles Deleuze, moreover, served as Shapiro's Muse (his "sustained inspiration") for this project:

By directing a series of engagements and juxtapositions among different thought models and different historical moments, I seek to make the present surprising and contingent rather than simply a refinement of certain widely accepted chronologies of historical political trends. In addition to engaging in critical interventions that make use of genealogical and deconstructive modes of interpretation (among others), I make use of the radical temporality of cinematic composition, which, by its mode of presentation, resists the perspectives of the characters and groups whose actions it portrays.

By "cinematic" writing Shapiro means poststructuralism by other means. He wants to deploy jump-cuts, flash-backs, montages, and voice-overs in order to disrupt a more linear or bounded narrative and exemplify an alternative politics of shifting and unsettled identities. Cinematic presentations both show and tell, but the former can undercut the latter. The question we might pose about Shapiro's overall aspiration toward filmic creativity is: Should we regard this particular production, departing as it does from industry standards, as an indie flick, an art-house experiment, or a mockumentary?

Shapiro assembles and directs an ensemble cast of Deleuze, Derrida, Foucault, Lyotard, Guattari, Benjamin—and even Carl Schmitt makes an important cameo appearance—but Kant is the star of the show. Some might question booking Kant as a headliner (hardly the show-stopper with his humorless penchant for talking-head monologue), but Shapiro insists that Kant has been unfairly typecast as a mere method actor posing as high-minded aesthete. Indeed, that is the high concept for the book as a whole: Kant, whose dramatic range allegedly extends from cognitive philosophizing to proto-poststructuralist performativity, can be enlisted to illuminate the real-world effects of several off-beat and low-budget films, which in turn allegedly confirm his cross-over appeal. Does the stretch work? Will Shapiro be able to attract financial backing to get the idea off the ground? If not, what is the point? Why not call a book a book and title it simply and aptly, *Kantian Political Thought*? (The third time-tested rule of screenwriting is, know your genre; keep within it; avoid oddball mixes.)

As a work of political theory, *Cinematic Political Thought* makes many bold, intelligent, and impressive moves. It is

not the first book to move toward a confluence of political theory and film studies; one recalls Stanley Cavell's landmark *The World Viewed* (1980) or, more recently, Michael Rogin's *Blackface, White Noise* (1998), Mary Nichols's *Reconstructing Woody* (1998), Hanna Pitkin's *The Attack of the Blob* (1998), and others. Shapiro's eclectic appropriation of Kant, on reflection, makes perfectly good sense, even if the presentation is polemically pushed and rushed. Shapiro contends that Kant's legacy ought to be understood not only in philosophically stabilizing terms but also in critically political, even radical ways. Kant constructed personhood not just as an apprehending structure of cognition but as a judging intellect that emerges in a political context.

Scratch at the transcendentalist overlay and right below the surface you will always find a Kantian acknowledgment of untamed contingency. Kant moves toward enlightenment neither initially nor even ultimately to introduce unquestionable universalist truths but, rather, to resist ongoing closed-mindedness. He mobilizes a global citizenship that opposes parochial allegiances. In the Kantian scheme of things, the world cannot be understood and experienced without mediation, and any mediating structure betrays some kind of productive human intervention. Shapiro is ambivalent about many of Kant's contributions but finally views him as a critical healer, not as a polarizing dogmatist, which is why he deserves star billing.

The purported connection between Kant and cinema is that both produce an enlarged subject that assumes communicability to a wide audience. The unspoken trick is that such a hyperbolic fantasy can in turn condition and shape further experience, which is to say that in the modern mass-mediated world, the mass media are not simply reflective but also implicitly productive of our cultural meta-narratives; and those backlit productions often provide arrested, entrenched, occluded, and invidious forms of identity. Kant's formal critique of modernity, invoking a *sensus communis*, is instructive, according to Shapiro, but at the same time is insufficient to resist institutionalized forms of intelligibility.

To decode even further the circulatory politics informing modern movies and vice versa, Shapiro turns from Kant to post-Kantian Nietzscheans Foucault and Deleuze. In order to exhibit rival notions of time (à la Deleuze) in order to expose racialized narratives involving sports disciplines (à la Foucault), Shapiro compares two movies, *Hoop Dreams* and *Barry Lyndon*. In short, one is frenetically fast, and the other is painfully slow (trust me, do not assign *Barry Lyndon* to undergraduates as much as you might respect Shapiro's viewer picks); and the separate pacing evinces and reinforces separate epistemes of temporality.

Subsequent chapters take on globalization (and its discontents) and masculinity (and its discontents), respectively. According to Shapiro, many of our nationalist and globalist narratives are based on identity stories that become fix-framed in a collective imaginary. Puritans, Zionists, CPAs, Christian ecumenicals, and security analysts such as Sam Huntington are all prone to such exclusionary conjurations. Shapiro draws upon the movies *Father of the Bride II* and *Lone Star* to make his case. Anxiously heteronormative notions of masculinity and whiteness, prominent in the 1980s and 1990s and bound up with statist and capitalist concerns, can be teased out through a close screening of *To Live and Die in LA*, aided by Shapiro's astute commentary. Since a copy of this film may not be available in your neighborhood Blockbuster, one has to wonder about it, *Father of the Bride II*, and some of Shapiro's other citations: How representative or influentially performative have such films been?

The erudite is situated right next to the kitschy, and Shapiro foregrounds his jarring juxtapositions by calling them cin-

ematic. I see them instead as elegiac, or as another kind of jeremiad, in either case symptomatic of another possible anxiety, namely, that bookish political theory increasingly is being displaced by multimodal modalities in the late-modern world. Our filmic eye candy probably stands in some need of heavy-handed contestation, but somehow I doubt that many movie-goers will be running out of the theaters to pick up a copy of Kant's *Third Critique* as a result of this book. From my armchair perspective, I give *Cinematic Political Theory* a thumbs up for learned decency, a thumbs down for prolixity, and a "PG" rating overall (recommended for postgraduates only).

Rethinking State Theory. By Mark J. Smith, New York: Routledge, 2000. 281p. \$100.00.

Bob Jessop, *Lancaster University*

Mark Smith has written a dense, challenging, and provocative analysis of three contrasting approaches to power and how they are shaped by different philosophies of social science. This is not a book for the theoretically faint-hearted or meta-theoretically challenged. Indeed, those who pick it up expecting to find a simple guide to recent state theory will be badly disappointed. For it does not provide a survey or critique of state theory as such. Nor does it provide a new theory based on self-evident assumptions about the nature of the state and politics. Instead its author offers a sustained *meta-theoretical* commentary on the intellectual conditions of possibility of serious engagement with the state and state power from a broader, societal perspective. Smith attempts this because he discerns a crisis in the taken-for-grantedness of the typical objects of inquiry of such disciplines as economics, politics, and sociology. He claims that their respective objects are increasingly seen as complex, uncertain, and contested spaces and that these disciplines themselves have become disoriented. *Inter alia*, this requires a rethinking of the state as an analytical object. In pursuing this meta-theoretical project, Smith draws heavily on the "critical realist" position (initially known as "transcendental realism" or "critical naturalism") of the British-based philosopher of science, Roy Bhaskar. Thus his analysis begins with some crucial distinctions among empiricism, idealism, and realism and explores their different ontological, epistemological, methodological, and substantive implications for the analysis of social relations. It then addresses the philosophical and theoretical development of three very different theorists of power, who are taken as interesting if not wholly representative exemplars of empiricism, idealism, and a mixture of idealism and realism, respectively. Smith concludes with some of his own meta-theoretical comments on state theory.

The three theorists chosen for dissection are, according to Smith's own labels, the neopluralist Robert Dahl, the interdisciplinary neoliberal Friedrich von Hayek, and the neo-Marxist Bob Jessop. Smith justifies his choice on three grounds: their relevance for contemporary discussions about the state and the good polity, their distinctive focus on the relationship between the state and cultural and economic institutional forms, and their distinctive ontological and epistemological standpoints. He also claims that each advances "an account of the 'sociality of politics,' i.e., the embedding of politics within a broader social order. This takes the form of "polyarchic civility" (a distinctive form of citizenship enabling citizens to exercise full autonomy) in Dahl's mature work, "intersubjective catallaxy" (enabling knowledgeable economic actors to engage in the realization of plans and motives in market economy—in part through what they learn observing the effects of their actions) in Hayek's writings,

and the relationship between the state and “societalization” (or the production of society effects) in Jessop’s work. In exploring the foundational assumptions of these three theorists’ reflections on power, their different intellectual trajectories, and the limitations in their mature theoretical and political positions, Smith hopes both to generate new insights into their respective analytical objects and to develop his own guidelines for rethinking the nature of the state.

Based on his distinctions among empiricism, idealism, and critical realism, Smith provides an extended analysis of Dahl’s movement from an empirical realist, one-dimensional analysis of power to an idealist (in ontological and epistemological terms) account of the structural preconditions and subjective requirements of polyarchic civility. He emphasizes that Dahl shifted toward a normative commitment to democratizing the economy as well as the polity and suggests that this shift is rooted in Dahl’s adoption of a neo-Kantian idealist epistemology. Nonetheless, Dahl continues to neglect the complexities of the state’s internal structures and mechanisms, of the public-private distinction, and the circuits of formal and informal power that shape policy making and implementation. However, it is only by exploring these issues, Smith claims, that one can fully understand the link between political institutions and their cultural conditions.

He then examines Hayek’s social and political thought. He argues that, in addressing the problematic standing of time and the social distribution of knowledge within economic theory, Hayek forges a creative synthesis between the Kantian critique of rationalism and phenomenological explanations of social action. In this context, Hayek develops the notion of *cattallaxy* (a self-generating economic order) and then extends it to a particular account of the “constitution of liberty” based on the rule of law and a nightwatchman state as a matrix for the exercise of individual free choice.

Smith then explores the present reviewer’s work. He provides a very detailed analysis of my work from its earliest presentation in 1979 through work published in 1997. I learned much about my own intellectual development from this, even though I do not fully recognize some aspects of his interpretation. Smith correctly identifies the major turning points in my work and, in particular, its concern with synthesizing the regulation approach in political economy, a neo-Gramscian interpretation of Poulantzas’s state theory, and critical discourse analysis and the catalytic role of my “coqueting” with ideas from the German systems theorist, Niklas Luhmann. Whether or not Smith is correct to argue that my work in 1997 still retained strong elements of idealism (in its ontological and epistemological senses) as well as moving toward critical realism is best left to others to decide. For no theorist is ever completely contemporary with his or her intellectual development.

The book concludes with some critical comments on the recent popularity (described as “me too-ism”) of critical realism because it encourages theoretical complacency. Thus Smith rejects Jeffrey Isaac’s recent description of the later Dahl as a critical realist, Tony Lawson’s critical realist appropriation of Hayek, and my own claim to have embraced critical realism. Above all, he suggests that all three theoretical approaches tend to miss the importance of ontological depth of the real world (i.e., the stratification of the real world and its associated causal mechanisms, capacities, and liabilities) and that they are insufficiently sensitive to the complexities of representing that world both in everyday practice and in scientific observation. This leads him to present in the final pages of the book a new critical realist model of concept formation and empirical inquiry that would be adequate to the sociality of the political, i.e., its embedding in the economy, political institutions, and civil society.

This book is not an easy read for those without a solid grounding in the philosophy of the social sciences and a ready familiarity with the works of the theorists investigated. Indeed it places demands even on those with such background understanding. But it certainly merits close attention by those who want an intellectual challenge and, like this reviewer, are interested in the meta-theoretical foundations of social inquiry. The summaries of the work of Dahl, Hayek, and Jessop are full and fair, the criticisms are well developed and well defended against alternative interpretations, and the theoretical agenda presented by way of conclusion merits attention. This is a book that can be recommended to specialists and for library purchase rather than for course use. It will prove valuable to those interested in exploring the hidden philosophical assumptions of other currents in social theory and in developing a well-founded postdisciplinary analysis of the sociality of state power and the conditions for effective political participation.

Civic Liberalism: Reflections on Our Democratic Ideals.

By Thomas A. Spragens, Jr. Lanham, MD: Rowman & Littlefield, 1999. 271p. \$70.00 cloth, \$24.95 paper.

Stanley C. Brubaker, *Colgate University*

Of the many endeavors in the last three decades to restate the central aspirations of liberalism, this important work is one of the most balanced, nuanced, and cogent. Part of its success lies in its willingness to stretch the boundaries of what we call liberalism, but in doing so, Spragens only brings liberal theory into better alignment with intuitions and sensibilities underlying liberal practice.

His argument is divided into two parts. The first assesses the state of contemporary liberal thought; the second offers his own “civic” alternative. Spragens groups theorists of liberalism into four broad schools, each characterized by a tendency to mistake a central but partial truth for the whole. Thus, “Liberal Realists”—a motley group that includes Hobbes, Rorty, and Shklar—teach us to respect the sober and somewhat fragile achievements of security and civil peace, which safeguard us from our worst fears but neglect our best hopes. “Libertarians” stress moral reality and integrity of individual lives and the economic and political benefits of a market economy but exaggerate the absolutism of self-ownership as well as the tendency for government regulation to set us on “the road to serfdom.” “Liberal Egalitarians”—Rawls and company—remind us of the role of chance in the allocation and development of natural talent, but fail to take seriously our separateness as distinct and morally responsible individuals. “Difference Liberals,” such as William Connolly and Iris Young, properly warn of the tendency of dominant groups to “naturalize” and “privilege” contingent differences, thereby threatening the ideals of liberty and equality, but they themselves privilege and so encourage overstated claims of oppression, thereby facilitating the balkanization of political life.

In light of the success and failures of these other models, Spragens offers in Part Two his “civic” alternative. As befits a liberal theory, liberty and equality retain their central place, but neither is conceived as a goal to be maximized. Instead, liberty is restated as “autonomy,” a “constitutive” and “threshold” good, that puts together the “positive” and “negative” dimensions of liberty that Isaiah Berlin misleadingly sundered. Equality is restated as a “moral postulate” that recognizes our distinctively human capacities for rational self-direction and moral judgment as well as an instrumental good that facilitates other aspects of civic liberalism. In politics, it seeks a substantive equality of influence. In economics, it competes with the need for incentives and the moral claims

of individual desert, presumptive self-ownership, and beneficence among family and friends.

Important as are these restatements of liberty and equality, what gives civic liberalism its most distinctive character is its emphasis on civic friendship and virtue. Friendship, of course, is not a distinctively liberal ideal and, for many theorists, is an ideal that is not even compatible with liberalism. Rejecting Aristotle's account of friendship in favor of that of C. S. Lewis, Spragens places less emphasis on the essential goodness of friends than on their "common affection for an object that they do not embody but rather appreciate" (p. 202)—whether that be pushpin or poetry—and by lowering the bar, he seeks to render friendship complementary to liberalism's ideals of autonomy and equality. Furthermore, by fostering goodwill and trust and thus greater capacity for social action, friendship, he argues, is conducive to limited government, toleration, deliberation, and compromise. Friendship combines with equality to place renewed emphasis on public venues—public libraries, parks, zoos, museums, festivals, and schools. And it moderates autonomy to preserve their civic atmosphere and purpose; so schools need not tolerate disruptive students, libraries can throw out the drunks, and museums can insist on decorum.

There is much to admire in this work: its criticisms are incisive, but balanced; its restatement of liberal ideals is nuanced, mindful of the trade-offs among political goods, persuasive, and well moored in the moral intuitions underlying liberal practice; its spirit is generous, though tough-minded; its style is clear, lively, and friendly. Indeed the work exemplifies many of the ideals that civic liberalism extols.

Despite his rhetorical effort to stay within the liberal tradition, it might be more accurate, however, to understand Spragens' work as a synthesis of liberalism and republicanism. And it is important to note that the republican tradition on which he draws is "horizontal" rather than "vertical," that is, its morality derives more from equality than excellence. Accordingly, his work is not altogether immune from the risks of the former or mindful of its need for the latter. In extending equality in politics from the formal equality of voting to substantial equality of influence, he casually endorses restrictions on political speech (or at least spending on political speech), ignoring the empirical literature on the limited independent impact of money and neglecting the warnings from Madison's Federalist Ten on the threat of such egalitarian impulses to the "first object" of liberal government. One must wonder then how well civic liberalism would safeguard freedom of speech against other contemporary lateral pressures such as the feminist attack against what it calls pornography (i.e., whatever has the effect of subordinating women) or the politically correct attack against "insensitive" speech on college campuses. The "vertical," or perfectionist, strand of liberal republicanism, in contrast, by differentiating types of freedom and holding speech higher than mere expression, allows for the restriction of obscenity and vulgarity without threatening political speech or the pursuit of wisdom. Despite its relation to his project and independent importance, however, Spragens gives perfectionist liberalism comparatively little attention. More emphatically than most liberal theories, civic liberalism does stress the importance of virtue in the citizenry. But virtue becomes an instrumental good serving the ends of autonomy, equality, and friendship, not the embodiment of human excellence. Jimmy Stewart and Charles Kuralt, not Socrates or Lincoln, are its exemplars.

Civic liberalism would bring us a polity better suited for the human spirit than any of the four models Spragens criticizes. It would underwrite a centrist approach to a range of policies—welfare, social security, health care, and immigration—with substantial support in the polity but championed by neither

left nor right. It would restore a civil society of vibrant voluntary organizations. And it would support a vital public realm of "effective public schools, accommodating public spaces, inviting public parks, accessible public libraries, and fiscally viable public hospitals" (p. 258). Perhaps it is too much to ask of a liberal theory that it also be inspiring.

Reproducing the State. By Jacqueline Stevens. Princeton, NJ: Princeton University Press, 1999. 307p. \$49.50 cloth, \$18.95 paper.

Somer Brodribb, *University of Victoria*

Jacqueline Stevens grapples with the meanings of political society and affiliation and how we think about what constitutes family, nation, ethnicity, and race. How do we come to know ourselves and others through these political artifices and naturalized identities? Her project is to trouble our complacencies and make visible the arbitrary practices that produce the inclusions and exclusions of the "state-nation" (p. 43). She examines democratic, communitarian, and liberal theories of political society and finds little attention there to the problem of membership and the ways groups are constituted. Birth, the family, ethnicity, and national origin are undertheorized or considered to derive from natural, ancestral ties. Stevens addresses these inadequacies, and superbly reveals the centrality of birth and kinship practices to political societies.

Reproducing the State is a stunning and original rethinking of justice and the politics of difference. Stevens draws attention to Rawls's understanding of political society "as the location that settles differences, rather than the form that gives rise to them" (p. 4). Her challenge is this: "People starving to death in Ethiopia, dying in cattle cars en route to the United States from Mexico, and losing their homes for want of the right ethnicity in Bosnia...all of these necessarily follow from the prerogatives of political societies to regulate membership according to family ties. These events are not accidents in a world system of familial-based nation-states, but rather are their bases and outcome" (p. 7). The state constitutes and then regulates the intergenerational family form, and this grammar of membership underwrites the nation and reinforces the rules of affiliation and the racial taxonomies of political society.

Methodologically, Stevens draws from Foucauldian forms and practices, Hegelian sense-certainty, and Derridean deconstruction. She is critical of Marx (who somehow forgot intergenerational wealth) and Foucault (who dismissed the sovereign power of the state). Politically, Stevens draws from Rosa Luxemburg and turn-of-the-century anarchists, who "understood that the façade of 'citizenship' which follows from the state obliterated the rights of human beings around the world" (p. 41). Theoretically, she positions herself "critical, neo-Hegelian" (p. xiii). Practically, she urges us "to think through ways of reproducing political societies so that kinship principles would play a diminished role. Two means for accomplishing this would be the elimination of any state involvement in marriage and the curtailment of citizenship requirements based on birth or ancestry" (p. 280).

In contrast to other feminist scholarship, Stevens takes up the role of states in creation of gender rather than the role of gender in the creation of states (e.g., see Carole Pateman, *The Sexual Contract*, 1988). Certainly, this book is a phenomenology of affiliation, not a philosophy of birth (e.g., see Mary O'Brien, *The Politics of Reproduction*, 1981). Stevens is concerned with the traffic in kinship, not the traffic in women (Gayle Rubin, "The Traffic in Women: Notes on the 'Political Economy' of Sex," in Rayna Rapp Reiter, ed., *Toward an Anthropology of Women*, 1975). For Stevens, gender is

constituted through, rather than constitutive of, political societies.

In the context of critical studies of racism and nationalism, this work is unlike the materialist approach to the racist appropriation of labor power represented by Colette Guillaumin (*Racism, Sexism, Power and Ideology*, 1995). As Stevens tells us, the book is "largely about the heuristics of certain forms of being" (p. 280). Contrary to those who contend that racism creates race, she argues that "race always entails race-ism" (p. 173). Indeed, race is "the culmination of political society, family, nation" (p. 173).

An exercise in meticulous scholarship, *Reproducing the State* unravels the complexities of categories of being and belonging. Same-sex issues are clearly of interest to Stevens, and their importance is raised throughout the book. Chapter 4, on race and the state, and chapter 6, on religion and fundamentalism, are rewarding reading. Stevens challenges all origin stories and racial taxonomies based on geographical territories and outlines how they are reproduced through birth certificates and laws of miscegenation and legitimacy.

I am ambivalent about the contribution made by *Reproducing the State*. Often, feminist critical race theory and lesbian/queer scholarship are not fully engaged. A discussion is begun but not sustained or is relegated to the footnotes—truly moved to the margins, and in excruciatingly small type. This is odd in a wide-ranging collection by a well-read scholar who can balance discussions across the Norman conquest, surrogacy adoptions, and an equal rights complaint about Kozy Kitten cat food. The effect is to shortchange these critical analyses, and this diminishes the significance of her contribution. Yet, she challenges some of the key notions of these literatures.

This attempt at a political theory based on how the state reproduces and recognizes populations is situated too narrowly in a structural analysis of discourse with phenomenological longings. In the end, Stevens returns to Hegel rather than hegemony, or at least centered and not scattered hegemony (e.g., see Inderpal Grewal and Caren Kaplan, eds., *Scattered Hegemonies: Postmodernity and Transnational Feminist Practices*, 1994). Emphasizing how identity is tied to the state, Stevens has little room for communities of resistance, social movement praxis, or oppositional imaginations. One clipped and unsatisfying paragraph on "what is to be done" concludes the book. Perhaps it could not be otherwise if transnational feminist practices and literatures are not critically considered. Work on racialized boundaries and democratic futures that could have been analyzed includes M. Jacqui Alexander and Chandra Talpade Mohanty, eds., *Feminist Genealogies, Colonial Legacies, Democratic Futures* (1997). Nevertheless, her book would be well considered alongside transnational feminist cultural studies that confront liberal theory (e.g., Caren Kaplan, Norma Alarcon, and Minoo Moallem, eds., *Between Woman and Nation: Nationalisms, Transnational Feminisms, and the State*, 1999).

This is a dense and challenging work that nevertheless maintains strong thematic coherence as it ranges across political theory, philosophies of language, a phenomenology of belonging and otherness, cultural anthropology, and the legal histories of naming, inheritance, migration, and kinship. It is beautifully prefaced with a fabulously engaging story of the author's work in a library in Croatia, her own genealogy of belonging and classification. Overall, it offers intriguing insights into urgent intellectual and practical problems and provides careful empirical evidence to illustrate how normative claims are substantiated. Jacqueline Stevens has produced a rich and profound work of scholarship that deserves serious attention.

Democracy and Trust. Edited by Mark E. Warren. Cambridge: Cambridge University Press, 1999. 370p. \$59.95 cloth, \$22.95 paper.

Virginia A. Chanley, *Florida International University*

The essays in this edited volume are the product of a seminar and conference at Georgetown University. In different ways, each contribution addresses the complex nature of the relationship between democracy and trust and helps to define the issues and questions involved in this relationship. The contributors address the topic from a variety of perspectives, some primarily theoretical and others combining theory and statistical analysis. As editor and contributing author, Mark Warren provides an effective framework for the diverse set of contributions that comprise the volume and develops a typology of theories of trust and democracy.

In the introductory chapter, Warren defines the topic of democracy and trust as it is developed in the volume and describes the organization of the chapters around a set of distinct, but closely related, questions. He identifies increasing societal complexity and interdependence as creating a paradoxical relationship between trust and democracy (pp. 3–4). With increased complexity, differentiation, and interdependence, individuals have greater choice and opportunity. Along with greater choice, however, there is both a greater need to trust and a reduced ability to participate, either directly or indirectly, in the decisions that shape one's life. In other words, at the same time that individuals are faced with greater choices, there is less opportunity to be involved in making those choices. Rather, citizens must increasingly rely on their elected representatives and other governmental authorities if they are to take advantage of the increased opportunities that accompany increased complexity and interdependence. Thus, Warren identifies a central task for scholars of democracy and trust as developing an understanding of the conditions under which trust is justified and identifying the institutional arrangements that facilitate the development of justified trust. Increases in justified trust help to reduce the costs associated with political decision making while, at the same time, enhancing the possibility of democratic decision making in increasingly complex and interdependent societies.

Warren organizes the book around a distinct set of issues, including questions about whether and when trust is desirable, the kinds of trust that benefit democracy, the relationship between trust and civil society, and the relationship between trust and collective action. In the first set of contributions, Russell Hardin, Claus Offe, and Ronald Inglehart each address the question of whether and when trust in government is desirable. These contributions are also useful for illustrating both the strengths and the weaknesses of the volume as a whole.

Proceeding from a rational choice perspective, Hardin argues that trust is possible only when someone knows the motivations of the person who is being trusted. In this view, it does not make sense to speak of trust in the institutions of government, as citizens are unlikely to know their elected representatives or the bureaucratic agents involved in governing well enough to be able to assess the motivations of their actions. Rather, Hardin proposes that it makes sense to speak of depending upon government or feeling reassured when government is predictable. Trust in government, however, is not desirable, because in modern society citizens are not in a position to assess the trustworthiness of governmental officials.

In contrast to Hardin, Offe argues that trust in the institutions of government is both possible and desirable. For Offe, trusting an institution requires knowledge and mutual acceptance of institutional rules, values, and norms, as well

as the expectation that these rules, values, and norms will be followed. If there is a lack of trust in the institutions of government, this presents a problem for democracy, as strong governments are typically required to resolve complex problems of collective action that face modern societies.

Inglehart takes yet another approach to address the question of the desirability of trust in government. Based on empirical analysis of data from 1990–1991 and 1995–1997 World Values Surveys in 41 countries, Inglehart argues that the stability of existing democracies relies on subjective well-being and interpersonal trust. In contrast, trust in the institutions of government or trust in political elites is not particularly important for the maintenance of existing democracies. In Inglehart's view, declining trust in the institutions of government reflects a decline in respect for authority that has resulted from increased material well-being and the development of postmaterial cultures. Citizens now evaluate their governments by a more exacting standard, and trust in government has declined without posing a threat to the stability of government.

Illustrating the strength of this edited volume, Hardin and Offe approach the question of the desirability of trust from alternative theoretical approaches, whereas Inglehart addresses the question on the basis of statistical analyses across a range of nations. Thus, the reader is presented with alternative conceptions of trust and a statistical analysis that provides for an empirical assessment of the importance of trust in government. Illustrating the weakness of the volume, the reader is left with questions about which approach is most useful or accurate in addressing the question about the desirability of trust. Similarly, the contributions by Eric Uslaner, Orlando Patterson, Jean Cohen, Rom Harre, James Scott, and Jane Mansbridge do not rely on a single conception of either trust in government or democracy. Like Inglehart, Uslaner and Patterson employ statistical analyses to test some of their ideas. Like Hardin and Offe, the remaining authors employ a more theoretical approach to address the issue of trust and democracy, relying on examples as appropriate to illustrate their arguments.

Given that many of the issues raised in the volume are not new to the study of trust in government, perhaps the most unique and important contribution of the work is Warren's outline of a democratic theory of trust (see pp. 310–43). Distinguishing among three approaches to trust in democratic theory, which he identifies as neoconservative, rational choice, and deliberative, Warren proposes that the deliberative approach provides the most promising model for addressing questions about the kinds of political institutions that are most likely to solve problems associated with the paradoxical relationship between trust and democracy.

Overall, the volume makes a valuable contribution to the study of trust and democracy. The authors raise more questions than they answer, but the volume as a whole introduces readers new to the topic to current theoretical and empirical approaches in this area of research. Moreover, in drawing attention to questions about the relationship between trust and democracy and identifying distinct approaches in studying this topic, the volume helps to provide direction for future research.

Democracy. By Albert Weale. New York: St Martin's Press, 1999. 244p. \$59.95 cloth, \$19.95 paper.

Richard Bellamy, *University of Reading*

Like other volumes in the series *Issues in Political Theory*, which Weale edits with Peter Jones, this book combines an overview of the topic suitable for advanced undergraduates

and above with an original argument reflecting the author's distinctive perspective that is likely to stimulate fellow specialists. The combination is an attractive one, which, when it works—as it does in this case—ensures that we do not get the sort of deadly dull textbook that merely rehashes other people's ideas. There are two novel features of Weale's argument. The first is his justification of democracy in terms of its capacity to enable members of society to advance their common interests as political equals in a situation of human fallibility. The second is his attempt to delineate the form of democracy, including the type of representation and decision rule, most likely to promote this purpose. I take each in turn.

Weale believes that we should justify democracy mainly in terms of its having consequences that we can defend as beneficial on principled grounds. In Weale's view, the main attraction of democracy lies in its ability to promote certain public interests under conditions of fallibility, in which no one occupies a privileged position with respect to their political knowledge or judgment, and in communities where people regard themselves as political equals. As he admits, quite a lot is packed into this thesis, that many will find contentious. Nonetheless, in my view it has the great virtue of stressing the important yet limited *practical* role of democracy. It acknowledges, for example, that there are circumstances in which expert adjudication, hierarchical decision making, or individual judgment might be preferable because fallibilism, equality, or public interests, respectively, do not apply. To that extent, his approach has decided advantages over those defenses that appeal primarily to its intrinsic merits in realizing a favored principle or principles, such as autonomy. Of late, theorists have tended to favor this tack. Yet it can lead to the assumption that democracy is either good in itself or good for everything. Different versions of deliberative democracy tend to fall into each of these traps. On the one hand, deliberation gets praised for its role in developing individuals as "political" creatures but in ways that make democracy sound like an educational seminar rather than a decision making process. On the other hand, democracy is expanded to all areas of social life, thereby threatening to overload the capacity of any system to make decisions effectively. In contrast, Weale offers a much more instrumental view that simply stresses that if collective decisions are adequately to reflect the different and often conflicting concerns of those to whom they apply, then we need a form of legitimization that nevertheless protects individuals from having their concerns be simply discounted. As he notes, this aim embodies the most practical interpretations of autonomy and consent arguments for democracy—namely, their antipaternalism and their stress on the importance of people sharing moral responsibility for collective decisions. Both notions can be derived from political equality and are far more suited to a pluralist society than arguments linked to more comprehensive justifications that link democracy to self-realization.

Weale's emphasis on the practical uses of democracy also leads him to give greater attention than many theorists do to the institutional arrangements needed to ensure that democratic processes operate in the desired way. He seeks to make what he regards as realistic assumptions about human motivation and social circumstances that avoid either excessive pessimism or utopian optimism. So he denies that knavery should be taken as a general rule and supposes that ongoing political communities embody a degree of reciprocity that cannot be entirely conceived as merely enlightened selfishness. However, path dependence sets limits on what institutions, no matter how well designed, can achieve in themselves. So representation must balance statistical representativeness against ensuring the accountability of the representatives, and the best mix will have to be sensitive to the complexion of

the society concerned. Likewise with decision rules, with the majority principle needing to be modified to reflect a diversity of opinions and the likelihood that different majorities exist for different issues. Weale's advocacy of a Condorcet count to settle these kinds of problem is one of the most interesting parts of the book.

In most respects, I found myself broadly sympathetic to Weale's approach. My main criticism lies in his not pushing his argument a bit farther. For example, given the sort of instrumentalist line he takes, it is surprising that he gives no attention to contestatory mechanisms of the kind that Philip Pettit has recently insisted upon as vital means for ensuring that democratic decisions genuinely track the public rather than factional interests. Indeed, the republican concern with the avoidance of domination captures much of the reasoning behind his justification of democracy. Given the emphasis on democracy's practical value, I also found the final chapter relating his ideal to real-world situations frustratingly (if also interestingly) elusive. For example, he gives a brief discussion of the infamous "democratic deficit" of the European Union, a topic on which he has written at length elsewhere. This is the sort of issue where I feel his approach has real strengths, though, disappointingly, he fails to bring them out here. Many commentators adopt the sort of arguments criticized above, whereby it is simply assumed that because democracy is a "good thing," the EU must have as much of it as possible. But this begs precisely the sorts of problems Weale's justification raises—namely, is this an area where collective decisions can appropriately be made? If so, concerning what? Are they likely to be better made democratically rather than by elites or technocrats? What forms of representation and decision rule will be necessary to secure political equality? These are rather concrete questions, ones surprisingly rarely posed by policy makers, and it is the merit of Weale's book to focus our attention on them.

Inclusion and Democracy. By Iris Marion Young. Oxford: Oxford University Press, 2000. 304p. \$29.95.

Barbara Cruikshank, *University of Massachusetts*

Engaging her contemporaries in debates over democratic ideals and processes, Iris M. Young offers a collection of seven essays that mitigate arguments on either side of those debates (participation vs. representation, localism vs. state, segregation vs. integration, identity vs. difference) by applying the critical ideal of inclusion. She argues that the normative legitimacy of democratic decisions rests upon the extent to which those affected by decisions are included in or have the opportunity to enter the decision making process. One might think that inclusion solves only one problem, the problem of exclusion, for democracy. However, Young extends the ideal of inclusion across manifold debates in democratic theory and speaks broadly to the less than ideal conditions under which we now practice democracy.

Hers is a process-oriented conception of inclusion rather than a spatial or physical one. Inclusion is not the end point, but a condition of democratic legitimacy. To be included one need not enter into a space of deliberation at all; participation cannot be limited to face-to-face interactions and representation is necessary in large-scale multicultural societies. Among other reasons, direct democracy should not be our ideal because our relations to others extend well beyond the local level at which we encounter others physically. If we think of representation in terms of process rather than identity or substitution, as the ongoing relationship of constituents to one another and to their representative, we will be able to sustain a larger measure of inclusion.

Young rejects the idea that inequality and difference necessarily stymie political inclusion. The deeper conditions of inclusion are not only social, economic, or racial, but also procedural: being heard and understood, autonomy, representation, and the ability to move across political jurisdictions that range from local and civic to global. Even under conditions of structural inequality we might still enliven democratic practice if we explicitly acknowledge and accommodate social structural difference in the democratic process. For example, in these terms an argument for inclusion under conditions of residential segregation need not take a side in favor of either group solidarity or integration, of difference or identity. Segregation can be both good and bad; it offers the supportive backdrop of group solidarity for those marked by difference but it also leaves those whose privilege it is to be unmarked by difference unchanged. Still, it matters less where or how we live differently among ourselves (even if it is not perfectly voluntary) if we think of inclusion in terms of process rather than the integration of spaces, institutions, or neighborhoods or the assimilation of cultural difference. Separateness itself is less the problem than that democratic processes do not take existing structural differences into account. That would mean shifting the focus away from the excluded and toward the processes that privilege certain social perspectives and modes of expression.

Young offers the ideal of "differentiated solidarity" as an alternative to the ideals of integration and cultural assimilation as a means of balancing inclusion with the realities of a divided world. She develops the conceptual tools to counterbalance the dangers of involuntary exclusion against the autonomy of groups and persuasively uses social structural difference as a means of mitigating debates over identity and difference. Less persuasively, she suggests that difference can be transformed from an obstacle to a resource in democratic deliberations. Consistent with the critical traditions that she invokes, Young favors ideals for their critical potential.

Viewed procedurally, democracy is above all the best means we have for collective problem-solving because it allows us to approximate the ideals of inclusion, justice, and equality. Young makes a sound case against orienting deliberations toward the common good. Whether the starting point or the conclusion of our deliberations, unity excludes those who do not share our perspectives and understandings. We also exclude the possibility that democratic engagement will transform us or our interests and perspectives on the common good. There is one serious omission in this collection of essays. If we accept a procedural and minimalist theory of democracy as a means of collective problem-solving, we will need to know how a problem gets defined as a collective one. How will we come to accept, say, the problem of racial segregation as *our* problem rather than *their* problem? Young's answer is caught up in solving the problem of inclusive political communication and does not directly address how the agenda is set for collective deliberation. This is an old problem in democratic theory but one that continues to nag the critical tradition in particular.

Young can be charged with having too many things both ways. For example, she criticizes the literatures on civil society for imposing exclusionary norms of civility upon politics. In defending unruly demonstrations and boisterous modes of expression and debate, Young unmasks the biases and exclusions that norms of deliberation impose. At the same time, she proposes that one of the most important powers of otherwise powerless groups is to shame the powerful into recognizing their claims to justice. How can we deliver both a politics of shame and a politics that does not impose cultural norms upon others? Distinctions between shameful and inclusive actions are not easy to sustain beyond examples like

the WTO and anarchists. The focus here on procedure solves some problems and, of course, generates others.

Young takes up the crucial question of jurisdiction introduced in her earlier work. Our jurisdictions should extend as far as our obligations of justice to accommodate our relations to any others affected by our decisions. Citing Onora O'Neill, Young argues that our obligations of justice follow a causal chain that extends globally. She couples that argument with one for "relational autonomy." Procedurally, autonomy means noninterference, while relationally a group must remain accountable to the interests and perspectives of others. In addition to the problem of scale, how can we be both autonomous and accountable in any meaningful way? How can democracy accommodate both autonomous local institutions and global obligations of justice? It is her style to mitigate, to find a way of posing the question so that we might have it both ways. It is persuasive and simultaneously inconclusive.

As the ideals pile upon one another in these essays, the reader might get a sense that we are getting too far away from the real world of democracy. Her ideal of a reasonable citizen seems pretty far-fetched in the real world of debate TV and partisan politics: A reasonable citizen is one who is open to having their views and perspectives change in the

process of deliberation. How many citizens do you know who are rational by this measure, who go into a political debate with the expectation that their opinions, their perspectives, and their interests will be transformed? But Young's conception of deliberation, like her conception of democracy, is minimalist. If we do not expect to undergo transformation, we cannot be engaged in authentically democratic deliberation. By treating democracy minimally and procedurally as a means of collective problem-solving, Young does in fact achieve a balance between the real and the ideal that stands out brilliantly in the field of normative democratic theory. Her voice, like her arguments, is temperate but never tepid. However subtle, there is a potent set of arguments here that will draw the attention of those who favor agonistic theories of democracy back to normative theory. It will bring those who favor radical democracy back to exiting conditions and the shortage of negative liberty. Proponents of civil society are reminded that the state is the only institution we have for coordinating public expenditures. All in all, these essays will be of critical interest to everyone working in democratic theory and especially to those ready to grapple with the paradox of her opening line: "Democracy is hard to love" (p. 16).

American Politics

Crossing Borders, Crossing Boundaries: The Role of Scientists in the U.S. Acid Rain Debate. By Leslie R. Alm. Westport, CT: Praeger. 2000. 160p. \$58.00.

William R. Mangun, *East Carolina University*

Leslie Alm presents what may be the best study yet produced on the acid rain policy debate, at least with regard to its scientific underpinnings. The book describes the evolution of the current U.S.-Canadian acid rain policy agreement and focuses on the role of scientists in the formulation and implementation of acid rain policy. Alm found that most natural scientists believe they have little influence on the policy process. He suggests they are not trained to understand policymakers, and policymakers are not trained to understand science. Both have a narrow focus that causes them to perceive selectively what the other is saying. As does Lynton K. Caldwell (*Between Two Worlds: Science, the Environmental Movement, and Policy Choice*, 1990), Alm informs us that scientists and policymakers operate in two totally different worlds using two different languages and two different time scales; this complicates the policy process.

Crossing Borders, Crossing Boundaries is a most important book for both natural scientists and political scientists interested in how science affects public policy. This book is particularly important for scientists who are even remotely interested in becoming involved in the shaping and analysis of public policy. They will gain insight into a totally new and strange world of politics, in which perception is equally or more important than factual reality, almost totally unlike the world of science, where hard facts rule. Alm does a good job of selecting important quotations from congressional hearings that point out to scientists that they, like Dorothy and Toto, are no longer in Kansas but in a totally different and unknown world. He shows how policymakers who attempt to make scientists appear to be advocates of positions the policymakers favor can manipulate "scientific research." The book also does a good job of informing scientists of what to

expect if asked to testify before Congress and, accordingly, how to prepare testimony. The author demonstrates how science must be made policy relevant.

Students of the policy process will acquire considerable insight into the complex world of scientific issues as well as many of the difficulties inherent in the translation of scientific results into public policy. They will learn that scientists seldom are willing to provide conclusive "cause-and-effect" evidence that will support a particular proposal. Alm suggests that scientists who become substantially involved in promoting a specific policy do so at the risk of endangering their professional reputation. Such concerns encourage many scientists to be conservative in their statements, because they realize that new scientific evidence may be just around the corner that could substantially alter the basis of their research findings. Such conservatism often frustrates policy entrepreneurs trying to sell a specific policy solution. "Policy makers are selective, predisposed, and want certainty and scientists cannot give them that" (p. 92).

Alm employs John Kingdon's policy agenda setting framework to describe the role of scientists in acid rain politics. For example, Alm goes to considerable lengths to describe the central role that scientists play in establishing a consensus about the need for policy action on environmental problems, in general, and acid rain in particular. Similar to Kingdon, Alm shows how scientific experts have helped shape the alternative policy courses in the acid rain debate.

Alm's research spans more than ten years. During this time, he scrutinized public hearings on both sides of the U.S.-Canadian border in order to obtain detailed information about which scientists were called upon to participate in acid rain deliberations. He also conducted numerous interviews with American and Canadian natural and social scientists. His attention to detail provides considerable insight into the complex interaction between science and politics in public policy formulation.

Alm depicts what happens when the highly complex world of science collides with political realities. His interviews with scientists highlight necessary ingredients for successful collaboration with policymakers. Neither group has an accurate

perception of the world in which the other must operate. Scientists do not appear to realize that compromises must be made among competing interests, and policymakers do not seem to realize or want to acknowledge that science-based policies simply have to operate with a high degree of uncertainty.

This book also has strengths with regard to insights into comparative politics. Alm perceptively identifies both political and scientific aspects, shaped by science, that continually loom over the delicate webs of cross-national policy linkages. He points out that natural and social scientists in the United States and Canada have different perceptions of their roles in the policy process. He also suggests that such differences may influence the policy position of the two governments, which potentially threatens the very foundations of transnational agreements. Alm made extensive efforts to address both Canadian and U.S. positions (scientifically and politically), and those interested in obtaining a better understanding of the intricacies of cross-national policy formulation and implementation should read this book. Despite a somewhat narrow focus on acid rain policy, this volume is useful as a case-study text for courses in environmental politics, international environmental policy, and science, technology, and public policy.

Judicial Politics in the D.C. Circuit Court. By Christopher P. Banks. Baltimore, MD: Johns Hopkins University Press, 1999. 200p. \$38.00.

Isaac Unah, *University of North Carolina at Chapel Hill*

For decades the U.S. courts of appeals were afflicted with the proverbial middle child syndrome. They were given less than deserved attention by legal scholars and political scientists, and their decisions commanded less media and popular attention than rulings by the Supreme Court and even decisions of federal district courts. To many observers, the appeals courts were relatively invisible. Circuit courts are convalescing from this affliction because increasingly political scientists are turning their analytical attention to these thirteen important stations of judicial power in American society.

Scholars have used a variety of techniques to analyze these courts. Some choose to examine the courts of appeals comparatively, by emphasizing differences in institutional structure, decision outcomes, and judicial personalities. Others perform a detailed and intuitive case study of only one. This latter technique may limit generalizability, but it gives political scientists and, especially, students of the judiciary a useful sense of the historicity and doctrinal contributions of individual courts. Christopher Banks has written an important book that falls within this second category.

Judicial Politics in the D.C. Circuit Court contributes to the broader judicial politics literature by providing a detailed historical account of the major political and jurisprudential developments in the Court of Appeals for the District of Columbia Circuit after 1960. According to Banks, the post-1960 period was one of significant transformation for the D.C. Circuit. It emerged from relative obscurity to become a dominant political force not only in supervising local community courts and liberalizing the rights of criminal defendants in the Washington, D.C., area but also in superintending the policy decisions of federal administrative agencies and in serving as what is putatively the most fertile ground for the cultivation of prospective nominees for the Supreme Court.

The book is divided into five chapters. Chapter 1 chronicles the D.C. Circuit's great authority in the judicial hierarchy. Chapters 2 and 3 discuss the internal and external political forces that have shaped the current status of the D.C. Circuit.

Chapter 4 discusses the significance of en banc reviews and the degree of scrutiny afforded by the Supreme Court over circuit decisions. Chapter 5 summarizes the main argument and speculates on the D.C. Circuit's future. From the outset Banks notes accurately that, with few exceptions, precious little has been written about this court, even though it occupies a central place in the federal judicial system.

Part of the difficulty is the lack of detailed data (beyond the descriptive material available from the Administrative Office of the Courts [AOC] that would permit rigorous empirical analysis. The recently completed court of appeals database compiled by Donald Songer (Principal Investigator) begins to address this difficulty. Indeed, analyses using AOC data are by necessity historical and descriptive rather than deductive and theoretical. Banks relies upon descriptive data from this federal agency in his coverage of the D.C. Circuit and all the numbered circuits, and these data enable him to emphasize the distinctive features of the D.C. Circuit. Yet, like many scholars who write about the courts of appeals, Banks could not obtain comparative data on the Court of Appeals for the Federal Circuit, the only circuit court with limited subject-matter jurisdiction and the court most similar to the D.C. Circuit in jurisdiction and geographic location.

Banks does not explicate and test a new theory of judicial behavior. Instead, he seeks to write a descriptive book about an important circuit court, and he succeeds. Banks relies upon a critical evaluation of various theories of judicial behavior, especially the hierarchical model (chap. 4) proposed by Donald Songer, Jeffrey Segal, and Charles Cameron ("The Hierarchy of Justice: Testing a Principal-Agent Model of Supreme Court-Circuit Court Interactions," *American Journal of Political Science* 38:3 [August 1994]: 673-96), to analyze events within the D.C. Circuit from 1960 to 1997.

The main argument is that, since the 1960s, the D.C. Circuit has had two distinct reputations in the legal community, and each influences the court's work. The first was developed during the 1960s, when the D.C. Circuit positioned itself as a progressive forum for adjudicating criminal disputes. Because of escalating crime in American society and the social upheaval of that period, the circuit was vilified for its progressive or lenient stance on the rights of criminal defendants. Congressional Republicans championed wholesale changes in the court's personnel and jurisdiction in an effort to fight crime and restore public order.

The second reputation is that of a quasi-specialized, de facto court for adjudicating administrative appeals, a direct result of the transformation of the court's personnel and jurisdiction. In chapter 3, Banks describes how political forces both inside and outside the D.C. Circuit influenced the court's decision making in administrative law, including differences in orientation among the judges, which led to the development of the popular "hard-look" doctrine that expanded the scope of judicial supervision of administrative agency decisions. In examining both reputations, Banks helps us understand how powerfully Washington politics permeates the judicial system.

Toward the end of the book, Banks addresses the politically controversial issue of subject-matter specialization in the courts. Political elites are sharply divided on the propriety of establishing specialized tribunals staffed with experts to adjudicate legal controversies in a specific policy area. Banks spells out the pros and cons in this debate. Although he seems tepid on this issue at first, his preference is clearly pronounced by the end. The D.C. Circuit is traditionally considered a generalist court, but since the 1970s the court has decided the greatest proportion of administrative agency appeals directly involving the federal government. Therefore, Banks advocates congressional reform to restrict its substantive

jurisdiction to cases from administrative agencies because "the D.C. Circuit operates best as a specialized court" (p. 136). This would turn the D.C. Circuit into a specialized court of administrative law. But Banks is extremely cautious and calls repeatedly for more studies of the issue before any reforms are implemented. This caution undermines the force of his own evidence showing that the D.C. Circuit is the most active tribunal in deciding administrative appeals and that this makes it appropriate for the D.C. Circuit to be formalized as a specialized court.

Legislative Learning: The 104th Republican Freshmen in the House. By Timothy J. Barnett. New York: Garland, 1998. 333p. \$60.00

Gregory R. Thorson, *University of Minnesota at Morris*

The 1994 elections were watershed elections in several respects. Perhaps most significantly, the Republicans gained control of both chambers of Congress for the first time since 1953. In the process, the 1994 elections also swept into office a large, relatively homogeneous group of 73 House Republican freshmen determined to change the political system. With size comes an opportunity for power in Congress, and the 73 Republican freshmen elected in 1994 were determined to exert considerable influence over the legislative process. Not since the 1974 elections produced the 76 freshmen Democrats (i.e., the Watergate Babies) have political scientists focused so much attention on a single class of legislators. Timothy Barnett's *Legislative Learning* is a nice complement to similar books already written about this interesting group of legislators, including Richard Fenno and Michael Armacost's *Learning to Govern: An Institutional View of the 104th Congress* (1997) and Nicol Rae's *Conservative Reformers: The Republican Freshmen and the Lessons of the 104th Congress* (1998).

Whereas Fenno and Armacost focus on the inexperience and failures of the Republican leadership to develop this new group of legislators adequately, and Rae shares his "insider" experience as an APSA Congressional fellow during this period, Barnett's primary contribution is finding the theoretical significance to these important historical events. In this respect, he is quite successful. His treatment of the rise and subsequent fall of the Republican freshmen class of 1994 is put into a rich theoretical context that includes extensive discussions of Fenno's multiple goal approach, ambition theory, and Sinclair's application of principal-agent theory.

Most of Barnett's arguments do not defy previous interpretations of the events that took place during this period. For example, most political scientists have long argued that the goals of the Republican freshmen class of 1994 were more policy-oriented than reelection-oriented. Barnett's treatment is dedicated not so much to determining the factual basis of these claims but to examining their theoretical relevance. His focus is on to what extent the behavior of the Republican freshmen class of 1994 was consistent or inconsistent with other members and whether any differences are supportive of certain theoretical perspective more than others found in the literature. For example, Barnett's treatment of the tension between Mayhew's reelection goal perspective and Fenno's multiple goal approach is both provocative and insightful.

The evidence used in Barnett's book is largely anecdotal and is derived mostly from secondary sources. Nevertheless, Barnett uses these sources skillfully. For example, in some of his most interesting observations, Barnett describes in great detail the difficulty the House Republican leadership had

in controlling the Republican Class of 1994 during the passage of special rules governing debate for committee funding of the House Oversight Committee and the flood relief bills passed in 1997. His coverage of these events leads to broader discussions of the motivations and goals of the class generally.

Barnett also introduces a limited amount of original data and presents the results of several personal interviews as well. His survey data are limited to just 10 questions answered by members' high-level legislative staffers. The survey had several measures aimed at determining the cohesion of the class as well as its perceived mandate. Not surprisingly, Barnett finds that members of the 1994 Republican Freshmen class had a high level of class cohesion and the perception of a reform-oriented mandate.

Overall, Barnett's *Legislative Learning* is a well-done theoretical volume that attempts to understand the Republican Freshmen Class of 1994 through competing interpretations of principal-agent analysis and member goal and ambition theories. Scholars hoping to find specific hypotheses developed and tested with empirical data will be disappointed. Nevertheless, due to its focus on the theoretical implications of this important class of legislators, I would recommend it to supplement the Fenno, Armacost, and Rae texts.

Presidential Transitions: From Politics to Practice. By John P. Burke. Boulder, CO: Lynne Rienner, 2000. 437p. \$65.00.

Mary Anne Borrelli, *Connecticut College*

Scholars have long been participant-observers in presidential transitions, which provide extraordinary opportunities to refine their understanding of continuity and change in the presidency. Political science, more generally, has benefited from this engagement. Whether we read the elegant memoranda of Richard Neustadt or the advice-laden briefing papers of the White House 2001 Project, we gain a new appreciation for the challenges confronting a chief executive who must build an administration, establish a policy agenda, and achieve demonstrable political results within a few short months of the election. In *Presidential Transitions, From Politics to Practice*, John Burke makes a notable contribution to our knowledge of these events, both as historical drama and as political developments. Quite simply, this book can be enjoyed for its narrative and appreciated for its analysis by those interested in presidential institutions and policymaking.

Burke focuses on the decision making processes that are established during the preinaugural weeks and tested during the postinaugural months. He gives due attention to the cabinet and to various units within the Executive Office, but his attention is fixed on the White House staff. He readily acknowledges that the White House office is an institution, which is to say that it evidences autonomy, adaptability, complexity, and coherence. Yet, Burke insists, it is remarkably flexible and even malleable. Presidents have conceptualized and managed the personnel and units within the White House office in very different ways, even in the most recent decades, which has significant implications for both executive and (by extension) legislative decision making. "The way in which transitions craft, or do not craft, those processes has consequences, sometimes severe" (p. 377).

The transitions of four presidents—Carter, Reagan, George H.W. Bush, and Clinton—are assessed from their preelection planning efforts through to their postinaugural outcomes. The work of presidents-elect is analyzed in terms of the transition organization, nomination and appointment decisions, and the development of a policy agenda; that of

the presidents is examined in terms of decision making processes, presidential management of the White House staff, and domestic and foreign policy outcomes. Bracketing these discussions are chapters that consider the role of transitions in the modern presidency, the limitations of rational actor models in explicating this aspect of the presidency, and the lessons to be learned from the four administrations. As this array of topics suggests, Burke is as attentive to the concerns of practitioners as he is to the interests of scholars.

What distinguishes this work is an extensive reliance on (and cross-referencing of) primary documents and sources. The collections of the presidential libraries and other archives, as well as extended interviews with the principals, reveal the course of managerial and policy decisions. Without venturing into counterhistorical speculations, Burke weighs the importance of roads taken and not taken, including the implications of each for White House operations. Although the Clinton chapters necessarily rely more on secondary sources, they offer original data and insights. Burke explains this book's relationship to other works on presidential campaigning and governance, and it will undoubtedly serve as a springboard for future studies.

In essence, Burke's analysis can be distilled into a forceful assertion that the president is at the center of the presidency—notwithstanding the influential roles played by various other actors and offices—and that each president's managerial choices are important. Transitions matter, then, because they reveal a president-elect's organizational priorities vis-à-vis the White House, as distinct from the campaign or from prior executive positions. Although attuned to patterns of political behavior, Burke is sensitive to the distinctive character of each president, which makes his rejection of rational choice models, for example, entirely comprehensible. It also increases the likelihood that this volume will serve as a lightning rod for debates about research design and methods in presidency studies.

Martha Kumar, in her written and oral presentations as director of the White House 2001 Project, stresses four basic components in setting up the White House office: organization, process, personnel, and values. Burke, who contributed an essay to that project, considers each of these elements in *Presidential Transitions*, but he places greatest emphasis on organization and process. Who holds which position in the White House is important, but Burke maintains that their choices and actions occur within an organizational and procedural context. Values certainly matter, but he suggests that they are most routinely manifested through relationships and decision making. Thus, an investigation of personnel and values is part of the larger study of organization and process. Each is integral to the White House office, but Burke assigns a particular primacy to organization and process. This claim challenges the presumptions of many organizational theorists, as well as presidency and policy scholars, and fuels debate about the White House staff as a bureaucracy and as an institution. In turn, the book's empirical richness, in conjunction with its theoretical contentiousness, ensures that it will be a valuable resource for political scientists as researchers and as educators.

Maximization, Whatever the Cost: Race, Redistricting, and the Department of Justice. By Maurice T. Cunningham. Westport, CT: Praeger, 2001. 192p. \$62.50.

Mark E. Rush, *Washington and Lee University*

With the Supreme Court decision in *Easley v. Cromartie*, the 1990s round of redistricting litigation came to an end just

in time for the new decade's round to commence. Looking back upon the Court decisions from the 1990s as well as the extensive scholarly commentary on them, anyone new to the field of voting rights in the United States might wonder how the Voting Rights Act, which was a straightforward attempt to remedy indisputably unjust political practices, could have given rise to the I-85 district in North Carolina.

The key, says Maurice Cunningham, is the evolution of the strategy taken by the Voting Rights Section of the Civil Rights Division (CRD) of the Department of Justice to implement the Voting Rights Act (VRA). Cunningham approaches the furor surrounding minority voting rights as a disinterested observer who laments the loss of prestige and credibility (p. 10) that the CRD has sustained as a result of its actions under the VRA in the 1990s. Under the leadership of John Dunne and Deval Patrick, the CRD gradually yet unquestionably shifted its approach to maximizing the number of majority minority districts from a policy that balanced competing concerns about minority representation with state-level concerns about incumbency, different aspects of local representation, and so forth. To recapture its prestige and credibility—and therefore reestablish its capacity to protect effectively minority voting rights—the CRD must "recapture the balance missing from its effort in the 1990s" (p. 10).

Maximization, Whatever the Cost is a refreshing addition to the VRA literature because it is not a piece of advocacy. Instead of criticizing the stands taken by supporters and critics of the CRD and the Supreme Court in the 1990s, Cunningham demonstrates that the CRD is subject to the same pathologies and interest group pressures that affect any other bureaucratic arm of government. The result is an intriguing study of the tensions within the CRD and the role and influence of the interest groups that lobby it.

Cunningham's book is bound to upset many participants in the voting rights policy arena precisely because the analysis is so evenhanded. Minority voting rights advocates are bound to chafe at the contention that the CRD's maximization policy was not in keeping with the spirit of the VRA. Similarly, the author notes that critics of the CRD's maximization policy also suffered from tunnel vision. They were justified in at first fearing and then fighting against the maximization policy, but it is also clear that their very narrow interpretation of the VRA would hardly have served the purposes for which the act was intended.

In large part, the CRD suffers because it is charged by the VRA with two contradictory roles. On the one hand, Section 2 empowers the CRD to assist minority plaintiffs in vote dilution suits. On the other, Section 5 charges it with an adjudicatory role in assessing changes to state election laws. Cunningham argues that in the 1990s the CRD subsumed the latter role in pursuit of the former.

The CRD dispensed with the first prong of the Section 2 vote dilution test set forth by the Supreme Court in *Thornburg v. Gingles* (1986) (which required that a minority group be geographically compact) in favor of a standard that defined "discriminatory intent" on the part of a legislature as the adoption of any other districting scheme when one that provides for more majority-minority districts is available. The CRD then incorporated this interpretation of Section 2 into its Section 5 preclearance procedures. The result was a policy that withheld preclearance unless a state could demonstrate that it had indeed maximized opportunities for minority representation.

Cunningham demonstrates the effect of interest groups on the CRD's policymaking. It is no surprise that members of what he calls the voting rights issue network (the

ACLU, NAACP Legal Defense Fund, Mexican American Legal Defense Fund the Lawyers' Committee, and so on) lined up behind and worked closely with the CRD and its staff attorneys in advocating the policy of maximization. The intimacy of this relationship ultimately did the most damage to the CRD's integrity because it appeared as if the division had allowed itself to be captured by interest groups. This was manifested most clearly in the criticisms leveled at the CRD in the *Johnson v. Miller* (1995) litigation emanating from Georgia. Of course, the newcomer to the field will wonder what the Republican Party was doing right there, working alongside all these groups that would seem to be its traditional adversaries. Cunningham indicates that it made sense for the GOP to pursue and advocate the maximization policy (despite its philosophical opposition to affirmative action) because it enhanced opportunities to elect Republicans.

Ironically, the CRD's focus on maximization not only blinded it to the counterproductive effect it was having on minority interests (by helping the GOP) but also rendered it deaf to the pleas of black elected officials. They were willing to "settle" for minority influence districts precisely because they had gained positions of influence and established themselves as effective politicians in a manner clearly envisioned by the framers of the VRA.

Cunningham's study reveals several different fates that legislation can suffer at the hands of interest groups. The right thing can be pursued the wrong way (enhancing minority political opportunities via a maximization policy). The wrong (or at least unintended) thing can happen the right way (using the VRA to enhance Republican interests at the expense of Democratic power and therefore minority interests). Finally, the right thing can be done the right way (implement the VRA in a manner that balances theoretical ideals of minority voting rights with the concerns of local—including minority—elected officials).

Perhaps the most poignant discussion concerns the tension manifested by the second scenario. "Elite" actors, such as career voting rights attorneys, found their theoretical yet abstract vision of justice at odds with the interests of the minority elected officials (and, presumably, their constituents) for whom they were supposed to be working. Cunningham notes (pp. 100–1) that members of the voting rights issues network became so preoccupied with achieving their maximization goal that they overlooked the fact that the VRA was designed to enhance and repair the democratic process in general.

This slender volume merits much praise. My only criticism is the wish that it were a bit longer. Cunningham peppers his discussion with excerpts from and references to interviews with key players, and it is extensively footnoted with references to respected academic sources and state actors. But at points I found myself wishing for more detail and embellishment, such as Dunne's reconciliation of his maximization policy with his membership in a Republican administration, or the discussion of how the state legislators finally caved into CRD pressure to maximize the number of majority-minority districts. The account of how black elected officials were accused of selling out because they had problems creating bizarre districts, such as the I-85 district in North Carolina, is gripping—and makes the reader wish for even more detail.

Maximization, Whatever the Cost is a breath of fresh air and a succinct inquiry into the politics and implementation of the Voting Rights Act. It puts the many works of advocacy that characterize the voting rights literature into a thoughtful political science perspective.

The Prudential Presidency: An Aristotelian Approach to Presidential Leadership. By Ethan M. Fishman. Westport, CT: Praeger, 2001. 160p. \$52.50.

Michael J. Korzi, *Towson University*

A sense of puzzlement is likely to be the first reaction to Ethan Fishman's book. Aristotle and presidential leadership? These are topics that we tend not to associate with each other. Yet, as Fishman makes clear in this admirable and provocative work, Aristotle had much to say about proper political leadership, much that is directly applicable to evaluating and understanding American presidents and their acts of leadership.

Fishman is very clear about what he wants to accomplish, and his book is accordingly brief and tightly argued. It is not meant as an exhaustive theoretical account of Aristotle or as a detailed examination of presidential leadership acts. Rather, Fishman intends to "provide a straightforward application of an ancient strategy for effective leadership to information presently available in a language and style accessible to general readers and students in courses on the American presidency and the history of political philosophy" (p. 13). He begins with a theoretical chapter that explicates Aristotle's theory of prudential leadership and discusses three other "competing leadership strategies that [Aristotle's] theory implies—idealism, cynicism, and pragmatism" (p. 6). With this theoretical foundation in place, the bulk of the book constitutes an application of these concepts to particular acts of presidential leadership. A concluding chapter considers further the implications of Aristotle's theory of prudential leadership for presidential studies in particular and presidential leadership more generally.

There are several audiences for this book—general readers as well as students and scholars of political philosophy, the presidency, and leadership studies—but most especially presidency scholars. Fishman is rightly critical of the lack of normative analysis in presidency studies and, indeed, political science in general. Presidency scholars (Fishman singles out for special consideration Richard Neustadt, James Barber, and George Edwards) often do yeoman work in explaining the causes and effects of presidential behavior, but they are typically reticent, or perhaps evasive, when it comes to normatively judging that behavior. Presidents are labeled "good" if they are energetic, or persuasive, or successful in passing legislation. But political scientists rarely question whether the decisions presidents make, or the policies they pursue, are moral or laudable or, to use a rather unfashionable word, "virtuous." By returning to Aristotle and his theory of prudential leadership, Fishman tries to develop a language and model by which to make normative and evaluative sense of the American presidency and presidential leadership. "Presidents serve the public interest of the United States most effectively when their behavior conforms to Aristotle's venerable, but no longer generally venerated, theory of political prudence" (p. 6).

Fishman largely succeeds in giving us a language for making evaluative judgments. The leadership categories of prudence, pragmatism, idealism, and cynicism turn out to be very useful in explaining and interpreting leadership acts of American presidents. For example, idealism is an accurate characterization of Woodrow Wilson's stubborn leadership on the Treaty of Versailles, and cynical leadership best characterizes Bill Clinton's actions during the Lewinsky scandal. Of course, prudence is the key model of leadership, around which the others revolve.

Prudential leadership occurs when leaders "emphasize universal ideals without romanticizing them and do their best to honor values in the face of practical obstacles" (pp. 22–3).

This is to be distinguished from pragmatic leadership, which is often conflated with prudential leadership, that is, “values are secondary to tangible results” (p. 22). Prudential decisions in American history are Lincoln’s leadership during the Civil War and Theodore Roosevelt’s support of conservation; pragmatic decisions include Truman’s use of the atomic bomb and Kennedy’s leadership during the Bay of Pigs. Although “the difference between prudence and pragmatism is especially difficult to appreciate” (p. 25), Fishman takes us quite a way toward clarifying the distinction. Undoubtedly, some will quibble with Fishman’s interpretation and characterization of certain acts of leadership, but in the main they ring true and help us better understand the range and scope of presidential leadership.

One of the more interesting and provocative contributions of the book is an understanding of the relationship between private character and public action. In light of the Lewinsky scandal, which was a partial motivator for the book (p. 14), this question takes on immediate relevance. Clinton and his defenders argued for a strict separation between private behavior and character and presidential leadership. In fact, some used Clinton to prove that there is no relationship: To wit, Clinton was an outstanding president who had a flawed or immoral private character. Aristotle and Fishman clearly disagree with this viewpoint. Aristotle rhetorically asked: “Is it not true that men who have no command of their passions will fail to serve their own interest even though they possess self-knowledge and self-loyalty and will equally fail to serve the public interest (even though they possess a knowledge of public affairs and public loyalty)” (p. 31)? Whereas Stanley Renshon (*High Hopes*, 1996) gave us a psychological explanation of the connections between Clinton’s private and public behavior, Fishman provides a more philosophical perspective, one that is sure to provoke further debate over the legacy and leadership of Clinton.

Two aspects of the book need refinement and development. First, because the pursuit of “universal ideals” is a hallmark of prudential leadership, one would like to see a more systematic discussion of what might constitute universal ideals and values. There are oblique references to such standards as the Ten Commandments and the Golden Rule (p. 112), but a more substantial discussion is warranted. Second, and more important, Fishman’s model of leadership places somewhat unreasonable expectations on presidents. “The less we expect of our presidents, the less we are likely to get from them” (p. 119). This may be true, but we need to be more discriminating about exactly when and under what circumstances we should expect great leadership of the Aristotelian, prudential stripe. What if the context and times do not permit prudential leadership?

Fishman confronts Stephen Skowronek’s *The Politics Presidents Make* (1993) in the last pages of the concluding chapter, but he does not fully address the powerful challenge that Skowronek’s arguments pose. Skowronek’s seminal study suggests the very real limitations of expecting great acts of leadership from all presidents, regardless of circumstances or their place in “political time.” Surely, we should criticize presidents for squandering opportunities for great acts of moral leadership, but we also should be sure that they did, in fact, have the reasonable opportunity to exercise such leadership. Thus, Fishman makes an important break with the presidency literature in his focus on normative analysis, but his individual-centered leadership perspective, being insufficiently attentive to context and institutions, is too conventional.

Nevertheless, this book is a welcome addition to the presidency (and political science) literature. Fishman has given us a language for better describing and evaluating presidential

leadership, and in the process he demonstrates the continuing relevance of political philosophy and normative analysis to contemporary politics and political science. These are no small achievements.

The Power of the American Presidency, 1789–2000. By

Michael A. Genovese. Oxford: Oxford University Press, 2000. 273p. \$19.95.

Presidential Power: Forging the Presidency for the Twenty-First Century. Edited by Robert Y. Shapiro, Martha Joynt Kumar, and Lawrence R. Jacobs. New York: Columbia University Press, 2000. 525p. \$49.50

Richard S. Conley, *University of Florida*

It has been more than forty years since the publication of Richard Neustadt’s *Presidential Power and the Modern Presidents* (1960). In that seminal work Neustadt rebuffed systemic, legal, and constitutional approaches to emphasize the personal basis of presidential power and the centrality of presidents’ reputation and persuasive skills. Michael Genovese’s book and the collection assembled by Shapiro, Kumar, and Jacobs are timely and useful additions to the reevaluation of the individual and institutional bases of presidential power, influence, and leadership across time. If scholarship on the presidency is at a crossroads, these works invite us to journey in different analytical directions.

Genovese joins Sidney Milkis and Michael Nelson (*The American Presidency: Origins and Development*, 1998) in producing an authoritative and informative account of the historical development of presidential power. The work focuses on addressing a central problem that teacher-scholars know all too well: “Students today are a-historical; they know virtually nothing about any president who came before George Bush” (p. ix). The writing is accessible to an undergraduate and general audience. The author also engages readers with attention to the personas and sometimes humorous peculiarities of past presidents, including the enigmatic Madison and the vibrant Jackson, as he astutely situates individual presidents’ short- and long-term contributions to the office.

The book is organized chronologically, and Genovese does not endeavor to forge a comprehensive theory of presidential power. He does, however, draw useful, if intuitive, distinctions between the formal (constitutional) and informal (extraconstitutional) foundations of presidential influence. Borrowing from his previous work (with Thomas Cronin, *The Paradoxes of the American Presidency*, 1997), Genovese plays upon the contradictory leadership expectations and constitutional ambiguities that presidents have faced since the beginning of the Republic. These include the systemic checks and balances aimed at reigning in presidential power and the wording of Article II, which provides the basis both for restrictive and expansive interpretations of presidential authority by Congress, the Court, and the electorate.

The evolution of the presidency, Genovese posits, is “a story of elasticity and adaptability; of leadership and clerkship; of strong and weak officeholders; of change and stasis” (p. 16). The themes accentuated throughout the brief studies of individual presidencies—the pivotal role of precedents, foreign policy actions, popular politics, struggles with Congress, changing economic and international contexts—emphasize broad historical cycles of rising and declining presidential influence. Genovese’s central objective is to highlight the ways in which the office has been transformed across time without any fundamental restructuring of the Constitution. He notes that when presidential power has surged forward, the “great” and “near great” presidents found themselves in challenging governing positions. The common threads that link such icons

as Washington, Jackson, Lincoln, Theodore Roosevelt, and Franklin Roosevelt include skill and the selfless pursuit of a higher purpose in the bid to expand presidential power. It is the combination of individual political acumen, governing context, and the demands upon the presidency relative to the larger American political system to which Genovese fittingly credits the adaptability of the institution to changing domestic and international circumstances across time.

Historians may find one of the book's main virtues, brevity, to be a liability. Because Genovese cannot cover the intricacies of each administration he is compelled to set forth the high and low points and the major historical developments succinctly. An ambitious "political time" framework à la Stephen Skowronek (*The Politics Presidents Make*, 1997) is not fully developed, although useful "periodic" distinctions are drawn. Genovese skillfully crafts a thoughtful, well-documented volume that will surely be appreciated in undergraduate courses on the presidency and political development.

Genovese's concise analysis of turning points in the modern era (since Franklin Roosevelt) and the challenges recent presidents have faced, particularly with respect to economic and domestic policy and the end of the Cold War, is well balanced. Above all, the concluding chapter is insightful and bound to spark lively classroom debates. By underscoring the ebb and flow of presidential influence over three centuries, Genovese reminds us that those elements of our "leadership adverse," separated system that simultaneously sanction and constrict executive power—and are often blamed for gridlock—are not wholly unique to the modern presidency. The continuity that connects the "traditional" and "modern" presidency is the enduring search for equilibrium comprising bold yet democratically accountable leadership.

What of modern presidents' attempts to surmount systemic obstacles and leave legacies commensurate with the "great" presidents of times passed? Should Neustadt's focus on personal power continue to orient presidency scholars today? *Presidential Power: Forging the Presidency for the Twenty-First Century* is a superlative collection of articles that critically examine Neustadt's influential legacy. Shapiro, Kumar, and Jacobs have produced a volume that rivals Michael Nelson's sixth edition of *The Presidency and the Political System* (2000) in theoretical scope and surpasses it in sophisticated empirical data analysis. Their volume is appropriate for advanced undergraduates and should be required reading for graduate courses on the modern presidency.

Derived from a Columbia University conference in 1996, the essays in *Presidential Power* take up a range of issues pertaining to Neustadt's power approach. The authors test key hypotheses, raise new questions, and challenge core assumptions. The book is organized into six parts, and a senior scholar provides an overview and commentary on the essays at the beginning of each section. Chapters by George Edwards and John Gunnell place Neustadt's approach within traditions of American political science. The second section covers issues of personal power—bargaining with Congress, the timing of presidential speechmaking, and advisory structures. The third and fourth parts focus on the institutionalization of the presidency and the position of the executive relative to other systemic actors. The fifth section discusses presidential polling and media communication. The concluding chapters consider the effect of Clinton's proposed impeachment as well as a somewhat plaintive essay by Neustadt himself on the future of presidency research.

If the essays strike a melancholic chord with Neustadt, it is because the objectives of the authors tend to be at odds with the original purpose of his classic study. Whereas that work was meant as a primer for officeholders on the practice

of personal power, these essays are heavily weighted toward rational choice assumptions and the "new institutionalism." They focus less on individual skill and are far more concerned with concrete theoretical development in presidential scholarship. As Jacobs and Shapiro note in the concluding chapter, Neustadt's work "no longer offers a reliable roadmap embodying the consensus among contemporary scholars" (p. 489). Many of the contributors reject "Neustadt's emphasis on the personal basis of power in favor of an analysis of institutions and political structures" (p. 491). At the same time, some of the underlying themes of Neustadt's classic work, including "institutional partisanship" (the view that presidents should prevail over other systemic actors), presidents' relationship with the public, and reputational issues, are prevalent in the essays.

Neustadt is critiqued from a variety of angles, and some disconfirming evidence is presented alongside traces of his well-established insight on presidential power. Several authors take issue with Neustadt's choice to hold the institutional context of the presidency constant. Charles Cameron, for instance, considers how party control of Congress shifts legislative leadership from "coordination" under unified government to "strategic bargaining" through the veto power under divided government. Others, like Nolan McCarty and Rose Razaghian, suggest that presidents engage in more "strategic anticipation" than the bargaining model proffered by Neustadt. As they attempt to control the bureaucracy through appointments, presidents may create the false impression of a "honeymoon" early in their term by saving controversial appointments until later. Others, such as Bert Rockman, are skeptical of Neustadt's organizational prescriptions for the presidency. Neustadt touts Franklin Roosevelt's "multiple advocacy" approach as the key to success, yet Reagan, for example, was largely successful (at least in his first term) through a far more structured organization. Renée Smith finds that presidents can act as "teachers" through "self-made happenings," but they do not always maximize the potential of "prestige" with the public. Public approval does not appear to drive decisions on the timing of presidential speeches.

Embedded in the collection of articles are other considerable insights, controversial theses, and sophisticated methods to which a brief review cannot do justice. Some of the articles draw data from archival material (Matthew Dickson and Diane Heith); Robert Lieberman compares Neustadt's approach to Skowronek's "regimes" in political time; and Congress-centered arguments about legislative delegation to the president are presented (David Epstein and Sharyn O'Halloran). All told, the collection accentuates how the new wave of research on the presidency focuses on the institutional and structural constraints on presidential power and the executive's direct connection to public opinion. This approach is at variance with Neustadt's emphasis on the personal basis of presidential power and "insider" reputation.

Not all scholars will agree with the general perspective of *Presidential Power*. The "psychological" presidency (e.g., James David Barber, *The Presidential Character* 1992) and interconnecting issues of personality, leadership style, and presidential politics (Fred I. Greenstein, *The Presidential Difference*, 2000; Alexander L. George & Juliette L. George *Presidential Personality and Performance*, 1998; Erwin C. Hargrove, *Presidential Leadership*, 1966) are deemphasized. Nonetheless, the critiques of Neustadt are a tribute to his pioneering work in charting the course for presidency scholarship. The essays also reflect the theoretical maturation of the subfield in recent decades as scholars continue to map out new directions in presidency research.

Not Whether But When: The U.S. Decision to Enlarge NATO.

By James M. Goldgeier. Washington, D.C.: Brookings Institution Press, 1999. 218p. \$42.95 cloth, \$18.95 paper.

Sharyl Cross, *San Jose State University*

James M. Goldgeier makes a major contribution to the contemporary case study literature concerning American foreign policy formation. Based on extensive interviews with more than 75 key participants (William Perry, Richard Holbrooke, John Shalikashvili, Leon Panetta, Anthony Lake, Strobe Talbott, and so forth), Goldgeier reconstructs a richly detailed account of the policy process that culminated in the decision to expand the NATO alliance eastward. The study illuminates the complex interplay of political considerations, bureaucratic interests, and individual preferences and skills (even chutzpah) that led to the admission of the first tier of new NATO member nations in Eastern/Central Europe.

No less a figure than George F. Kennan, principal architect of America's post-World War II containment strategy, described the decision to expand the alliance as "the most fateful error of American policy in the entire post-Cold War era" (George F. Kennan, "A Fateful Error," *New York Times*, February 5, 1997, p. A23). Many scholars recognized the potential adverse consequences for relations with postcommunist Russia, the economic costs of admitting new members, questions concerning the potential military-to-military interoperability of Eastern/Central European nations with NATO members, and many other considerations, and they were at a loss to explain the administration's decision to pursue what appeared as a less than optimal policy choice. Goldgeier's balanced and objective treatment of this contentious issue makes clear the confluence of considerations—right or wrong—that resulted in a 1998 Senate vote (80–19) in support of NATO enlargement.

In this concise (six chapters) and well-written study, Goldgeier carefully reconstructs the range of variables that led the Department of State on a fast-track trajectory in support of enlargement, with the primary objective of creating an incentive for democratic transition in the newly independent states of Europe. Based on the interviews with General John Shalikashvili (Supreme Allied Commander in Europe SACEUR), Charles W. Freeman, Jr. (Assistant Secretary of Defense for Regional Security Affairs), and other U.S. officials, Goldgeier provides detailed evidence to suggest that the Pentagon was reluctant to embrace enlargement; it favored the Partnership for Peace initiative as a first step toward constructing the preconditions for eventually integration of new members into the alliance. The Pentagon also appears to have assigned a higher priority to limiting the potential for renewed confrontation with Russia. Beyond defining the core distinctions between the Department of State and the Pentagon, the analysis expands to evaluate the weight of Congress, Russian political realities, U.S. public indifference, and interest groups composed of U.S. citizens of Eastern/Central European descent in the formation of policy.

Perhaps of most interest is Goldgeier's attention to the significance of key individuals in securing U.S. commitment to NATO enlargement. "Lake and the president laid out the vision between September 1993 and July 1994, and Holbrooke had the 'chutzpah' to move the policy forward in the fall of 1994" (p. 11). The author concludes: "My numerous interviews with enlargement opponents within the administration and with enlargement proponents outside of it have created the overwhelming recollection that in October 1994 Assistant Secretary of State for European and Canadian Affairs Richard C. Holbrooke pushed forward an enlargement policy that was no more than theory to that point" (p. 152).

Goldgeier persuasively establishes the significant influences exerted by National Security Advisor Anthony Lake and, even more important, Assistant Secretary of State Richard Holbrooke in securing U.S. backing for NATO enlargement. Goldgeier observes that ambiguity in presidential decision making creates opportunities for policy entrepreneurs (in this case, especially Lake and Holbrooke) to strike decisively in advancing their preferences. The case study reveals how personality, skills, persistence, and conviction of the single policy entrepreneur can be directed toward "outmaneuvering" the bureaucracy to achieve desired objectives.

According to Goldgeier, Holbrooke served the role of enforcer, taking Clinton's words in support of NATO expansion to the implementation phase. Among several illustrations, the author carefully recounts a meeting in September 1994, shortly after Holbrooke's return to Washington as Assistant Secretary of State for European Affairs; it involved more than thirty officials, and Holbrooke abrasively and astutely set the process in motion. Goldgeier includes General Wesley Clarke's recollection of this first interagency meeting called by Holbrooke: "Kruzel spoke first, since he was the policy guy, and said, 'Why is this policy? It's supposed to be an interagency process.' Holbrooke crushed him like a bug. He said, 'It is policy.' Ash Carter walked out of the room" (p. 74).

Goldgeier's study contributes to discounting some of the simplistic explanations put forward to account for NATO enlargement, such as the claim that Clinton favored the initiative only to gain the Polish vote. Clearly, the decision was far more complex, encompassing a range of individual actors and institutional interests.

Yet, despite the painstaking research effort, there are still some unanswered questions. Perhaps most significant, the factors that led Clinton to support expansion of the alliance are not entirely clear. According to Goldgeier's account, the visits of Polish and Czech presidents Lech Walesa and Vaclav Havel with Clinton, in conjunction with the opening of the United States Holocaust Memorial Museum in 1993, involved "emotional appeals" for support of NATO expansion that made a significant and lasting impression (pp. 20, 165). But did these appeals prove decisive in persuading the president to favor this policy course? Are there equally plausible alternative explanations? How were Clinton's views shaped by discussions with the policy entrepreneurs. Boris Yeltsin's admonitions concerning enlargement, or strategic considerations?

Clinton's speeches indicate that he desired dual objectives: support of NATO enlargement while simultaneously keeping Russian democratization and the favorable climate in U.S.-Russian relations on track. Goldgeier makes an interesting point advanced by psychologists: "When faced with the problem of competing objectives, individuals often convince themselves that there is no contradiction, that they can, in the words of one senior official discussing the two tracks of this policy, 'walk and chew gum at the same time'" (p. 160). Perhaps this explanation is sufficient. In the end, however, without conclusive evidence regarding the shaping of Clinton's views and motives, one critical area of the case study remains at least partially speculative. Future revelations in interviews with the former president or the release of his memoirs could yield additional material that will be useful in completing this analysis.

The study have been enhanced by devoting greater attention to the influence of the European dimension in the formulation of policy. No one can dispute that to "understand the development of NATO enlargement requires understanding the interplay of process, politics, and policy in Washington,

D.C." (p. 5). Europeans and other observers might take issue, however, with the claim that "all of the key decisions were made in Washington" (p. 5). In fact, concerns were raised throughout the enlargement debate about securing the approval of the 16 NATO members and their respective legislative bodies. Even if the "whether" and "when" were ultimately answered in Washington, expansion was contingent on complex political processes within other alliance nations. Furthermore, it would be interesting to ascertain the extent to which Clinton's position—or that of his key advisors—were influenced by European perspectives or considerations.

Goldgeier offers interesting insights concerning theoretical models. For example: "A classical model of bureaucratic politics tells us that where individuals stand depends on where they sit, and that how they perceive a situation also depends on their bureaucratic role. The NATO enlargement policy process suggests that which bureaucratic position is crucial for understanding the orientation of officials at lower levels, the higher up you go, the more longstanding beliefs rather than a particular job title prove critical in many cases" (p. 156). Goldgeier insightfully observes that support of enlargement from Madeleine Albright, Lynn Davis, Anthony Lake, and others resulted from assigning the highest priority to promoting democratization in Eastern and Central Europe, and the positions of these officials derived from individual preferences, values, or personal and professional experiences even more so than any view that might have been predicted as a result of institutional affiliations at the Department of State or elsewhere.

The author challenges some of the central theoretical arguments in the literature, but core questions and conclusions advanced in this study remain quite fundamental: "How do policies typically develop within the United States government?" (p. 5) "The development of the U.S. initiative to enlarge NATO reminds us that the decision process is often just that—a process—rather than a specific moment" (p. 152). Much that is revealed in this study is likely to be of greater interest to university students seeking to understand the making of American foreign policy than to seasoned specialists, who might expect major new theoretical contributions.

This book should be a priority for those interested in a detailed, focused case study that reconstructs the complex array of factors in a post-Cold War U.S. foreign policy decision likely to have significant consequences for generations to come.

Interest Groups, Lobbying, and Participation in America. By Kenneth M. Goldstein. Cambridge: Cambridge University Press, 1999. 170p. \$49.95 cloth, \$16.95 paper.

Beth L. Leech, *Rutgers University*

Studies of interest-group influence on politics and studies of mass participation in politics typically have been quite separate undertakings. With a few notable exceptions, research projects have been designed to examine one or the other, not both, and the influence of interest-group activity on mass political activity is too seldom considered. In this broadly integrative book, Kenneth Goldstein makes a convincing argument for why this should not be the case.

Goldstein's central argument about mass participation in public policy issues is that little about it is spontaneous. For example, since 1970 the number of citizens who contact their members of Congress by letter and phone has nearly doubled. This increase is remarkable in that it took place during a period in which we have seen a sharp *decline* in voter turnout in congressional elections. Goldstein argues that while more citizens than ever are calling and writing their members of

Congress, they are doing so as part of orchestrated efforts by elites, by organizers who facilitate the participation of those outside the beltway.

Goldstein's argument about mass participation parallels an argument made by Stephen Rosenstone and John Mark Hansen in their 1991 book, *Mobilization, Participation, and Democracy in America*. In that book they argued that declines in voter turnout resulted at least in part from declines in party mobilizing efforts. They, too, see mass participation as linked to efforts by elites. Goldstein extends this argument and shows that it holds true for other types of political participation, such as letter-writing and contacting government officials.

The book also contains an important argument about the nature of lobbying strategies. Goldstein argues that the main goal of an outside lobbying campaign is to create the "traceability" that Douglas Arnold wrote about in *The Logic of Congressional Action* (1992). A traceable issue, according to Arnold, is one that is salient, has perceptible consequences, and is linked to an identifiable government action by a legislator. An outside lobbying campaign, Goldstein argues, creates that traceability, but the grassroots message also may be used to give cues to potential challengers, signal information about the direction and intensity of constituency opinion, and frame an issue.

That is a lot of different things for a message to communicate, and the complexity of both this and Goldstein's other theoretical models provides both the strength and the weakness of his book. Lobbying decisions and lobbying strategies are complicated. Lobbyists use multiple tactics, undertaken with multiple goals in mind. Goldstein acknowledges this and eschews any attempt to simplify the process into a caricature.

Two types of evidence support the arguments made in the book. On the interest-group side, there are interviews with 41 interest-group representatives regarding their organizations' activities in 94 grassroots lobbying campaigns. On the individual side, there are survey data from participation and recruitment questions in the Battleground poll of 1994, conducted jointly by Democratic and Republican polling firms. Data from these polls, supplemented by National Election Study and Times Mirror survey data, allow Goldstein to test the effects of recruitment contacts and to investigate who is being recruited.

Goldstein begins by developing a model of how grassroots lobbying decisions are made: when to target, where to target, whom to target, how to target. The fundamental assumption in this model is that not all grassroots lobbying contacts are equal. Some constituents will be more likely to respond to mobilization efforts than others and the opinions of some types of constituents will matter more to the legislator than others. Recruitment choices, according to Goldstein's model, will be driven in part by the desire to lower the participation costs for the citizens most likely to influence the legislative outcome. His cases support this prediction. For example, groups opposing the Clinton health-care plan mobilized constituents in the districts of members of the five committees that were considering the Clinton bill. Even within those committees, however, grassroots efforts were not evenly distributed, but were focused more strongly on members of committees where the vote was closer.

His theory predicts and his data show that when the lobbying objective is a short-term legislative goal, interest groups will target districts represented by undecided voters and influential legislators. The aim is to provide these legislators with information about the possible negative electoral consequences should they vote contrary to the sentiments voiced in the grassroots campaign. However, when the interest-group goal is more long-term, the objective in the grassroots

campaigns shifts to providing information to voters and potential challengers in districts where there are legislative opponents who are judged to be vulnerable.

Goldstein notes that some scholars have concluded that campaigns like the "Harry and Louise" ads opposing the Clinton health-care plan were not successful because surveys at the time found little national recall of the ads. National recall is a poor indicator of success, however, because the ads were targeted only at specific districts, not the nation as a whole. He quotes a lobbyist: "We were trying to move congressional votes, not Gallup numbers. We didn't need to convince all Americans or all congressmen, we just had to convince the ones that mattered" (p. 77).

This is not a soothing book. A trade association executive is quoted: "Grass roots mobilization is used for one purpose, period—to influence legislative policy. It's not about getting more Americans involved. It's not about educating people on the issues. It's not about making Americans feel good about their political system" (p. 125). Individuals are targeted for mobilization in part based on the likelihood that they will participate if mobilized. Thus, mobilization campaigns tend to exacerbate, rather than mitigate, existing biases in representation.

Goldstein does not suggest that the interest groups are at fault here, that grassroots campaigns should be abolished, or that scholars who worry about the dangers of too much mass participation are correct. Rather, he points out that the reason stimulated participation is so effective is because mass participation rates are low. Low turnout rates make legislators pay particular attention to information about the types of constituencies that are likely to vote and less attention to those that are not likely to vote. This makes grassroots campaigns with electoral objectives easier to achieve, since narrow constituencies can have a large influence in low-turnout contests. Therefore, one solution is to work to increase voter participation, thereby minimizing the unrepresentative effects of the grassroots campaigns. An additional reform suggested by Goldstein is to require interest groups to report the money they spend on grassroots efforts. Such reporting currently is required for expenditures on direct lobbying and campaign contributions, but not for grassroots campaigns.

This book provides a richly theoretical description of lobbying and participation that crosses subfield boundaries and makes crucial connections between political science theory and the realities of political life. We do not walk away from this book with a one-sentence jingle that explains the causes and effects of outside lobbying campaigns but, instead, with a clearer understanding of the linkages between interest organizations and other political institutions.

Flag Burning and Free Speech: The Case of *Texas v. Johnson*.

By Robert Justin Goldstein. Lawrence: University Press of Kansas, 2000. 269p. \$35.00 cloth, \$14.95 paper.

Scot Guenter, *San Jose State University*

This is another in the Landmark Law Cases and American Society series from Kansas, joining such topics as the Salem witchcraft trials, *Marbury v. Madison*, the Pullman case, and *Bakke*. The series editors have chosen wisely in making this selection, both in topic and author. Goldstein is the nation's leading expert on legislation and judicial review related to flag desecration. He brings to this project considerable knowledge, prolific publications in this area, and a clarity of voice and focus. There are no provocative and startling new approaches or theories, but that is not the purpose. I highly recommend this book as assigned coursework for college seminars on civil religion, the separation of powers doctrine,

First Amendment rights, Supreme Court functioning, or the interplay between politics, the media, and the Constitution. It will work well in classes not only in political science but also in intellectual and cultural history, American studies, philosophy, and communication studies.

Aware that subjectivity is inevitable in an analysis of such a controversial and often emotional concern, Goldstein openly shares his stance in the Preface: "I believe that although flag desecration is usually a completely counterproductive activity that primarily alienates people, it nonetheless is a form of political expression and therefore must be constitutionally protected in a society that claims freedom of expression as its philosophical touchstone" (p. xv). He summarizes the three basic arguments in favor of criminalizing flag desecration, then ponders scenarios when it might serve "socially useful purposes" (p. xviii). Goldstein is precise in establishing chronologies of events and carefully selects quotations to let the adherents of varying positions speak for themselves.

The text is organized into nine chapters. The first is the best brief overview I have yet seen on the early cultural history of the American flag, and it emphasizes the beginning of what Goldstein terms the "Flag Protection Movement" (FPM) around 1890. Chapter 2 is a succinct yet comprehensive summation of flag desecration laws and prosecutions in the period 1897–1980. Getting to the main focus of the book, chapter 3 offers a longer, more detailed consideration of the Texas trials of Gregory Lee Johnson for his infamous flagburning protest outside the 1984 Republican National Convention in Dallas, which renominated Ronald Reagan for president. Next, there are chapters on the Supreme Court handling of and ruling in that case, the media and political backlash that followed, and a comparatively lengthy appraisal of the congressional debate provoked by that backlash, which ultimately led to the Flag Protection Act (FPA) of 1989. The last one-third of the book turns to how the FPA was tested in the courts from October 1989 through May 1990, the Supreme Court response in *Eichman* in June 1990, and the ongoing and unresolved struggle during the 1990s over a constitutional amendment to outlaw flag desecration.

I particularly enjoyed the way Goldstein elaborates on the workings of the Supreme Court in chapter 4, including private musings of the justices and procedural insights gleaned from clerk interviews. He also brings to life some of the more dramatic personages involved in the chain of events, such as the revolutionary Gregory Lee Johnson, the dynamic lawyer William Kunstler, or the meticulous solicitor general destined for later fame, Kenneth Starr. Two other features accentuate this work's usefulness as a college text: a handy chronology of flag desecration law and a superb bibliographic essay that points to the best reference sources should students wish to follow up on different aspects of this topic.

The American flag is the most sacred symbol in our civil religion. Historically, in times of economic and political crisis or great cultural dislocation, forces in society have emphasized cohesion through enforced demonstrations of nationalistic commitment. This is not going to change, although the specific rituals, rules, and ceremonies may. But we also have a long (and to my mind, admirable) tradition of honoring, protecting, and even expanding our interpretation of First Amendment rights. The tension of these coexisting truths has been at the core of much of America's historical experience, and the fact that this issue is not going away is what makes this book not only timely but also significant for undergraduate reading and reflection.

George Bush was in the White House in June 1990, when the Supreme Court, in a 5–4 ruling, upheld the right to express symbolic political dissent by burning a flag. He immediately

renewed the call for a constitutional amendment to correct what he felt was an error on the part of the Court. That drive, of course, did not succeed, although it came very close. In 2002, in large part due to a 5–4 ruling by another Supreme Court (with two members appointed during his father's presidency), another George Bush sits in the White House, and many of his political views and practices are committed to fulfilling the earlier agendas and aims of his father. We live in a time when close Court decisions are overturning or re-examining issues of privacy, the separation of church and state, and the relationship between federal and state authority. Goldstein convincingly explains why the movement for a constitutional amendment against flag desecration diminished in effectiveness from 1995 to 2000, but he ends the book by reaffirming how divided our nation is on this issue and how it will inevitably resurface in legal and political battles.

For the most scholarly and thoroughly documented analysis of *Texas v. Johnson* or for a wide range of primary sources to assess, look to earlier books written or edited by Goldstein (*Burning the Flag: The Great 1989–90 American Flag Desecration Controversy*, 1996; *Desecrating the American Flag: Key Documents of the Controversy from the Civil War to 1995*, 1996). For a handy, accessible text appropriate for general readership, and faithful to Goldstein's well-established scrupulous attention to all his sources, this is the book for you.

Nonprofits in Urban America. Edited by Richard C. Hula and Cynthia Jackson-Elmoore. Westport, CT: Quorum Books, 2000. 235p. \$65.00.

Peter J. Haas, *San Jose State University*

This volume purports to explore questions about the changing role of nonprofit organizations in contemporary urban America. The stated theme is to explain how and why such organizations "attempt to sculpt the landscape of urban policy and political action rather than simply react or adapt to it" (p. 1), although the nine articles do not embrace it with equal rigor. The editors, who also contribute a chapter, further state that the text will explore "when and where such efforts are effective and when and where they are not" (p. 1). These are timely and important issues, given the Bush administration's stated goal of increasing reliance on nonprofit organizations.

The essays are extremely uneven, both in terms of their ability to deliver on the editors' vision and in their overall quality. The highlight of this nevertheless worthwhile volume is the synthesis in the first chapter by Steve Rathgeb Smith. He does an admirable job of weaving a comprehensive literature review on the evolution of scholarly thought about the origins and roles of nonprofits into the more specific concerns of the articles in this volume. His contribution deftly identifies the lacunae in existing theory, which tends to overemphasize the role nonprofits can play in enhancing social welfare, and points to more explicitly politically significant functions that nonprofits are fulfilling in urban areas. He seeks to show how the other contributions to this volume exemplify various aspects of this more political purpose for nonprofits.

Although generally trenchant, Smith's essay creates something of a straw man argument that is repeated several times throughout the book. Smith maintains that Lester Salamon's oft-cited "partnership" theory of nonprofit organizations cannot account for the emergence of more politically motivated nonprofits. Yet, few readers will be surprised to learn that some nonprofits may frequently take a more independent and even antagonistic stance toward the public and private sectors. It is true that Salamon's theory emphasizes the economic logic behind nonprofit creation, but it seems unenlightening

if not unfair to discredit it on the basis of what may well be a limited number of exceptions that are the focus of this text.

There are many inadequacies in most of the subsequent chapters, which for the most part focus on a particular community or type of nonprofit organization or both. A common foible is that the cases tend to underplay severely potential concerns about method. Beyond a brief article based on a limited survey of nonprofit directors, the essays are only loosely anchored in any kind or rigorous approach to data collection and analysis. Contributors Joseph Cordes, Jeffrey Henig, and Eric Twombly, for example, make the rather bold assertion that increasing "privatization" of nonprofits may risk "further depletion of the role of purposive mission," converting them into "more material-based organizations" (p. 59). Yet, they offer only anecdotal examples of this sort of conversion, which leads me to wonder whether and to what extent it is really occurring.

Todd Swanstrom and Julia Koschinsky argue that community-based housing organizations (CBOs) have the primary goal of "addressing place-based inequalities . . . that limit the ability of citizens to realize their full potential as active economic, social, and political beings" (p. 75). They find that contemporary CBOs are instead emphasizing "service delivery and real estate development" (p. 85). Once again, however, the evidence is at best sketchy, and the reader cannot judge the validity of the authors' analysis, including the extent to which CBOs actually embrace the authors' notions about their goals. One does not expect definitive data in a volume of essentially case studies, but the contributors collectively ignore such concerns.

Several articles freely substitute normative assertions for more objective analysis. A common undercurrent is the idea that nonprofit organizations should be vehicles of political change, rather than merely alternate modes for urban service delivery. Several authors suggest (as do Swanstrom and Koschinsky) that cutbacks in public funding have forced nonprofits to "sell out." If that is indeed the case, they need to consider to what extent, if any, the clients of these organizations would be better served by more focus on political expression and less on the delivery of tangible goods and services.

Some of the articles seem to be too sanguine about the effect of political efforts by nonprofits. In their essay, editors Hula and Jackson-Elmoore describe the development of "governing nonprofits" in Detroit. These are defined as nonprofits for which "targeted empowerment" of otherwise disenfranchised or alienated groups is a "core organizational goal" (p. 130). They strongly suggest that such groups have succeeded in increasing the representation of African Americans and, therefore, the city's "civic capacity" to address social and economic issues. They fail to consider that this development occurred during a period when Detroit lost a significant portion of its white population to suburban areas, which made increased minority representation almost inevitable, and they do not demonstrate that African Americans are now any better off than they would have been absent these organizations. Similarly, Marion Orr extols the "crucial role" of Baltimoreans United in Leadership Development (BUILD), citing a few limited accomplishments, such as the city's support for a "living wage" that was essentially ignored by corporate employers. I wonder whether and to what extent the political presence of BUILD is actually improving the lives of poorer residents.

In sum, *Nonprofits in Urban America* is an often provocative but equally disappointing foray into the political aspects of American nonprofits. Too frequently, potentially significant questions are posed (or implied) but left unanswered or are answered with inadequate documentation. Nevertheless,

the volume offers a useful descriptive survey of the range of political dimension of nonprofit organizations, particularly for those unfamiliar with this terrain.

Faithful and Fearless: Moving Feminist Protest Inside the Church and Military. By Mary Fainsod Katzenstein.

Princeton, NJ: Princeton University Press, 1998. 270p. \$40.00 cloth, \$18.95 paper.

Laura R. Woliver, *University of South Carolina*

The power of social movements to transform what we take for granted and what we contest is beautifully displayed in *Faithful and Fearless*. Mary Fainsod Katzenstein shows how some protest in American society has moved inside institutions. Particularly, she means, feminist protest: "Less lawbreaking than norm-breaking, these feminists have challenged, discomfited, and provoked, unleashing a wholesale disturbance of long-settled assumptions, rules, and practices.... Sometimes by their mere presence, but more often by claiming specific rights, and by demanding in certain facets the transformation of the institutions of which they are a part, feminists have reinvented the protests of the 1960s inside the institutional mainstream of the 1990s" (p. 7).

The study takes as its starting point the fact that what used to be seen as outlandish has become commonplace in our society. The book offers an account of how feminist activists have worked to effect change in core institutions of American life. Katzenstein's fieldwork and interviews focus on the institutions of the United States military and the American Catholic Church. From 1988 to 1997 she interviewed about 120 individuals active in women-centered reform efforts within these two institutions. Her findings are compelling for many reasons. One is that the two institutions she studied represent the best test cases for her thesis. If change is occurring within the Catholic Church and the military because of feminist/womanist pressures (two of our stodgiest, thickly moss-backed, phallocentric institutions), then indeed the "sisterhood is powerful." Different tributaries flow from a common feminist source and set in motion social changes.

She studies the feminists, mostly women, not in the public eye who throughout the last three decades (1960–1990s) have challenged in their everyday lives the institutions in which they work and live. The women engage in unobtrusive mobilization within these patriarchal edifices. These very same institutions, in turn, have a power of their own to shape differences in contemporary feminism. For example, military feminists have a strong belief in equal rights and equal opportunities. Their strategy is simply to have existing laws implemented within the military. The military women are liberal feminists, practicing interest-group feminism, whereas the Catholic feminists are more radical. They want to transform cultures, institutions, and society. They directly confront and contest poverty, homophobia, racism, war, and violence. The feminists within the Catholic Church in America are activists for radical equality. This Catholic feminist protest is a more radical discursive politics. Discursive politics, Katzenstein explains, is the politics of meaning making (p. 17). The American Catholic feminists utilize language, cognition, books, and conferences to process and articulate their vision of a just world which includes a feminist worldview. Their meetings, networks, prayer groups, conferences (indeed, confessional conferencing), and reports constitute discursive activism. They have, therefore, a difference in perspective. They form organizational habitats (protected spaces) within the larger institutions. Within the Church, their enclave becomes like a "women-church;" it is not removed from the male bastions of power but seriously engaged as a dissenting, discursive

voice. Like the women in the military, the women-church activists use the ethics and rhetoric of the institution to force the institution to abide by its own promises and principles. The Leadership Conference of Women Religious, for instance, works to revision women in the Church and the Church's role in social justice.

Radicalism in the Church is shaped by the lack of legal redress since the Church is a private and religious organization. These feminist radicals in the Catholic church are seeking an understanding of the structural or systemic bases of inequality in the church and in society. They are not like Mother Teresa of Calcutta, nursing the poor and ameliorating their pain and suffering. Rather, they focus on "the identifying and rooting out of the very *systems* that cause the poor to be poor, the homeless to be homeless, and that cause people to die of poverty or oppression" (p. 21).

Katzenstein's research displays the multilevel significance of the law for women who seek equality within institutions. The legalization of claims making in American politics can assist in the institutionalization of feminist protest. She also explores the meaning of protesting from inside institutions. Katzenstein cautions us against the view that inside activism signals the end of the challenges that movement politics initiates.

For women within the military, Katzenstein explains the complicated and contradictory role of the law. She writes, "The law's role is also a normative one, shaping the way activists come to define themselves, see the world around them, and prioritize their agendas. For how can we explain otherwise the fact that activists seize some opportunities but not others?" (p. 165). The military women are feminists by any other name. They do not directly claim the mantle of feminism, yet all their beliefs, positions, justifications, and orientations are feminist.

Katzenstein shows how these women must be *Faithful and Fearless*: "Given the previously rigid gender-cast system in both military and church, and given the continued risks to career, status, and respect that those who challenge gender ascription incur, such women must be fearless to be faithful to their institutions on feminist terms" (p. 164). Cultures agree on what requires debate and what does not (p. 35). Feminist issues now require debate, and this is great progress. Backlashes against feminism, then, occur because of feminist progress. Katzenstein deftly proves that by demanding a co-equal place inside *male-dominant institutions*, feminists have transported protest into mainstream institutions and have changed institutional and social givens.

Katzenstein's study is sure to be a classic in social movement, feminist, religious, and democratic theory. It is richly deserving of all the awards it has already received (the Marion Irish Award of the American Political Science Association for the best book in women and politics, for instance) and will receive in the future.

Environmental Injustice in the United States: Myths and Realities. By James P. Lester, David W. Allen, and Kelly M. Hill. Boulder, CO: Westview, 2000. 216p. \$19.00.

Michael E. Kraft, *University of Wisconsin-Green Bay*

Over the past decade the study of environmental justice has sparked considerable debate, with conflict often exacerbated by conceptual and definitional muddles, scarcity of pertinent data, and disagreement over which methods to employ. This book by the late James Lester and his colleagues is unlikely to diminish the controversy. Yet, as a comprehensive attempt to clear out the conceptual underbrush and bring hard data to bear on difficult empirical questions, the book merits

attention by all those concerned with issues of environmental equity. The preliminary findings have been presented at conferences from 1994 on, and many are already familiar with this research. As the authors note, however, they began with a skeptical posture toward the strident assertions made regarding environmental justice. In the end, they reconsidered that position and found much evidence to support concern over environmental inequities in U.S. society.

Environmental justice issues rose rapidly on the policy agenda during the 1990s, prompted by numerous but flawed studies that suggested a disproportionate effect of toxic chemicals and hazardous wastes on low-income and minority communities. Those findings were reinforced by political pressures from environmental justice groups to take corrective action to address what was often termed environmental racism. Horror stories of communities that suffered from high levels of exposure to air and water pollution and hazardous waste sites made the argument both graphic and compelling.

Political scientists entered the fray to try to identify the key empirical questions and to bring appropriate data to bear. Attention has focused on class and race inequities in exposure to environmental risks (and the presumed health consequences), largely from living in close proximity to hazardous waste treatment, storage, and disposal facilities. As scholars such as Evan Ringquist have noted so well, the methodological challenges faced in trying to address such concerns are formidable.

For example, we have good data on the location of polluting facilities, but living near such a facility is not the same as exposure to its toxic emissions. We also have fairly accurate measures of major chemical emissions by industries, courtesy of the U.S. Toxics Release Inventory (TRI), from which we infer human exposure. Further complicating the situation is the circumstantial evidence that links toxic chemical exposure to health effects, which may appear decades later.

The challenge for scholars has been to unravel this complex situation, largely by examining the relationship between chemical releases and the demographic characteristics of exposed populations. Even when statistical relationships of this kind are found, they may not tell us why environmental inequities occur and what might be done through public policies to reduce them.

Lester, Allen, and Hill offer a multilevel analysis of these relationships at different periods using multiple dependent variables. They examine the contiguous 48 states, the 2,080 U.S. counties for which TRI data are available, and the 410–414 U.S. cities with more than 50,000 residents where they could use TRI data. They employ seven measures of environmental harm to broaden the picture, including TRI chemical releases, air pollution levels, and Superfund sites. They focus on four categories of possible explanations for environmental inequities: race and ethnicity, class, political mobilization, and exogenous variables, such as pollution severity, political culture, government fiscal capacity, business climate, and the strength of environmental interest groups.

The argument is presented in ten relatively brief but densely written chapters, accompanied by 13 figures and 28 tables. The early chapters examine the nature of the environmental justice problem, review the literature (with particular attention to conceptual and methodological issues), and provide an elaborate history of the environmental justice movement.

The heart of the book is a well-developed model of environmental justice that seeks to integrate the variables noted above; three chapters that separately examine environmental injustice at the state, county, and city level; and a summary of the findings and conclusions. The authors find no relationship between political mobilization and environmental injustice,

mixed results for social class, and strong evidence of race-based injustice. Most of the additional explanations examined here, other than the severity of pollution, also yield mixed results. Two final chapters deal with policy implications. The authors review federal and state policy on environmental justice, noting in particular the impressive variation across states, and they systematically examine alternative policy designs based on the criteria of rationality, equity, and efficiency.

Readers may quibble with the selection of particular methods and in some ways question the writing style. For example, why focus on states, counties, and cities but not, as some scholars do, on finer levels of analysis, such as ZIP codes or Census tracks? The review literature tends to be rather heavy on citations (15 or 20 for some sentences) but light on a summary or assessment of what other scholars have done. This may not matter for those already familiar with the literature, but it will be difficult for undergraduates, and even many graduate students, to follow the authors' reasoning in these sections.

Similarly, the authors argue that case studies are an unreliable way to study environmental justice and that only national data of the kind they use can permit generalization. But case studies have some redeeming virtues. They often do a better job of conveying the real human burden created by environmental injustice as well as the political, economic, and social constraints that affect efforts to change the situation. The addition of some case studies might have complemented the statistical analysis.

The two policy-oriented chapters will be of special interest to many. After a detailed and finely structured policy analysis, the authors conclude that a risk-based solution that reflects the severity of pollution problems best meets their criteria. It can be justified in terms of rationality, efficiency in the use of scarce societal resources, and equity for all concerned. This conclusion squares well with arguments of environmental policy scholars over the past decade about the need to set priorities based on such risk levels. Yet, it is at odds with the views advanced by leaders of the environmental justice movement, who tend to favor race- or class-based solutions.

Lester, Allen, and Hill have much to say about how to study environmental justice and how to deal with the inherent conceptual and methodological complexities. Their analysis offers valuable insights into the multiple causes of environmental injustice and what might be done to address it. As a research report that seeks mainly to advance knowledge and further the debate over environmental equity, the volume will be of greatest interest to other scholars in the subfield and to graduate students in environmental politics and policy.

Putting Trust in the U.S. Budget: Federal Trust Funds and the Politics of Commitment. By Eric M. Patashnik. Cambridge: Cambridge University Press, 2000. 231p. \$54.95 cloth, \$19.95 paper.

Daniel J. Palazzolo, *University of Richmond*

Nearly four decades ago, in a path-breaking book, Aaron Wildavsky taught us, among other things, that "in the most integral sense the budget lies at the heart of the political process" (*The Politics of the Budgetary Process*, 1964, p. 5). Building on Wildavsky's study, many scholars have contributed to our understanding of various aspects of budget policy and politics. Some have discussed the role of trust funds in the federal budget, but none have made them the central focus of their research. Eric Patashnik fills an important void in the literature by explaining why trust funds are created and how they affect deliberation over tax and spending decisions. Using the trust fund mechanism as a point of reference, Patashnik also tests theories of policy history, public choice, and institutionalism.

Aside from a chapter that contains a regression model to test the response of trust fund and general revenues to political and institutional factors, the empirical focus is on case histories of five trust funds—Social Security, Hospital Insurance, Highway, Airport and Airways, and the Superfund. Also examined are the failed attempts to create trust funds for energy security and lead paint abatement. The regression analysis shows that political forces have differential effects on trust fund revenues and general revenues. This chapter is somewhat unrelated to the case histories, which are more central to the book's main objectives, that is, to explain the origin, evolution, and consequences of trust funds. Using the historical-institutionalist method, Patashnik draws out the variations among trust funds, the common traits, and the nuances in their design and evolution.

Patashnik's central argument is that trust funds are intentionally designed policy instruments that seek to provide durability and reduce uncertainty over spending and revenue decisions. They are created when program advocates convince others that establishing a long-term commitment to a program is better than maintaining the flexibility to make ongoing policy adjustments. Among the four distinct rationales for trust funds (making users pay, maximizing budgets, reducing uncertainty, and guarding the Treasury), reduced political uncertainty is found to be most important in all the cases (p. 189). Program advocates for each trust fund demonstrated the value of locking in policy commitments and the need to bind future policymakers. Conversely, in the energy security and lead paint abatement cases, advocates were unable to demonstrate that a trust fund was needed to make the programs work or that a long-term commitment was preferable to preserving flexibility.

By dedicating revenues to a specific purpose for both present needs and future contingencies, trust funds create an implicit contract and define the terms for subsequent policy debates. In this respect, they are similar to entitlements and indexation because they contain procedural safeguards, have greater autonomy than routine programs, and provoke symbolic sensitivities that limit the flexibility of policymakers.

Patashnik deftly points out, however, that although trust funds constrain future choices, the programs financed by them are neither insulated from politics nor impervious to change. The programs are typically financed on a pay-as-you-go basis, and they are challenged by threats of insolvency, new ideas, pressures from interest groups, or changes in institutional procedures. The durability of trust funds rests on their capacity to serve a mediating role in policy debates. "Trust fund financing clearly does not eliminate the impact of other political forces, such as struggles among contending groups. But trust funds do influence how those struggles are played out by distributing procedural advantages, reinforcing symbols of moral deservedness and blame, and affecting perceptions of political fidelity and defection" (p. 16).

Patashnik also describes the variation in the design features, autonomy, and durability of trust funds. Social Security is the most durable because individual beneficiaries rely heavily on the government to make good on the program's promise. The Hospital Insurance Trust Fund for Medicare, which is as politically potent as Social Security, is less stable partly because it is more reliant on general funds. The Highway Trust Fund's durability relies on the support of a nexus of producer groups, government officials, and motorists, but it has a low degree of individual beneficiary reliance and periodically has been challenged by budget guardians, environmentalists, and urban mass transit interests.

Despite the variation among trust funds, Patashnik identifies four common attributes that distinguish successful adoption: a base of support for the expenditure program,

plausibility in terms of a precedent and an appropriate revenue source, a demonstration that trust fund financing is necessary to make the program work, and neutralization of opposition to the idea (pp. 183–6). Trust funds often are supported both by program advocates, who want the program protected from the vicissitudes of the annual budget process, and by fiscal conservatives, who view the mechanism as a means of cost control.

Patashnik's insights into the politics of trust fund budgeting contribute significant new knowledge, but the theoretical aspects of the book should not be overlooked. The author effectively points out the shortcomings of the policy inheritance perspective, which views policymakers as severely constrained by their predecessors, and rational choice theory, which argues that institutional structure dictates policy. Patashnik shows that institutional arrangements mediate rather than cause policy outcomes; that policymaking is an evolutionary process and programs are responsive to historical nuances and acts of individual policymakers; and that ideas, not just interests, are critical forces in the resolution of policy conflicts.

In terms of the design and durability of trust funds, Patashnik finds that both competing theories of political transaction costs are incomplete. One of these contends that institutions are efficient and inherent to government, and the other maintains that costs are inflated and contrived to serve the narrow interests of politicians. Either theory might lead us to expect that more trust funds would have been adopted over the years. Yet, not only do agencies such as the Congressional Budget Office and the Office of Management and Budget fight to limit trust funds in order to maintain as much flexibility as possible over annual spending and revenue decisions, but also "Congress itself seems to exercise a degree of collective self-restraint" (p. 193). Patashnik notes that "trust fund advocates rarely can win merely by asserting their narrow mutual interests. . . . Given the multiplicity of veto points in the U.S. system, it is difficult for a coalition to get its structural projects enacted without fairly broad support, and other factions cannot be expected to share the former group's parochial interests" (p. 195).

The only shortcoming is the very limited effort at the end to consider the normative challenges to trust funds. Serious consideration of these matters would require at least another chapter. Yet, any suggestions about what could have been done take nothing away from what has been done. Patashnik's study is a very important contribution to the literature on budgetary politics and policymaking generally. Some may wish for a more parsimonious explanation of the origin and evolution of trust funds, but political scientists would not be better off for it.

In Defense of a Political Court. By Terri Jennings Peretti. Princeton, NJ: Princeton University Press, 1999. 371p. \$27.50.

Sara C. Benesh, *University of Wisconsin—Milwaukee*

Peretti's book purports to refute conventional constitutional scholarship about the dangers of a political Supreme Court, lauding such a political nature as supportive of the peculiar brand of democracy practiced in America. It seems, in large part, that she is quite successful in doing so, although I am certain that it is far easier to convince me of such a notion than it would be to convince the legal scholars whom Peretti so vehemently attacks.

Peretti begins her book with a thorough exposition of the state of legal scholarship on the Court, especially with regard to the undemocratic nature of judicial review and the

legitimacy costs involved in such Court behavior. Indeed, the breadth of the review is remarkable. She discusses and defines neutralist approaches to judicial review, critical legal studies analyses, and reviews conducted by so-called skeptics. She finds fault with all of these interpretive models. First, the neutralists are “simply wrong” (p. 35) in thinking that an objective theory of constitutional interpretation exists that justices can apply neutrally, which allows them to maintain legitimacy and thus reconciles judicial review with democracy by allowing the Court to play a “special” apolitical role. The critical legal studies folks are right about the indeterminacy of the Constitution, but their responses to this reality (utopian speculation and judicial restraint) “are of no practical value” (p. 54). Indeed, according to Peretti, a Court that subjugates its preferences by restraining its power actually detracts from democracy and is more apt to hurt its legitimacy than a raging ideological Court would. Finally, the skeptics are misguided because, while they recognize the indeterminacy of the Constitution and recognize the potential for judicial mistakes, they still regard the Court as special, arguing that sometimes its decisions should be final because they are philosopher kings or prophets of some kind. They see the Court as superior to Congress in making certain types of decisions, yet argue that the Court should be subject to this inferior branch. Peretti deems this accommodation of judicial review and democracy “nonsensical” (p. 70).

In Part II, she attempts to defend her central claim that “there is nothing wrong with a political Court or with political motive in constitutional adjudication” (p. 73). Indeed, she argues that there are democratic benefits to such behavior by the Court. Value-voting on the Court promotes political representation (since judges are selected for and then decide in accordance with their political values), engages political checks on the Court (since policy-minded justices will care more about the policies they are espousing and as such be sure that they are implemented), and leads justices to consensus-building activities (since justices need to accommodate abhorrent views in order to install their policy preferences). The constitutional scholars are also wrong in their assumption that a political court begets legitimacy crises (since the public knows little about the Court, cares little about the Court, and is not adversely affected by politically driven decision making). Peretti systematically discusses each and defends each via reference to a huge body of empirical political science research.

Additionally, all of these theorists are fundamentally wrong, according to Peretti, because they totally misunderstand the U.S. brand of democracy. She argues that, because it is based on pluralism and not majoritarianism, complaints about the Court’s lack of electoral accountability lose their bite. Indeed, in a pluralist democracy, in which emphasis is placed upon multiple means of influence for multiple groups, the Court’s behavior is perfectly normal. The Court is merely one of several coequal branches of government, each offering different sorts of responsiveness and different levels of attention to certain types of groups. The Court is a unique entry point, first, because it may be better suited to hearing claims of the otherwise disenfranchised (although I am not altogether impressed by her justification of this point) and, second, because of its ability to review laws once they are in action and applied to a given controversy. In this way, the Court sees the unintended consequences of legislation, which the legislature is unable to see. Neither the Court nor the constitution is mythical in Peretti’s analysis. They are merely additional checks in a pluralist nation. And this, in itself, is worthwhile.

However, there are a few things that I think Peretti could have done better or made stronger arguments for. For exam-

ple, she discusses in detail the various external checks on the Court, concluding that even though they are not often used, they may still constrain the Court. I think, given her focus on quantifiable arguments and her criticism of the constitutional theorists who make remarks that are not quantitatively justifiable, that she should avoid making a strong argument for the efficacy of external checks. Indeed, little quantitative evidence of such an influence exists and the studies she cites are not convincing in that they are overwhelmingly case studies of peculiar instances where such an effect is identifiable. Not just neutralist legal scholars argue that political checks on the Court are ineffective; many attitudinal scholars also do so. Additionally, it is not clear why *only* policy-motivated justices would behave in this manner. Surely those judges who are motivated by legal policy, a particular reading of the constitution, or the intent of the framers also care greatly about the policy being made? Too strong a case is made for the influence of public opinion and for the nonfinality of Supreme Court decisions given the empirical evidence on this point.

Peretti could strengthen her argument that Congress is as countermajoritarian an institution as the Supreme Court (if we use constitutional theorists’ definitions which center on electoral accountability) because of the incumbency advantage that exists. Surely, electoral sanctions are not perfect in that most-representative body and so that institution may also make decisions that are not strictly accountable to the public.

In short, this is a good, worthwhile book for both legal and judicial scholars. Political scientists can learn much from Peretti’s review of constitutional theory, and constitutional theorists much from her review of empirical political science. Perhaps we all need to critically analyze our opinions about the Supreme Court. Is it really all that special?

The Art of Political Warfare. By John J. Pitney, Jr. Norman: University of Oklahoma Press, 2000. 256p. \$24.95.

Michael John Burton, *Ohio University*

For political scientists, the world of politics is often comprised of policy markets and aggregated data points. Regression models, actor preferences, and game structures are disciplinary norms. For political professionals, however, politics often has a strongly militaristic bent: It is war by other means. John Pitney analyzes political affairs in the way that political professionals commonly do, using armed conflict as a metaphor to understand complex political phenomena.

Pitney argues that “politics resembles warfare, so military literature can teach us something about political action” (p. 3). The language of politics owes much to military combat—Pitney notes “the war[s] on poverty, crime, cancer, drugs, and AIDS” along with Bill Clinton’s campaign “war room” and Pat Buchanan’s ringing exhortation: “Mount up everybody and ride to the sound of the guns” (pp. 8–9). Political officials, activists, and operatives see politics as a species of warfare, and “military metaphors . . . in turn make politics more warlike” (p. 4). The similarities between politics and war run deeper than just language. Both activities are passionate, confrontational endeavors that “expose participants to peril and uncertainty” and require “elaborate strategies and tactics” for their conduct (pp. 5–6). Politics is not exactly the same as war, but neither is politics just like a marketplace, an academic metaphor that is apt to neglect “hard-to-quantify phenomena such as duty, courage, and compassion” (p. 19).

Throughout the book, Pitney draws tight parallels between war and politics. For military and political leaders, strategy is the integration of means and ends, and political professionals and military planners attend to the tactical details of

"geography and logistics" (pp. 140–59). Battle terrain opens some military options and forecloses others; political context defines the range of tactics available to political strategists and sometimes decides the location of the conflict. "The leaders of the Southern Christian Leadership Conference targeted Birmingham, Alabama, in 1963 because they knew that city government would overreact and bare its segregationist teeth" (p. 146; see also p. 94). Martin Luther King, Jr., in the tradition of the great generals, studied the tactical contours of the battleground and planned accordingly (p. 184). Pitney's book is filled with similar anecdotes that reinforce a warfare-driven interpretation of political events.

The Art of Political Warfare is not a simplistic restatement of battle doctrine; it is a sophisticated understanding of political problems in terms of military strategy and tactics. Pitney once served as a senior researcher for the Republican Party, and he understands that no single strategy can fit all political situations. The reason for studying Sun Tzu's *Art of War*, Carl von Clausewitz's *On War*, and even contemporary military training manuals is to gain an understanding of strategy and tactics from a discipline—military science—wherein these things are matters of survival (p. 20). The prospect of war has provoked rigorous thinking about the nature of strategy. Political scientists, who study a domain of human activity that often relies on strategic principles, should not overlook the intellectual value of military scholarship.

Pitney's book serves an important function: It introduces basic concepts of military theory and demonstrates their relationship to political phenomena. Students of war examine mobilization, fatigue, intelligence, deception, and stealth. Those who study legislative politics have an intuitive grasp of such military problems as defining victory, navigating through the fog of war, and eliminating internal friction. Passage of the 1990 Budget Enforcement Act resulted from "a combination of stealth and congressional fatigue" that allowed a major legislative agreement to get through both houses of Congress "even though most members were barely aware of it" (p. 191). "A military approach would draw scholarly attention to such muddy patches of the political process" (*ibid.*). More generally, adding the military lens to political interpretations based on games, markets, physics, ecology, medicine, theater, and education—and, one might add, written texts, human sexuality, artificial intelligence, and other metaphors current in political scholarship—"will help us understand the political battlefield": "The picture may not always be pretty, but we can learn much from appreciating the art of political warfare" (p. 199).

Pitney does not claim that warfare is the only metaphor applicable to politics; he believes that political understanding demands a broad range of scholarly approaches. Nevertheless, he argues vigorously that the military metaphor receives less attention than it deserves. By representing politics as warfare, Pitney invites political scientists to seek a more detailed understanding of military strategy. *The Art of Political Warfare* merely begins the conversation.

Pitney does not elaborate on the stark differences between the teachings of Sun Tzu, who sought nimble conflict, and Clausewitz, who counseled total war. He does not try to reconcile seeming contradictions: On one hand, the threat of brutality toward the conquered can hasten surrender (p. 103); on the other hand, combatants who fear violent recrimination tend to stiffen their resistance (p. 178). The delineation of military classics and the resolution of long-standing tactical disputes lie beyond the scope of the book. Its mission is more carefully focused. Writing in a discipline that does not comprehend the full value of military metaphors, Pitney persuasively argues that the literature on warfare can bring fresh insight to the study of politics.

The Art of Political Warfare is sufficiently lively and readable to suit any undergraduate course in American politics, and it would work particularly well in a graduate seminar that attempts to view the broad sweep of the political landscape. More important, the thrust of Pitney's argument might encourage some scholars of politics and government to begin studying not only David Easton and William H. Riker but also military historians, such as John Keegan and Shelby Foote. Reading Pitney's book, advocates of an interdisciplinary science of politics may find new value in the literature on warfare.

Capital, Labor, and State: The Battle for American Labor Markets from the Civil War to the New Deal. By David Brian Robertson. Lanham, MD: Rowman & Littlefield, 2000. 320p. \$75.00 cloth, \$22.95 paper.

Andrew Battista, *East Tennessee State University*

This important new study argues that American labor markets have been and are governed by employers to a degree unique among Western capitalist democracies; that this pattern of governance is the outcome of crucial struggles among unions, employers, and middle-class labor reformers from the Civil War to the New Deal; and that American political institutions strongly shaped the struggles and their outcome. In the nineteenth century, all Western countries largely protected employer control of hiring, firing, wages, hours, and working conditions, but in the twentieth century nations other than the United States began to curb employer prerogatives and extend worker protection in the form of labor regulations, trade union and collective bargaining laws, public management of labor supply and demand, and work insurance (the four major types of policy in Robertson's framework). In the United States, fewer such protections were established, and the fragmented federal and state labor policies that were enacted were often undermined by lax enforcement or court rulings. On the eve of the New Deal, Robertson shows, U.S. employers had a degree of autonomy in labor markets unparalleled in European and other industrialized countries.

Capital, Labor, and State analyzes and explains this result by means of the changing and conflicting "policy strategies" of unions, employers, and labor reformers as well as the influence of political institutions in shaping and selecting among policy strategies. By the onset of the twentieth century, the American Federation of Labor (AFL) had concluded that it could not rely on government to protect workers. It abandoned the strategy of seeking inclusive public policies in favor of the union shop approach, whereby unions relied on their own economic power to organize employers, control the supply of labor, and manage the terms and conditions of employment through labor contracts. The strategy was decisively defeated, however, by the open shop campaign of both large and small employers in the first two decades of the century.

Thereafter the AFL retreated into a narrow defense of craft unions and skilled labor, otherwise conceding employer sovereignty in labor markets. Labor reformers led by John R. Commons and his colleagues, who initially favored governance of labor markets by industrial commissions staffed by middle-class experts, also then tailored their policy proposals to employer dominance. Later, the New Deal "mounted the most formidable challenge to employers' labor market prerogatives in the nation's history" (p. 260). But Robertson argues that New Deal labor market policies were limited in duration and effect, and by the 1950s employers again had an exceptional degree of control.

These strategies and conflicts were shaped and resolved by the limited authority of the federal government, the

separation of powers at both the federal and state level, and economic competition among the states ("competitive American federalism"). From the Civil War to the New Deal this institutional structure, according to Robertson, made it very difficult for labor to win—or even place on the public agenda—policies that meaningfully limited employer authority and thereby channeled it along the path of "voluntarism" and the union shop strategy. The structure not only favored employers opposed to changes in the policy status quo but also, by allowing mergers but prohibiting collusion, encouraged large and small firms alike to resist unions rather than use them to help stabilize production and prices. Even during the Depression this institutional structure limited the extent to which New Deal policies curbed employer sovereignty in labor markets.

Capital, Labor, and State has many virtues and makes a substantial contribution. It is distinguished by its careful conceptualization of labor market policy, thorough research in both primary sources and secondary literature (in political science, labor history, and industrial relations), focused and well-developed argument, and effective blending of historical, institutional, and political-economic analysis. To my knowledge no other work analyzes developments before the New Deal so comprehensively and systematically (a separate chapter is devoted to each of the four types of labor market policy) or with such careful and sustained attention to all four actors—unions, employers, academic reformers, and government.

Moreover, various scholars have emphasized the role of law and courts in shaping labor politics and industrial relations in the late nineteenth century, but Robertson makes a very strong case that other political institutions, especially federalism and separation of powers, had at least as great an influence. Indeed, even for political scientists not deeply concerned about labor market policy, this book should be of interest as an unusually detailed and cogent case study of how the distinctive governmental structure of the United States molded the development of political conflict and public policy.

I have two reservations about Robertson's argument. First, the claim that at the start of the twentieth century the AFL's union shop strategy aimed at a "fully unionized industrial capitalist economy" (p. 66) and "posed a militant, sweeping, and credible challenge to employers' prerogatives" (p. 85) surely exaggerates the potential of that strategy. Although the AFL grew significantly between 1897 and 1903, which generated an employer counterattack, the federation's small size and narrow social base, exclusion from or weakness in key new industries, subordination of industrial to craft unions, and lack of solid political allies all weigh against such a claim, which could not reasonably be made even of the Congress of Industrial Organizations (CIO) of 1937 or 1945.

Second, although Robertson makes a powerful argument that U.S. labor market policy has been and remains exceptional among the Western capitalist democracies, the "exceptionalist" approach limits his comparative analysis and the light it could shed on the U.S. case. That approach inevitably downplays differences in labor market policies, or any other characteristic, among the other Western countries and does not reveal whether and why the United States is closer to some countries than to others in its labor market policies or union political strategies. *Capital, Labor, and State* is by no means blind to these issues, but they are overwhelmed in the exceptionalist form of argument it pursues.

Yet, any reservations are themselves overwhelmed by Robertson's remarkably rich and impressive study of an issue that so agitated American politics from the late-nineteenth century to the middle of the twentieth. *Capital, Labor, and*

State is a considerable achievement that should be widely read and discussed.

The U.S. Supreme Court and the Electoral Process. Edited by David K. Ryden. Washington, DC: Georgetown University Press, 2000. 322p. \$65.00 cloth, \$23.95 paper.

Campaigns and the Court: The U.S. Supreme Court in Presidential Elections. By Donald Grier Stephenson, Jr. New York: Columbia University Press, 1999. 363p. \$49.50 cloth, \$19.50 paper.

John B. Gates, *University of California, Davis*

The controversy surrounding the 2000 U.S. presidential election has already generated major scholarly work and will do so for years to come. Both works reviewed here were written before the historic judicial events surrounding the November election of 2000. Far from irrelevant, each work offers unique insight into the fundamental rules surrounding political conflict and the historical flow of elections with major social and economic change. As such, we learn much about political science and the struggle over the proper analytical lens for understanding politics.

David Ryden's edited volume, *The U.S. Supreme Court and the Electoral Process*, examines the fundamental rules surrounding some neglected and not so neglected areas of election law. The charge to the distinguished contributors is weighty given the issues and resurgence of interest in the questions surrounding democratic governance. Can or should the U.S. Supreme Court adopt and defend, on the grounds of neutrality and consistency, a theory of representation? How does the judiciary approach one of the most important arteries of representation? More specifically, what rules should govern how political parties struggle with the inevitable tension between majority representation and minority interest in such diverse areas as regulatory politics and campaign finance? What reforms, if any, can the Supreme Court either endorse or initiate given the immediately "suspect" character of an unelected body deciding the constitutional course of representational politics?

Yet the Supreme Court may be ill suited to deal with the incredibly complicated and innately political context of representation. Nancy Maveety provides a useful framework for analyzing the Rehnquist's Court's general approach to political representation. She ultimately describes the Court's treatment as atheoretical. Howard Scarrow's more focused and even more pessimistic account of the Court's treatment of legislative districting reveals how the Court often veers far from the standard of neutrality and logic. A comparison of these two chapters highlights the challenges before the authors and the U.S. Supreme Court. Neither more general nor narrower investigations of representational rules can avoid the inherent value choices available to the judges at the level of the Supreme Court.

Anyone interested in election law, political parties, and democratic governance can learn much from this microanalytic work on the rules laid down in the American context. It is striking that the authors tackle these questions both with rugged scholarship and with an often-hidden eye toward how the different answers could play out in the new international political arena. Yet international readers, and even casual observers of the U.S. Supreme Court, should come with a rather broad knowledge of the Supreme Court's decisions on the U.S. Constitution and political representation. It is a shame that books must have broad and intriguing titles because their flaws ultimately are noted in the lack of coverage of a particular area. Nonetheless, given the quality of the work contained in *The U.S. Supreme Court and the Political Process* and the

events surrounding *Bush v. Gore*, 121 S.Ct. 525 (2000), readers should find this volume in a second edition very soon.

Donald Grier Stephenson's work on electoral politics is much different in its approach, theoretical focus, and method. Stephenson sets forth several macroanalytical propositions in *Campaigns and the Court*. As such, he examines each proposition with an historical method eschewing quantification and precision. He also employs a broader time frame and a very different focus beyond the institutional rules at the forefront of the Ryden volume. Stephenson forces us to integrate how environmental and broad social changes precipitate and interact with the complex character of the U.S. Supreme Court. More specifically, he examines the Supreme Court in the periods before and after so-called "critical" presidential elections. He seeks to test five propositions contained generally in the existing literature. First is the proposition that partisan involvement with the Court is more likely when the Court uses its power of judicial review to overturn rather than uphold the policy choices of the popularly elected branches. This proposition finds strong support in the historical record.

The second proposition is well-known: The Court will become embroiled in policy conflict with a new majority coalition following elections denoting major and long-term partisan change. Stephenson's third proposition relates to such major partisan shifts: The Court will begin within a decade to make policy more consistent with the new majority. This proposition does hinge on several nearly stochastic events such as turnover on the high bench. The fourth proposition focuses on how the Court's decisions can "clarify" or "define" the response of party leaders and their constitutional options on the issues surrounding major partisan change. Finally, when the Court becomes involved in the politics of the divisive issues surrounding major partisan change, there are attacks on the Court's legitimacy and its power of judicial review.

None of these propositions will be considered novel by careful readers of the work of Robert Dahl (1957), David Adamany (1973), Richard Funston (1975), William Lasser (1985), et al. Yet no single work in this literature attempts to test the validity of each proposition across such a long time frame, spanning from 1800 to the mid-1980s. With this book, Stephenson may well enter the ranks of constitutional historians such as Charles Fairman and Charles Warren. Rarely does one find such care and scholarship in addressing so many interesting and difficult questions.

Clearly, Stephenson has mastered the historical method and uses it to address generalizable propositions about the dynamics of the Supreme Court and important presidential elections. Similarly to those who use a strained theory to address important questions with precision and technical expertise, one walks away from the method utilized with serious reservations. Each conclusion is so qualified that no settled answer follows. While some may blame the method used, it is documented that other methods generate similar qualifications on these very questions. The reason is simple: Huge, macroanalytic questions often belie a single analytical strategy. Nonetheless, Donald Grier Stephenson's work stands at the forefront of a research tradition he warmly embraces.

The U.S. Supreme Court is dynamic in its rules and its place in American political history. Such trite offerings in no way take away from the lessons learned by Ryden's fine volume or Stephenson's masterful historical analysis. Yet one is reminded of Terri J. Peretti's *In Defense of a Political Court* (2000). The American judicial ideal of neutrality and consistency is perhaps nowhere more difficult to meet than when confronting the U.S. Supreme Court and the fundamental questions surrounding representation and democratic governance.

A Logic of Expressive Choice. By Alexander A. Schuessler. Princeton, NJ: Princeton University Press, 2000. 177p. \$49.50 cloth, \$16.95 paper.

Michael C. Munger, *Duke University*

An interesting aspect of life at Duke is the annual construction of our local Brigadoon. The well-ordered but ephemeral tent city is named "Krzyzewskiville," after Duke's head basketball coach. K-ville appears once a year in the weeks before the game against UNC-Chapel Hill, our arch rival. So many students want to see this game that an elaborate nonprice rationing scheme, based on a queue, has evolved to allocate tickets. "Tenting" students may have to wait two weeks or more to get tickets. The game is in January or early February, so they sleeping outside and try to keep up with their school work despite rain, snow, and subfreezing temperatures at night. Random checks (even in the middle of the night) are conducted by student representatives; if a tent is empty too often it is taken down, and the residents lose their place in the queue.

Why do 3,000 students queue up for 2,000 seats (which are never used, since students stand throughout the game)? Because the game is sold out (tickets on the "spot" market sell for \$1,000 or more), it is always televised over both broadcast and cable networks. Students cite fun as the reason, but deeper probing reveals a nearly uniform response: "I want to go to show support for the team. I'll cheer them on!" In other words, they tent out for two weeks in freezing temperatures because they feel their support makes a difference. No individual student can believe s/he makes a difference, of course, since that person's seat would be taken by someone else, just as fanatical, if s/he stayed home. Furthermore, one voice has a negligible effect in the cauldron of noise that is Cameron Indoor Stadium on the night of battle. No one fan "matters" in any important respect, yet each student eagerly pays huge queuing costs for the chance to be there.

What does this have to do with voting in a political contest? In a way, that question in Alexander Schuessler's starting point in *A Logic of Expressive Choice*. He claims that "individuals often are motivated by a desire to express their tastes, or preferences, because such expression has direct influence on who they are—on their identity." (p. 3) Schuessler's choice of title is interesting and gives an insight into his approach: He is proposing "a" possible logic of expressive choice, one that is different from, although not entirely opposed to, the more standard logic of collective action of rational choice theory.

Schuessler's goal is to give an internally consistent account of why a citizen might rationally (although not strategically) choose to express a preference in an election.

For example, and most simply, it is the voter's statement of her preference for the Democratic candidate, through voting, that *makes* the voter a Democrat. This self-definition through voting will at least in part determine her motivation to support a candidate. Following the very same logic, candidates campaigns are designed to draw voters into an election by making it attractive for them to identify with their vote choice. Consequently, to understand voting fully requires us not merely to map out the instrumental consequences of the individual's vote on the electoral result, as in modern political economy we most typically do; it additionally requires us to investigate the expressive, or in some sense *existential*, consequences of the voter for the voter herself (p. 3, emphases in original).

Schuessler (wisely) does not pretend to "resolve" the knotty paradox of participation. The argument rests, rather, on two key points. First, he claims that the paradox simply

disappears when the problem is realistically considered. Participation is not a contribution to a public good, with the associated problems of free riding and underprovision. Rather, the public good is an outcome, which will occur with or without the voter's participation. The participation decision is then a choice by the voter: "I can, *at the mere cost of participation*, purchase for myself the status of outcome-producer" (p. 5, emphasis in original).

Second, Schuessler argues that methodological individualism is the appropriate starting point, but that this individualism should be conceived as ontological. More simply, campaigns are not a process of appealing to a preexistent and fixed set of preferences. Instead, the preferences being expressed by voters are the product of the campaign itself. An effective campaign, according to Schuessler, will focus on the "who" and "how many" dimensions of the electorate. The who aspect derives from the (potential) voter's perception of the candidate's standing among the rest of the electorate. The how many aspect is a product of beliefs about just which people, or groups, can be expected to offer support. The aggregation process implied here is much more of a macro (social) to micro (individual) process.

I expect that many traditional rational choice theorists will be profoundly uncomfortable with this approach, and not a few will reject it out of hand. That would be a mistake. Theoretical rational choice is, or should be, quite agnostic about the source of preferences. The notion that they might be determined endogenously is not outlandish, particularly when the behavior in question is collective and social. Given the way that Schuessler specifies preferences, his citizens are perfectly rational.

Overall, this book is terrific. Unusually well-written, it offers many insights, large and small, into the voting choice and the nature of campaigns. It is this last point that may prove to be the book's lasting contribution; if voters behave as Schuessler claims, the very nature of the campaign will have to be rethought. This provocative and important book is a large first step toward directing that new thinking.

Crowded Airwaves: Campaign Advertising in Elections.

Edited by James A. Thurber, Candice Nelson, and David A. Dulio. Washington, DC: Brookings Institution. 2000. 178p. \$39.95 cloth; \$16.95 paper.

Karen M. Kedrowski, *Winthrop University*

The essays in this short volume address three themes: the characterization of political campaign television advertising, the need to measure its effects, and the role and implications of issue advertising. This is an ambitious agenda, but the book makes some important contributions to the literature on campaigns and on political ads specifically.

There is much to like. First, the book is comprehensive. Various chapters address all federal elections—presidential, Senate, and House. Both the issue ads sponsored by interest groups and those produced by the campaigns themselves are covered. Most chapters attempt to combine methods and to place analyses within the broader context of campaign studies. For example, in the chapter on House races, Herrnson and Patterson analyze district-level data in both competitive and noncompetitive races and in open-seat and incumbent races. Scholars know that these variables change campaign dynamics greatly.

Second, the authors take on important questions. For instance, much of the literature on voting behavior suggests that the key predictor is partisanship, and the media have no influence on vote choice, which is a serious blow to the careers of those who study political campaign ads. Yet, the

Iyengar and Petrocik chapter reminds us that "correlation is not causation." The authors start with the premise of "basic rule voting," that is, presidential vote choice can be predicted by either partisanship or approval of the incumbent. The latter accounts for the vote choices of independents, disgruntled partisans, and such outliers as Reagan Democrats. They show some intriguing evidence that media exposure over the course of a campaign may mobilize partisans to vote and may convince potential defectors to stay with the party's standard bearer.

Another fine chapter, by Kim Fridkin Kahn and Patrick Kenney, covers Senate elections. They test whether voters learn more from negative advertising rather than positive or issue ads. They conclude that more learning does occur from negative ads.

The features that make this book appealing to scholars are likely to make it unappealing to other audiences. For instance, the comprehensive review by Richard Lau and Lee Sigelman covers about 55 studies over 15 years on the effectiveness of negative campaign ads. They cautiously try to determine the "majority opinion," and although the chapter is rather thinly cited, it is worth reading. The bibliography is a treasure trove for anyone studying for comprehensive exams or doing a literature review for a dissertation.

Another example of narrow appeal is chapter 3, which categorizes political advertisements. Kathleen Hall Jamieson, Paul Waldmen, and Susan Sherr make two methodological points of considerable interest to scholars but of less interest to practitioners, undergraduates, or others. The first is that the conventional classifications of positive and negative ads are too facile. A more sophisticated approach would change the unit of analysis from the ad to the ideas and arguments it contains, which would reveal a smaller amount of negativity in campaign ads. Their second argument deals with the superiority of the Annenberg School's collection of presidential campaign ads over the University of Oklahoma collection. Again, this is a point of some importance only to scholars.

The last substantive chapter concerns issue ads. Written by Darrell West, it is different in focus and approach from the other chapters and does not fit as well into the entire volume. Most of the other essays are quantitative, but West uses a qualitative approach to describe the rise of issue advertising in federal elections nationwide, and he describes the various "solutions" proposed to this problem. The chapter includes little original research and relies primarily on anecdotal evidence. In the end, West does not convince me that issue ads are a "problem," much less that relatively incremental recommendations would solve it.

The Courts of International Trade: Judicial Specialization, Expertise, and Bureaucratic Policymaking. By Isaac Unah.

Ann Arbor: University of Michigan Press, 1998. 233p. \$47.50.

Stefanie A. Lindquist, *University of Georgia*

In *The Courts of International Trade: Judicial Specialization, Expertise, and Bureaucratic Policymaking*, Isaac Unah has ventured into territory that has remained largely uncharted by scholars of judicial politics. With the prominent exception of Lawrence Baum's work on specialized courts, few researchers in political science have chosen to explore courts that fall outside the federal judiciary's core hierarchy. Yet as Unah points out, these specialized courts, including the U.S. Tax Court, Claims Court, Court of International Trade, Bankruptcy Courts, and the Federal Circuit, perform critical functions that have the potential to affect business interests and shape bureaucratic performance in highly complex

regulatory and economic areas. In this book, and in his previous published research, Unah has initiated an important expedition into unfamiliar but promising terrain.

By focusing on the U.S. Court of International Trade (CIT) and the Federal Circuit, Unah raises a series of interesting research questions. First, how does decision making differ between generalist and specialized courts? Do models of judicial behavior developed in the context of generalist courts apply to judges on specialist courts? How does the institutional structure of administrative agencies affect judicial review of agency decisions? And do these specialized courts exhibit any particular policy orientation, even after controlling for the partisan affiliation of judicial personnel? Fortunately, the CIT's docket is composed of cases that allow Unah to construct a straightforward research design to analyze several of these questions.

The CIT hears cases appealed from the International Trade Commission (ITC), an independent regulatory commission, and from the Department of Commerce (DOC), an executive agency, both of which render related judgments in anti-dumping and countervailing duty (AD/CVD) cases. In these cases, the ITC judges the adverse impact of unfair trade practices on domestic industry, while the DOC determines whether a claim of dumping or foreign government subsidization can be supported by hard evidence. Both tasks are critical for the imposition of protective tariffs but are rendered by separate bureaucracies with different institutional structures. Aggrieved claimants can appeal an unfavorable determination by either agency to the CIT, where individual judges render decisions in AD/CVD cases under a deferential standard of review. A loss in the CIT may be appealed to the U.S. Court of Appeals for the Federal Circuit and ultimately to the U.S. Supreme Court.

Unah begins his analysis by setting forth a series of theoretical propositions regarding the behavior of specialized courts. Although his theoretical discussion tends to drift into normative territory, Unah first suggests that judges with specialized knowledge and expertise will less often defer to administrative agency decisions. To test this proposition, Unah compares reversal rates at the CIT of appeals from administrative agencies with reversal rates of agency appeals in the federal district courts and U.S. Supreme Court. Perhaps not surprisingly, Unah finds that the CIT is significantly more likely to reverse the agency's decision. While this finding may be suggestive, it cannot be viewed as conclusive, since the types of agency decisions appealed to the district courts differ substantially from those appealed to the CIT. In a later chapter, Unah compares reversal rates of agency appeals in the Federal Circuit and the D.C. Circuit, finding (contrary to expectations) that the Federal Circuit demonstrates a higher level of deference in agency appeals than the generalist D.C. Circuit. Of course, many cases in the D.C. Circuit are appealed directly from federal agencies, while the Federal Circuit hears cases appealed from a lower court. The Federal Circuit's deferential stance toward trade agencies could thus be attributable to the agencies' choices not to appeal losses in the court below.

Drawing on research presenting integrated models of judicial behavior based on legal and extralegal factors, Unah turns to theories of judicial decision making. In specifying a series of decision models at the CIT and the Federal Circuit, Unah includes variables that reflect judges' party affiliations, certain case facts related to the applicable statutory standards, economic indicators reflecting trade deficits and other factors, and industry-specific measures related to party resources. To evaluate his hypothesis that decisions rendered by an independent regulatory commission will receive greater deference than those rendered by an executive agency, Unah also includes a variable measuring whether the case was ap-

pealed from the ITC or the DOC. Based on his models of CIT decision making, Unah concludes that the court generally adopts a protectionist orientation, that it is more likely to reverse the DOC than the ITC, that statutory standards are significant determinants of CIT decisions, that industry political power has some, but not an overwhelming, effect, and that the political affiliation of CIT judges has little impact on case outcomes. At the Federal Circuit, Unah tests similar models and finds that the Federal Circuit is more likely to protect U.S. industry when the plaintiff is an economically powerful industry, when the U.S. holds a high trade deficit with the trading nation, and when the panel is dominated by a Republican majority. And, as in the CIT, the Federal Circuit is more likely to affirm the ITC's decisions than those of the DOC.

From these findings, Unah draws several broad conclusions. First, he argues that bureaucratic structures condition judicial review. Based on the finding that decisions from the ITC (an independent regulatory commission) are more frequently supported than those of the DOC (an executive agency), Unah suggests that the bipartisan, quasi-judicial ITC renders more defensible judgments than the more political DOC. Of course, this interpretation is somewhat speculative, since Unah's data do not allow a clear conclusion concerning the causal connection between institutional structure and judicial review. It is possible, for example, that the ITC is affirmed more frequently because its judgments concerning economic consequences for domestic industry are more clearly susceptible to proof with reliable data. Second, Unah concludes that integrated models of judicial decision making best account for judicial behavior in the CIT and Federal Circuit, as both specialized courts are sensitive to statutory factors, as well as extralegal pressures such as party capability.

Finally, Unah suggests that CIT judges' expertise translates into less deference to the judgments of administrative agencies, and that these specialized judges "embrace uncertainty far more courageously than do generalist [judges] in their dealings with the bureaucracy" (pp. 175-6). While it may be true that expertise leads to more intensive judicial review, some might quibble with Unah's characterization of such review as "courageous." As Dean Leeborn of the Columbia Law School noted in 1999 at the Eleventh Judicial Conference of the Court of International Trade, "some have said that expertise—there may be too much of a good thing—may lead to a distortion in a standard of review, as members highly experienced with the subject matter might be more inclined to second-guess administrative agency determinations" (195 F.R.O. 89). From this perspective, Unah's findings could support the more negative interpretation that expertise engenders overly intrusive judicial review.

Unah's book thus raises interesting empirical and normative questions about the nature of judicial review by a specialized judiciary and will surely generate further research on the relationship between specialized courts and the bureaucracy. Because the book represents an extended foray into rarely explored territory and substantially expands our knowledge of the specialized judiciary, Unah has made a significant contribution to the literature.

Reelection: William Jefferson Clinton as a Native-Son Presidential Candidate. By Hanes Walton, Jr. New York: Columbia University Press, 1999. 352p. \$49.50 cloth, \$22.00 paper.

Jay Barth, *Hendrix College*

At the end of the Clinton era, it is appropriate for political scientists to undertake analyses of this two-term president's

effect on any number of political and public policy phenomena. Clinton's most obvious talent showed itself in the electoral arena, where (even in a television era) he formed deeply personal relationships with voters, and many of these analyses rightfully focus on the foundations of Clinton's public support. Continuing this line of research, Hanes Walton examines a potential explanation for Clinton's relative strength in one region of the country—the turf on which his political career began—through an in-depth analysis of Clinton's electoral base across time in Arkansas and the South. The book examines a well-justified question, about the ongoing vitality of a "native son" phenomenon but numerous problems with the method by which Walton attempts to answer that question weaken this work. Numerous factual errors and muddled writing make it even more problematic.

Walton is quite conscious of grounding his analysis theoretically. He lays out the line of research on native sons in American voting, beginning with V.O. Key's highlighting of "friends and neighbors" voting in the Old South (*Southern Politics in State and Nation*, 1949). Walton clearly presents the ways in which later scholars built on this tradition, culminating with his own examination of Jimmy Carter's linkages to fellow southerners (Walton, *The Native-Son Presidential Candidate: The Carter Vote in Georgia*, 1992). Because of the tremendous shifts in southern partisanship since Key's work, an analysis of the persistence of regionalism in voting in the Clinton era is certainly pertinent.

Walton is less clear in another theoretical chapter. He notes the inadequacies of the frameworks traditionally used to describe electoral outcomes (e.g., realignment theory) and presents what he terms a "more complete" approach. His "political party perspective" posits that individual voters' choices as well as the historical context of a particular election are both important and, of course, linked. Walton goes on to make the (quite logical) contention that race is the central driving force in this vision of electoral politics in the United States. Yet, the discussion of the political party perspective is muddled, and, more important, Walton's argument about race is not well grounded in the literature on the interaction between race and American politics. In addition, the linkage between the native son theory and the political party perspective is never made.

Walton then sets out to paint a picture of the political-historical context of Arkansas and, more specifically, of its racial context. In the second of these chapters Walton makes his greatest contribution, one that is of primary relevance for students of Arkansas politics. As he makes clear, voting by African Americans in Arkansas has not received adequate attention, and Walton employs creative techniques that he terms "political archeology" to recapture their role in the state's politics in the late-nineteenth and early-twentieth century.

Walton then discusses Bill Clinton's political entrance into this context and undertakes a chronological journey through the ups and downs of his political career in Arkansas. The roles of Clinton's home counties and counties in which black voters are most prevalent are examined as the two parts of the state that serve as "friends and neighbors," to use Key's term. Unfortunately, a variety of methodological problems undermine Walton's attempts to investigate the two tangentially related parts of his theory as they relate to Clinton's electoral life.

By necessity, scholars of American electoral politics often rely upon county-level data. Walton, however, is not careful to note the limitations of such analysis in gaining an understanding of the voting behavior of the individuals living in those counties. Moreover, Walton fails to introduce key control variables to enhance one's confidence that home county

or racial composition are the true explanatory factors of electoral patterns. Throughout this portion of the text, Walton moves back and forth between analyzing vote percentages and raw votes. Because county populations differ, any statistical analysis of raw votes is irrelevant, but Walton relies upon correlation data on raw votes in drawing conclusions. He also ignores the fact that presidential and nonpresidential elections affect turnout, which undermines other of his points. In short, the rudimentary statistical analyses are very questionable.

More specific problems arise with the two theoretical arguments. Regarding the native-son phenomenon, Walton examines only electoral behavior in Clinton's home counties, not the homes of his opponents. In addition, in his analysis Key employs maps, and more of these would have made Walton's points much clearer than the series of confusing tables he uses.

Even more problematic is the analysis of race. Throughout the text, Walton uses the three counties in the state that have remained majority black to provide empirical insights. The electoral results in those counties, which have a large number of white voters, are his gauge of Clinton's support from African Americans in the state as a whole. Issues of statistical validity aside, Walton fails to discuss the fact that white southern voters, as Key found, behave differently depending on the size of the black population in their locale; whites in counties with a majority of African Americans feel "threatened" and base political actions on this fear. Furthermore, the role of African Americans in Arkansas politics is never united with the theoretical framework presented earlier in the work. This disconnection between the theoretical and empirical portions of the work is a weakness.

The descriptions of Clinton's races in the state are surprisingly weak. In the coverage of his first race for Congress in 1974, there is no discussion of the role of Watergate in aiding the Democrat Clinton against a long-time Republican incumbent, and the coverage of other races is lacking in crucial detail. Moreover, there are some obvious errors, including the outcome of a key gubernatorial election in the 1960s, the fact that Governor Winthrop Rockefeller (from New York) was not "homegrown," and the identity of the congressional district in which Little Rock lies.

Factual errors mar the credibility of the work more generally. These range from stating that Richard Nixon ran for president in 1964, to misidentifying both the states that George Wallace won in his 1968 bid for the presidency and the southern states captured by Clinton in 1992 and 1996, to the date of the Lewinsky scandal. Also, the writing is not of high quality, and arguments are not pulled together or clearly explained.

Walton attempts to compare the performance of Clinton as a presidential candidate in his home state and region with that of Jimmy Carter. It quickly becomes apparent, however, that dramatic changes in southern presidential politics during the Reagan-Bush era make comparisons of limited relevance. In the end, Walton makes the point that there remains a "regional variable," at least for southern candidates in presidential elections, but this work fails to move our understanding of contemporary southern politics beyond this truism.

The Politics of Women's Rights: Parties, Positions, and Change. By Christina Wolbrecht. Princeton, NJ: Princeton University Press, 2000. 266p. \$55.00 cloth, \$17.95 paper.

Kathleen Dolan, *University of Wisconsin, Milwaukee*

When we talk about the gender gap, students in my Women and Politics course are always surprised to learn that men used to be more likely than women to support the Democratic

Party and that Republicans were the original supporters of the Equal Rights Amendment (ERA). Many people forget, or are not familiar with, the significant party transformations on these issues over the last 50 years. Christina Wolbrecht's new book provides important insights into this transformation and is a valuable contribution. The study of political parties has not, to date, accounted for this phenomenon, and gender studies too often ignore the crucial role of political parties in our system.

Wolbrecht is generally concerned with exploring how, when, and why party issue realignments occur. More specifically, she seeks to explain the 180-degree turn of the two major parties on women's rights during 1952-92. On both counts, she succeeds admirably. A major contribution of this work is the careful explication of a theoretical framework that accounts for adoption of and change in issue position by parties. Drawing on theories of critical realignment, issue evolution, agenda setting, and congressional behavior, Wolbrecht suggests that the two parties' relative positions on any issue are determined by three factors: the issue, the party coalitions, and the party elite. She then incorporates a discussion of issue equilibrium and disruption, processes through which one can view changes in party position.

A second contribution is the thorough discussion and analysis of the broad issue of women's rights during the period under study. Wolbrecht carefully details the important players, events, societal changes, and political aspects of a movement that had profound consequences in our society. Beyond this, she moves from description to explanation through the application of her theory. She outlines a debate that began between the Democratic Party and its support for protective legislation and the Republican Party and its support for the ERA; it evolved into a debate between liberation (the Democrats and their embrace of feminism) and tradition (the Republicans and the increased influence of social conservatism).

In unraveling the changes in the debate, Wolbrecht addresses questions about the public salience of women's rights, the changing nature of the party coalitions, and the coincident changes in elite behavior. Relying on data from several sources, she demonstrates how the social and political context of the 1960s and 1970s gave women's rights greater salience among the public and political elites. The resultant disequilibrium in women's rights led to profound changes in the party coalition interests of the Democrats and Republicans and the responses of their leaders. Today, the two major parties take positions on women's rights that are very different from the past and also are very different from each other.

A third strength is the ability of the author to tackle difficult measurement questions and bring several different sources of data to bear on the central argument. The methodological aspects of the work are rigorous, appropriate, and follow from the theoretical framework that underlies her arguments. Documenting a complex transformation over 40 years is not easily accomplished, but Wolbrecht pulls together several creative sources of information to do just that. She employs survey data and content analysis of the *New York Times* to show the importance of increased public salience to the process of issue equilibrium disruption; material from congressional hearings and party platforms to illustrate the process of issue redefinition and agenda change; and cosponsorship of legislation, roll-call data, and interest group ratings to establish the shifting behaviors of party elites.

My one quibble is with chapter 6, where Wolbrecht attempts to show how changes in interest coalitions and the composition of party elites contributed to the transformation in issue position. "It is important to distinguish between party elites, such as members of Congress and party leaders, and the parties' coalitions of interest," by which she means

"the general array of interests associated with each political party . . . identifiers, voters, convention delegates, funding sources, activists, and organized allies" (p. 182). Yet, she relies on the composition of party delegations in Congress (arguably party elites) as a proxy measure of the composition of party coalitions. To that end, she offers party unity scores and geographic and ideological data on members of Congress for the years under study. In discussing the changes in party elite behavior, she also uses data on members of Congress, in this case cosponsorship of women's rights legislation as a measure of changing behaviors. Wolbrecht points out the difficulty of measuring party coalition activities, but since she makes an important distinction between coalitions and elites, her use of data on members of Congress to measure the behaviors of both leaves us without a clear sense of how external groups and actors influence party behavior.

But this is a small exception to take with a work that succeeds at many levels. It provides a strong theoretical framework in which to consider the development and change of party issue positions, and it provides a rich and detailed summary of a complex movement and set of issues. Scholars of political parties, public opinion, and gender politics will find much in this volume to like.

Pursuit of Justices: Presidential Politics and the Selection of Supreme Court Nominees. By David Alistair Yalof. Chicago: University of Chicago Press, 1999, 280p. \$27.50.

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It is a pleasure to review this well-written book, which should be of enormous interest to every serious student of judicial politics and the presidency. Based upon a thorough mining of the presidential papers of seven presidents and numerous interviews with key participants in the selection process, along with other primary and secondary sources, Yalof gives us a presidency-by-presidency take on the recruitment of Supreme Court nominees.

In his introductory chapter, Yalof lays out the variables that shape who is nominated. Among them are the timing of a vacancy, the political and ideological composition of the Senate, the popularity of the President, the attributes of the justice responsible for the vacancy, the realistic pool of available candidates, and the decisional and bureaucratic framework used by the President to reach the selection decision. Yalof suggests three decisional frameworks: an open recruitment process whereby recruitment begins after the vacancy occurs; a single-candidate process in which the nominee has been, for most intents and purposes, chosen in advance of an actual vacancy; and a criteria-driven process whereby, in advance of a vacancy, the president specifies criteria to guide selection.

In addition, Yalof discusses 10 factors that help explain how the recruitment process has evolved. Perhaps the most important of them are the growth and bureaucratization of the Justice Department; the growth and bureaucratization of the White House, particularly the White House Counsel's office; the divided government; an increasingly public confirmation process; increased participation by interest groups; and increased media attention. The variables including the decisional frameworks as well as the factors are considered more or less systematically in the subsequent five chapters and then in a more focused fashion in the last major chapter.

In his chapter on Harry Truman, Yalof traces Truman's personal process of picking Supreme Court justices, offering new light on the twists and turns that culminated in Truman's appointments of Burton, Vinson, Minton, and Clark. In contrast, Dwight Eisenhower's selection of Supreme Court nominees after Earl Warren was dependent upon the top officials

of the Justice Department, who worked from the constraining criteria that Eisenhower imposed including judicial experience, age (no one older than 62), and, as Yalof documents, religion in terms of finding a Catholic to appoint before the 1956 presidential election. The chapter on Kennedy and Johnson points up the differences between Kennedy/Johnson and Eisenhower and also between Kennedy and Johnson in the selection of Supreme Court justices. The path to the selection of Byron White was more circuitous than that for Goldberg. Most remarkable in the history of Supreme Court selection was Lyndon Johnson's, in effect, creating vacancies that he could fill with Abe Fortas and Thurgood Marshall, stories told before but well recounted and documented here by Yalof.

Yalof offers some fascinating revelations in the Nixon and Ford chapter including Warren Burger's behind-the-scenes maneuvering to get appointed to the Court as well as events leading up to the nominations of Haynsworth, Carswell, and then Blackmun to fill the vacancy created by the resignation of Abe Fortas. The story of finding replacements for the Black and Harlan seats is discussed and the documentary evidence suggests that Richard Nixon was indeed serious about wanting to place a woman on the Supreme Court.

Events leading to Ronald Reagan's appointments to the Court including the ill-fated nomination of Robert Bork and the aborted one of Douglas Ginsburg have been told before, but not until this book have they been grounded in the Reagan presidential papers, which gives Yalof's account greater authenticity. Concluding the main body of the study, Yalof notes several trends in the evolution of the selection process including the near-lock that federal appeals court judges have had on nominations since 1968 (Yalof points out that 16 of the 19 nominees have been appeals court judges) and the shifting of selection from the back pocket of the president to the back rooms of the White House Counsel's Office and Justice Department. Yalof notes that there has been a shift to the criteria-driven decisional framework. The open decisional framework accounts for only five nominations since 1945, the last one being that of Stevens in 1975. The single-candidate framework accounted for 9 nominations but the criteria-driven framework can be seen with 14. Yalof provides an epilogue in which he provides what he calls

a tentative analysis of the Bush and Clinton appointments to the Supreme Court relying on newspaper and secondary sources.

As thorough a study as Yalof's book is, there is an important omission. In only two footnotes do we find mention of President Jimmy Carter. Of course Carter did not have an opportunity to name a justice to the Supreme Court but he had ample opportunity to name lower court judges and the Carter administration selection process had an important influence on administrations to come. In particular, during Carter's administration key White House Counsel and Justice Department personnel began regular meetings to decide on recommendations to be made to the president. This was the beginning of the joint White House–Justice Department Judicial Selection Committee that has been a part of the process since. And most importantly, Carter and his administration pushed affirmative action in judicial appointments. Had Carter not been so insistent on finding qualified women for the bench and had he not been widely expected to name a woman to the first vacancy on the Supreme Court (he was seen as grooming Ninth Circuit judge Shirley Hufstedler, who resigned her lifetime position to join Carter's cabinet), it is questionable whether Ronald Reagan would have made his pledge to appoint a woman to the Supreme Court during the fall 1980 presidential campaign.

There are a few typographical or other errors. As for the latter, for example, the discussion of the growth and bureaucratization of the Justice Department fails to mention the Reagan Administration's creation of the Office of Legal Policy, which undertook the responsibility of screening, vetting, and processing all judicial nominations (p. 13). Edward H. Levi is described as "only marginally active in Republican politics" when in fact he was a former Democrat turned political independent (p. 126). Senator Hruska is described as being from Alaska when in fact he was from Nebraska (p. 247). *Miller v. California* is footnoted as the case that "included some pornography within the First Amendment's protections" (p. 133), instead of *Stanley v. Georgia*.

It should be evident that the errors are minor and that this book is a major contribution to our understanding of the nominating of Supreme Court justices. David Yalof is to be congratulated for his outstanding work.

Comparative Politics

Political Culture in Post-Communist Russia: Formlessness and Recreation in a Traumatic Transition. By James Alexander. New York: St. Martin's Press, 2000. 267p. \$65.00

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Of all the "traumatic transitions" in the postcommunist world, the Russian one ranks as one of the most tortuous and painful. Why is this? Does the problem lie in the institutional structure created in the early postcommunist years, or do the roots lie deeper, in Russia's innate authoritarianism and cult of strong leadership? Employing the concept of political culture, this is the question that Alexander seeks to answer. He approaches the idea in an unusual way, however, by focusing rather more broadly on the concept of "culture" and its potential for change rather than any narrowly defined notion of "political" culture. He insists that a nation's political culture cannot be reduced to one or two simple and measurable parameters but is a complex web of malleable relations and contradictions; and he seeks to understand them by using what he insists are nonstandard approaches.

The book takes an ethnographic approach to the study of postcommunist society. The fieldwork was conducted in the cities of Syktyvkar (the capital of the Komi Republic) and Kirov (formerly Vyatka) between October 1993 and August 1994. The material is marginally updated as a result of a further research trip to Syktyvkar in summer 1997, but the work stands or falls on the material gathered during the earlier research to these two provincial towns nearly a thousand miles to the northeast of Moscow. The general argument, rehearsed repeatedly though the book, is that traditional and predominantly quantitative methods fail to grasp the complexity of political culture. Alexander insists that only ethnographic methods, above all, open-ended face-to-face interviews, can provide the nuance that is forgone by closed-question surveys.

The two towns selected as case studies are illustrative of two federal categories: Syktyvkar is the capital of a republic and thus enjoys enhanced federal privileges and greater independence from the central authorities. Kirov, on the other hand, is just one of Russia's 50-odd ordinary *oblasts* (regions). A further difference between the two is that Kirov is an industrial town tied to the fortunes of the military industrial complex, whereas Syktyvkar is a relatively small administrative center

in a republic that has great natural resources that were once exploited by prisoners in the many labor camps that formed part of the *gulag* archipelago. The differences between the two towns allowed conclusions to be drawn from the differential impact of economic reform on public attitudes, assessed on the basis of some 30 interviews with representative individuals in each place. The author insists that it is misleading to suggest that there is a homogeneous national political culture and, instead, argues that each community will have contours of its own. To a degree this is proven, but largely in matters of detail: there appears to be a remarkable underlying similarity between the two areas in broader cultural responses to the breakdown of the old order and the troubled birth of the new.

The analysis is structured in terms of a tension between two types of political culture, "democratic" and "authoritarian," although Alexander allows some subtle blending of the two. He adds another variable, the notion of "formlessness," to suggest the flux undergone by political culture in a period of accelerated change. The basic conclusion is that Russian political culture still bears the imprint of Soviet authoritarianism, yet was "primed" (p. 3) for liberal democracy as it exited communism. The work explores the tension between continuity and change, examining the dynamics of both and the often contradictory political culture that emerges as a result. Crude versions of the "continuity" thesis—of an unbroken and implicitly unchangeable Russian tradition of authoritarianism—are rejected. Alexander nevertheless insists that the dominant trend in Russian political culture is support for the symbols and values of what we may call "Russian traditionalism." Although Alexander does not say so, his findings appear to be confirmed by the rise of Vladimir Putin's distinctive blend of a liberal modernizing agenda cast in a traditionalist form. The book suggests that a political system out of kilter with the political culture (*pace* Eckstein's notion of congruence) will be unstable and that the optimal outcome is a "resting place" (pp. 40–1) in which the two reinforce each other. It remains to be seen whether Putin's system is that stable "resting place" for Russia.

The work is thus an unusual combination of "thick explanation" (not description; p. 67) and a methodological critique of the traditional survey approach to the study of political culture. The author insists that the latter cannot begin to identify and quantify the nuances of popular understandings, beliefs, and preferences, arguing that words and concepts take on different meanings as they travel between cultures. This point is perhaps rather labored, since the better studies of political culture have always been aware of the need for historical contextualisation, but appears to be part of the author's own liberation (suggested in his comments about his two years spent in the Peace Corps in Ecuador in the mid-1980s) from the tyranny of quantitative methodologies. Similarly, the concept of "formlessness" is not adequately theorized: One wonders whether there would be substantively greater "form" in Western popular perceptions of terms such as "democracy" and "freedom." The precise relationship between formlessness and the broader problem of regime change and societal transition is not adequately spelled out; that is, to what degree is the so-called formlessness a structural characteristic of a society that has responded in traumatic ways to the challenges of modernity for well over a century? Or is it a local phenomenon confined to the postcommunist transition? The associated issue of the direction of causality is not adequately addressed: The relationship between institutional development and societal characteristics—the eternal tension in transition studies between state-centered and societally oriented approaches—is left hanging in the air.

The key test of any work studying political culture is the durability of the evidence and results. The bulk of

Alexander's research was conducted in the early 1990s, at a particularly traumatic time of institutional change, culminating in the violent confrontation between the presidency and the parliament in Moscow in October 1993. To what degree can his findings help us understand long-term patterns of development? The blend of political science and anthropological methods (although I doubt whether professional anthropologists would recognize his approach as in any way anthropological) does in fact produce results that transcend the narrow limitations of the time and place in which they were conducted. Although some of the claims for methodological originality are rather exaggerated, the study does help establish the contours of the formlessness (a contradiction that is recognized in the work itself), insisting that the experiences of democratization and marketization themselves contribute to shaping political responses; and thus primordialist approaches to the study of political culture are rejected. The enduring value of this work, however, perhaps lies not in the detailed analysis of the formlessness of the transition, but in the establishment of a framework in which we can begin to understand the process of "recreation" of a stable political order. This book takes us some way along this path but is constrained by its limited time horizon and its engagement in other debates. However, any attempt to understand the way in which tradition and innovation are being recombined in contemporary Russia will be the poorer without drawing on Alexander's theoretically sophisticated, methodologically robust and empirically rich work.

Public Health Policies and Social Inequality. By Charles F. Andrain. New York: New York University Press, 1998. 292p. \$50.00.

The Politics of Health in Europe. By Richard Freeman. Manchester, UK: Manchester University Press, 2000. 164p. \$69.95.

Governing the Health Care State: A Comparative Study of the United Kingdom, the United States, and Germany. By Michael Moran. Manchester: Manchester University Press, 1999. 196p. \$69.95 cloth, \$24.95 paper.

Accidental Logics: The Dynamics of Change in the Health Care Arena in the United States, Britain, and Canada. By Carolyn Hughes Tuohy. Oxford: Oxford University Press, 1999. 312p. \$45.00.

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International meetings about health-care issues—conferences, symposia, cyber-gatherings—have become something of an epidemic in the past decade. There is a brisk trade in the latest panaceas offered for the various real and imagined ills of modern medical care systems. When policy fixes fail in their country of origin, they are regularly offered to unsuspecting audiences elsewhere. Moreover, what travels as comparative analysis is often simply a collection of parallel descriptions of national health arrangements. So when there is a flurry of systematic comparative studies of health care by political scientists, a development illustrated by the four books under review, one ought to pay attention.

Ten years ago, there were relatively few political science texts on health care—the only truly comparative treatise by Odin Anderson was published in 1972, a book dealing with postwar medicine in Britain, Sweden, and the United States. (And Anderson was a sociologist.) It is not that health care was ignored, but the ferment was most evident in the growth of health *economics* texts.

Yet, over the last decade, there has been a literal explosion in comparative studies by political scientists and others. Academic conferences on comparative health studies have

attracted many scholars. A spate of comparative reports by international organizations such as the OECD and WHO has provoked commentary and controversy, as well as supplying a wealth of comparative data. Now we see a third stage, one of book-length treatment, four of which this review examines. (Two other works of comparative politics came to my attention too late for review here. One is the admirable 1998 portrait of Japanese medical care—contrasted with that of the United States—by John Campbell and Naoki Ikegami, *The Art of Balance in Health Policy*, 1998. The other is Colleen Flood's wide-ranging “legal, economic, and political analysis” of *International Health Care Reform*, 2000.)

One can evaluate this literature by, at a minimum, two standards. One is the degree to which the work advances our understanding of how and why various nations have developed their health-care systems. The second is the extent to which the analysis permits readers to draw plausible policy lessons—predictive and prescriptive—for the national systems studied? The issue for this essay is, How well do the four scholarly works of comparative politics under review satisfy these standards?

Tuohy's *Accidental Logics* stands out as a sophisticated, thorough, and insightful synthesis of comparative politics and policy in Canada, the United States, and Britain. As she makes clear in her title, the book concentrates on explaining patterns of policy continuity and change, buttressed by a thorough understanding of both the institutional details of modern health care and the demands of comparative political analysis.

Tuohy's central thesis is that a “common logic” dominates health-care policy cross-nationally. But, she argues convincingly, the working-out of that logic in any given country reflects the “accidents” of history, the combination of which gives rise to each country's particular national system. Tuohy examines the national relationships between the market and the state in health care along two common dimensions: the balance of influence among types of actors (state, private finance, and health-care professionals) and the mix of instruments of social control (hierarchy, market, and collegiality). This framework not only descriptively illuminates the medical care we see, but also enhances the reader's understanding of seeming puzzles in the tumultuous area of health care policy. Tuohy's discussion of the United States over the past two decades illustrates her contribution. She understands recent American health-care developments as a result of the intersection of “the logic of entrepreneurialism inherent in market-based systems” and the increasing “influence of private financial actors at the expense of the medical profession” (pp. 158–9). Her general medical-care discussion reflects as well a profound scholarly understanding of the complexity of professional regulation. And her analysis of professional autonomy highlights the centrality of physicians, a group that has influenced and continues to influence health-care policy everywhere in the world of industrial democracies.

Accidental Logics begins by noting the features of medical care that make it the object of intense policy concern, analyzing the pressures for change internal and external to contemporary national health arrangements. The speed of change in the three countries she studies intensively differs according to the particular “institutional mix” (defined as the degree of government hierarchical control, market forces, and professional collegiality) and the “structural balance” among state, medical professionals, and private financial interests. With this approach, Tuohy then illuminates the variation in the impact of reform ideas on policy practice in the three Anglophone nations she knows so well. Substantively, Tuohy gives us national portraits that conflict with much conventional wisdom: comparative stability in the basic policy framework of

Canada since the 1960s, tumultuous change in the United States in the 1990s, and a more limited degree of change in the world of British medicine from Thatcher to Blair.

Richard Freeman's contribution is of a different order. His book's scope is very ambitious. It is about,” as he states on the first page, “health, politics, and Europe.” The central question for the non-health-care specialist is whether the focus on publicly provided health care—which is what Freeman really has in mind, as opposed to the broader topic of health—illuminates European politics in especially striking ways. For health politics specialists, the issue is how the book's theoretical or empirical content advances the understanding of the subject.

Freeman's book is a readable, useful guide for the non-specialist to the shape of European health-care systems, their origins, major institutional features, and contemporary disputes. (By contemporary, I mean over the last two decades of the 20th century.) Indeed, the focus of the empirical chapters is the disputes about “re-forming” health care that have raged since the stagflation of the 1970s in France and Germany, Italy, Sweden, and Great Britain. Freeman's title might well have been the “politics of health reform in five European States,” emphasizing the pressures for change from those who use public health care, those who provide it, and those who pay for it.

The treatment of this subject is sensible, solidly documented, and very valuable for those trying to make sense of the very complicated, ever-growing arena of health-care funding, delivery, and regulation. The sequence of chapters begins with an introduction to the questions and the approach Freeman will take. It then moves to a very brief, but helpful sketch of European health-care politics in the century from 1880 to 1980 and takes up in comparative chapters the five national health-care programs. Freeman divides the substantive, descriptive chapters in two, distinguishing “national health services” (Italy, Sweden, and the United Kingdom) from what he conventionally labels as “social insurance systems” (France and Germany). Readers could use this dichotomy to analyze the health arrangements of other countries in western Europe, putting Holland and Belgium into the latter camp and Norway, perhaps, in the former one. Doing so would, however, raise the question of whether this particular classification is all that helpful. Does it really illuminate the political struggles over health care across Europe? Is thinking about policy arenas in terms of legal ownership/financial categories such as social insurance and national health service all that helpful in understanding what health policy matters are at issue and what patterns of resolution emerged in the Europe of the late 20th century?

The answer for this reviewer is simple. The formal designation of social insurance or national health service is but one of the potential factors shaping health-care politics and deserves no particular privileged status. Freeman concedes that the distinction is “not real” but “makes a wealth of information more manageable” (p. x). In fact, he regards the study of particular disputes in health care as warranting different analytical approaches, “necessarily eclectic,” as he puts the point (p. viii). So what the reader has here is an accurate sketch of European political/medical history, a well-informed summary of salient disputes in five of Europe's nations, and some interesting, but not fully developed approaches to understanding why health-care policies and programs have worked out as they have.

For scholars (and students) of comparative politics, it will be a valuable substitute for the outdated work of Odin Anderson, a useful companion to the descriptive, statistical portraits of the OECD, and a helpful companion to the many articles on particular disputes or national programs.

What the specialist reader will find disappointing, however, follows precisely from the virtues of the book for the general reader. This is an excellent synthesis of available understandings. But there is little that advances that understanding, or reveals why and how comparative analysis can make a substantial difference in how we either explain comparative policy development or inform policy disputes about health care with an understanding of the crucial political constraints that research has revealed. Freeman does make a good case for adapting theoretical approaches to the different disputes within medical care but does not provide an especially illuminating way to conceptualize that. He suggests rightly that, for both explanation and evaluation, the comparative method is as close to experimentation as social science is likely to get. But, there again, the justification of the comparative approach does not produce in practice an explanation of change and continuity that goes beyond conventional national accounts. For that topic, *Accidental Logics* is superior. Freeman's contribution, however, is no less worthy for being synthetic rather than a theory-building exercise.

Michael Moran's *Governing the Health Care State* takes as its central question the following: How can one explain the embrace by the Thatcher government of American-inspired market reforms of the 1980s and early 1990s? The puzzle arises because the apparent motivation of those reforms—the control of health-care costs—was something that the unified government of the NHS was already capable of doing. The answer to that puzzle, Moran argues, can be revealed only by cross-national analysis. And his investigation is part of a broader concern about why “in the early 1990s the health care systems of most of the advanced capitalist nations were reformed” (p. x).

Moran's book, like Freeman's, offers a standard, competent description of the main health-care features of the United States, the United Kingdom, and Germany. But the patterns emphasized are not as illuminating as promised. The interplay of health-care institutions and the state constitute what this book regards as its central “insight,” leading Moran to emphasize the analytical importance of what he terms the “health care state” (p. 10). This conception—health care as a subunit, so to speak, of the welfare state—defines the “closed loop feedback mechanism” of interaction between health-care interests (those of patients, providers, payers, drug firms, device manufacturers, etc.) and the political order. It is surely true that industrial democracies expend (publicly and privately) vast sums on health care. These in turn create a complex set of economic and political pressures. But to note that is not, in my view, to advance the reader's understanding appreciably.

The Moran analysis breaks out three elements of the “health care state”—consumption politics (access and cost), professional politics (expertise, ideology, and pressure groups), and production politics (health care as an industry commanding a substantial share of the GNP). The work is most helpful in dealing with consumption politics (chapter 3) and summarizes well developments in that sphere in the three countries. The other parts of the triumvirate are less well treated. For example, Moran unconvincingly regards the politics of professional regulation as simply what economists call “rent-seeking” by the medical professions, a “chance to gain competitive advantages in markets by securing preferential policy outcomes” (p. 14). There is no doubt that many regard professional self-regulation as simply window dressing for economic advantage. But one hopes for a more sophisticated discussion of such topics when issues of the quality of care are real and, in the case of the United Kingdom, a central feature of current criticisms of the NHS.

Moran returns in the end to his initial puzzle of why the United Kingdom turned in the latter 1980s to reform models

inspired, he claims, by U.S. intellectual entrepreneurs. His unremarkable conclusion is that “the form and direction of health care policy are responding to some forces deeper than pressure for cost containment” (p. 174). But Moran is unable to explain precisely why precisely the particular class of innovations appealed, although along the way the reader receives an overview of changes taking place in all three political and health-care systems. Descriptively helpful, this book promises more than it delivers in making sense of continuity and change in the health-care arrangements of these three democracies.

Charles Andrain's book on inequality and public health is the most ambitious of the works under review and the most disappointing. Andrain approaches health-care policies deductively. He treats the topic as derivative of three models of the modern welfare state, using Gosta Esping-Anderson's typology as his organizing analytic framework in Part I of the book. He attempts to describe and explain developments in eight countries, placing each in one of the three welfare-state models—entrepreneurial, organic corporatist, and social democratic. Canada and the United States are treated as instances of the first category; Germany, Holland, Japan, and France exemplify the second; and Sweden and Britain are examples of the social democratic mode. It is unclear whether Andrain uses these categories to characterize the respective governments *per se* as the predicate for understanding its health policies or whether, as he sometimes suggests, these models reflect his understanding of each country's health-care features (pp. 14–5). To the extent that it is the latter, one must wonder about the applicability of these models.

Consider the result for Canada. Its universal coverage, global provincial budgets, and bans on extra billing and supplementary health insurance place Canada, according to Andrain's scheme, in the same “entrepreneurial” category as the United States, with its hundreds of health insurance firms, millions of uninsured and underinsured, and no overall budget setting by government (p. 19). On the one hand, there is no doubt that Canada's economic system and national culture resembles the United States' more than it does Sweden's (Andrain's exemplar of social democracy). But that fact does not substantiate the claim that Canada's medical-care arrangements rest on core entrepreneurial values—as opposed to those of corporatism or social democracy. Indeed, Canada's hospital and physician insurance arrangements are among the world's most egalitarian, and I can think of no specialist literature that supports Andrain's conclusion here.

Andrain's mistaken treatment of Canada is worth emphasizing because it illustrates a key weakness of his approach to the comparative study of public policy, health, and social inequality. That weakness is empirical, the absence of a firm command of the literature on health-care systems. (His footnotes are extensive, but they come clustered at the end of paragraphs that do not link particular pieces of evidence with a specific argument. In that sense they reveal industry more than accuracy.) Canadians, we are told, “live under a more decentralized system than the Americans” (p. 36). This characterization reflects the powerful role played by provincial governments in administering Canadian Medicare. The implication is that substantial variation exists in Canada because of that provincial administration of a program whose core features are actually enshrined in federal legislation. The fact is that Canadians—across the provinces—have in their hospital and medical insurance a substantially common policy. But this does not imply—unfortunately it does for Andrain—that the United States is more centralized. By most measures America's nonuniversal “system” has a smaller role for government of any type, whether federal or state, and reflects substantial decentralization of health-care policy and

practice. The constraints of Andrain's deductive approach not only produces mischaracterization in this particular instance, but also undermines the credibility of his entire enterprise. Andrain begins with the presumption that "social inequality influences public health policies and their outcomes on people's health" (p. ix). That presumption, rather than a careful analysis of health-care policy, dominates the subsequent interpretations.

Even more fundamentally, this book rests on a conflation of public health and health-care policy. For Andrain, "public health policies" encompass all government decision making about health-care funding and allocation. At times, the term expands to include environmental, worker-safety, and labor policies. So what is presented is the work of a political scientist most interested in social inequality and governance, where health policy broadly understood is a vehicle for describing what is worthy and unworthy public policy. Social democratic governments, Andrain's logic suggests, "should promote the equality of workers," while entrepreneurial systems are presumed to reflect "fragmented power structure[s]" where "low income people must rely on public health programs for their health care services, [programs] based on means-tests [that] usually supply niggardly benefits" (p. 15). This logic, however, makes incomprehensible either Canadian Medicare or American Medicare.

Understanding health-care systems and how they fit into their political setting is demanding. It requires analysis that is both theoretically illuminating and substantively accurate. Judged by these exacting standards, the Tuohy book is an extraordinary work, concentrating more on explaining policy developments than designing a framework for evaluating reform options. The other three books fall short of her exacting comparative standard, but in quite different ways. Freeman's contribution is that of synthesis, not theoretical advance. In the case of Moran's work, the analytic contribution promises more than it delivers, though the book's comparative portraiture will be useful to many teachers and students. In contrast, the Andrain book is, to this reviewer, an unhelpful contribution to the comparative politics of health care.

The comparative politics field, however, gains from this expansion of scholarship into health-care politics. Not only is health care central to the fiscal status of most governments—consuming an average of approximately 9% of the GNP among OECD nations—but the arrangements of care prompt intense political conflict. The varieties of those conflicts are substantial—from the moral disputes over cloning, abortion, and assisted suicide to the intense labor struggles over hospital closures and union organization, from the ideological struggles over rationing to the distributive struggles over research funds. Charting and explaining these differences more fully remain on the agenda for future works on health care and comparative politics.

Cuba Today and Tomorrow: Reinventing Socialism. By Max Azicri. Gainesville: University Press of Florida, 2000. 396p. \$55.00 cloth, \$24.95 paper.

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Max Azicri writes with acumen on the Cuban revolution in the very different decades of the 1960s, 1970s, and 1980s, and he offers an insightful assessment of the changes and challenges of the 1990s. His objective is to answer the puzzle of what he calls the "Cuban miracle": the island's surprising survival in the face of the deep economic crisis associated with the collapse of the socialist bloc and the ongoing "punitive" policies of the United States. Azicri is not the only scholar to attempt an interpretation of Cuba in the 1990s. Susan

Eckstein (*Back from the Future: Cuba Under Castro*, 1994), Ken Cole (*Cuba: From Revolution to Development*, 1998), Julia Jatar-Hausman (*The Cuban Way: Capitalism, Communism, and Confrontation*, 1999), and Robin Blackburn, ("Putting the Hammer Down on Cuba," *New Left Review*, July-August 2000 (4):5-36) are among those who have examined the nexus of Cuba's past and future in the post-Cold War context. An explosion of travel writing also demonstrates the intrigue of contemporary Cuba, as does the list of new detective thrillers—some of them bestsellers—with a Cuba setting. Azicri's book has a distinctive place in this literary landscape.

Cuba Today and Tomorrow is a careful, thoroughly documented, and wide-ranging analysis of landmark events in Cuba and its international relations in the 1990s. The fact-filled journey begins with the collapse of the Soviet system and the disturbing social dislocations of adjustment in Cuba, including a resurgence of prostitution; moves on to the economic and political reforms that enabled survival and then recuperation; and explores Cuban-U.S. relations during the Clinton administration, Cuba's outreach to the reordered world, and Pope John Paul II's visit to the island in 1998. Azicri distinguishes between domestic and external "environments," and his "underlying theme" is "that Cuba is undergoing another radical social transformation under the revolution, different from what was experienced before" (p. 5). This radical transformation is "reinventing" Cuban socialism. A more away from the "total socialism" of previous Cuban strategy (and dreams) "is not a matter of choice" (p. 5), but other choices matter in the reinvention of socialism in Cuba.

Azicri catalogs the increasingly well-known reforms of the 1990s, all of which serve three objectives: "to resist and overcome economic collapse, to safeguard the socialist system, and to integrate the country into the world economy" (p. 304). Economic reforms reflect capitalist adjustments to Cuba's economic model, ranging from legalization of dollars and self-employment to new fiscal policies, the opening to foreign investment, and free trade zones. Obdurate problems of development—and socialist development—persist, but the policy achievements (and failures) of previous decades explain mid-decade recovery as well as the potential for successful reinsertion in the global economy (p. 129). Which paradigm Cuba will pursue as it leaves behind the "total socialist" model is not yet clear (p. 175), and Azicri advises an expansion of private entrepreneurial space as well as renewed state control of the social programs (p. 175) that temper the pernicious combination of scarcity, tourism, and new inequalities, in order to reaffirm the egalitarian commitments of Cuban socialism.

The political reforms of the 1990s are more modest, among them the revision in 1992 of 77 articles of the Cuban constitution, the acceptance of "believers" as party members, and the inauguration of direct elections for provincial and national representatives. Azicri explains that democratic centralism has been redefined (p. 114), and he warns that typical questions about representative democracy are not "germane to the organic structure of a socialist political system" (pp. 121-122). He identifies two crucial issues within the single-party system: expanding the base of the party and ensuring that its role is one of policymaking, not implementation (p. 122). "Sensible electoral and political reform would reinforce the system as a socialist democracy, while increasing its political legitimacy at home and especially abroad" (p. 128).

The dynamics of reform in Cuba are contextualized in Azicri's discussion of the complexities of finding allies and friends in the postsocialist world, of UN deliberations on human rights and U.S. wrongs, and of the Pope's historic visit. Tensions between Cuba and the United States figure

prominently in the external environment, including the unprecedented legislative aggression of the 1992 Cuban Democracy Act and the Helms-Burton sequel enacted after the two-planes incident in 1996. Although thorny issues of dissent, democracy, and development are indeed caught up in the interplay of Cuban exiles' actions, U.S. domestic politics, and official foreign policies that have harmed Cuba, Azicri advises that "most problems on the island should not be blamed on Washington" (p. 296) and "the reform process must continue uninterrupted regardless of hostile U.S. policies" (p. 307). Azicri recommends that the reinvention of socialism continue with a further opening to resurgent Catholic/Christian values. These would provide an ideal moral and ethical antidote for the "creeping materialism" associated with economic reform and would foster political reform by pluralizing official ideology and the ideational foundations of everyday life (pp. 304–5).

Azicri does not engage in debates about how to study Cuba and does not forge his argument in opposition to others' interpretations. He writes with regard to the nature and outcomes of reform: It "is in the detail more than at the macro level that the nature of things is defined" (p. 305). Indeed, much is quite familiar in the Cuba that Azicri presents, including his assumptions about Castro's leadership. "Power is personalized in him," and Cuba's political culture—a "civil (secular) religion"—is Castro's (p. 302). Less familiar, however, are the many claims for the achievements of the revolution—its strength and resilience, for example, or the "inventive policy making, grassroots ingenuity, and vital national character resources" that aided survival in the special period (pp. 19, 7). Azicri also focuses with uncommon respect on the refinements of Marxist theory in Cuba in the 1990s, and although he is ecumenical in the sources he uses and drives the study with detail rather than a framework, his conclusions advocate the dialectical method. In daring to take seriously the possibility of a better and still-socialist "tomorrow," Azicri problematizes Cuba and what we are willing to see and know about it. He may leave some important details out of focus, including AfroCubans and their religious orientations, but his contribution to renewed debate is timely, intelligent, and welcomed.

Elections and Democratization in Ukraine. By Sarah Birch. New York: St. Martin's Press, 2000. 212p. \$65.00.

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Elections and Democratization in Ukraine represents a valuable contribution to our understanding of the dynamics of party systems development in postcommunist politics. Unlike many volumes that rely on largely anecdotal evidence and narrative, this book is extremely well organized and systematic. There is a clear theoretical structure, and systematic evidence is employed to test specific hypotheses. Using some of the classic works on the development of party systems and relations between parties and voters to provide a theoretical framework, the author derives a set of hypotheses about characteristics that might affect voter choices over time.

The empirical scope of the study is quite impressive, covering elections from 1989 to 1998, including the referenda on the fate of the USSR and on Ukrainian independence, as well as two parliamentary and two Presidential contests. Data were collected from the Archives of the Supreme Rada of the Ukrainian Soviet Socialist Republic, archives of individual regions in Ukraine and the Crimean Republic, and local newspapers. In addition, the book relies heavily on extensive fieldwork, particularly data derived from a March 1998 voter survey.

The author's central question is, What forms long-term electoral cleavages in postcommunist Ukraine? In answering this question she argues that, contrary to the currently popular claim that the Soviet Union left in its wake an atomized society with weak social divisions, the Ukrainian electorate has, from the advent of competitive elections, exhibited a relatively stable sociogeographic cleavage structure, a structure which can be explained by social cleavage patterns that developed in the Soviet period.

In particular, the author emphasizes the importance and persistence of regionalism in post-Soviet Ukrainian politics. She divides the country into five regions—"West," "Right Bank" and "Left Bank" of the Dniepr, "South," and "East." She convincingly demonstrates continuing, strong support for nationalist parties, candidates, and positions in the West. On the other hand, voters in the South and East have tended to back parties of the "left"—communists, socialists, and agrarian socialists. In addition to the regional cleavage, there have been continued (although slightly declining) ethnic bases for electoral support of the Ukrainian political parties. Indeed, she shows that, over the years, proponents of statehood and Ukrainian nationalism have drawn largely from ethnic Ukrainian supporters, whereas opponents are derived mainly from Russian speakers or are of Russian descent. Further, religious factors also help define political cleavages in post-Soviet Ukraine, where the highest level of nationalist support is found among Ukrainian-Rite Catholics and Ukrainian Autocephalous Orthodox believers, whereas adherents of the Russian-affiliated churches (the Ukrainian Orthodox Church–Moscow Patriarchate and the Russian Orthodox Church) tend to support the Left parties. These ethnic, regional, and religious cleavages have remained remarkably stable over time, contributing to continued debate within Ukraine over the nature of the state. A somewhat less salient cleavage structure (although increasingly important) exists concerning the nature of the economic system, which manifested itself particularly after the parliamentary election of 1998.

One of the more interesting findings of the book is that there appears to be little evidence to suggest that the underlying cleavage structure has molded itself closely to that of the party system. In other words, despite the existence of fairly stable patterns of sociopolitical cleavages, these are not matched by parties that represent these patterns of political cleavages. Indeed the parties in the Ukraine are largely conglomerations of shifting coalitions of personalities, without strong attachments to particular social constituencies (a pattern of development which is not unlike that in many other postcommunist states, such as Russia). In part, the author suggests that the evolution of the party system has been limited by the small number of contested elections held in Ukraine compared to other postcommunist states. In addition, there also exists a popular aversion to party politics, as such, and a tendency of many voters to vote on the basis of the nonparty attributes of candidates. This has led to the weakening of the ability of parties to match particular patterns of social and political cleavage.

This argument is consistent with the observations of many other scholars who have noted similar patterns of development in postcommunist Russian and Eastern European politics. Indeed, to a large extent, the pattern of Ukrainian party development is not very different from the patterns found elsewhere—that despite the existence of persistent voter cleavages, the party system does not match these cleavages. Perhaps one reason for this (a reason noted by other scholars but not emphasized by the author of this book) is that voting on nonparty attributes has something to do with the fact that the Ukrainian system (as in Russia) has maintained

presidentialism coupled with single-member district elections. Both of these institutional characteristics tend to favor individual personalities over party programs in electoral competition.

Another minor problem with this book is the absence of a discussion of party behavior in the Rada. This would have been useful in discerning whether distinct party orientations were emerging in parliament, providing an important corollary to the author's analysis of party development on the ground (among voters and candidates). Given that early parties in the West grew out of the legislatures as coalitions of political elites, we might expect a similar process of development in the Ukrainian parliament. Thus, some analysis of political elites within the Rada would have provided a more complete picture of party development in the Ukraine. This would have given greater insight into party system development (the focus of Chapter 7) than viewing party development only from the perspective of the "party on the ground."

Finally, the conclusion is very short and offered almost as an afterthought—there is some attempt to discuss the findings in light of larger theoretical issues, but not much more. What would have been of great benefit is a discussion of the findings in some comparative way—particularly in the conclusion. Indeed, given the substantial amount of work, among both East European and Western scholars, who find remarkably similar processes of the development of the electorate in countries as diverse as Russia and Lithuania, the Ukrainian case represents yet another example of the mismatch between party development on the ground (among voters) and party actors that comprise the postcommunist party systems.

Nonetheless, these rather minor omissions do not detract from the high quality of this book. In particular, this book will prove to be indispensable reading for scholars who seek to employ the systematic techniques derived from political science to understand better the realities of postcommunist politics.

Cycling into Saigon: The Conservative Transition in Ontario.

By David R. Cameron and Graham White. Vancouver: University of British Columbia Press, 2000. 224p. \$75.00.

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One defining feature of a constitutional democracy is that the defeat of the governing party in a general election leads to a peaceful, orderly, and more or less routine change of government. Given the inherently combative nature of electoral politics and the potential for disruptive action by the losing side, this is no small achievement, and when it happens it is justly celebrated. Transitions in constitutional regimes, moreover, provide a unique if momentary window through which to view the mainsprings of political power. This volume is a study of the 1995 transition from New Democratic Party (NDP) to Progressive Conservative (PC) rule in the province of Ontario. It makes an important contribution to the sparse literature on transitions in Canada and in parliamentary regimes generally. For students of comparative government, it usefully complements recent work on presidential transitions in the United States, particularly Charles O. Jones, *Passages to the Presidency: From Campaigning to Governing* (1998).

In the United States, a change of president involves an elaborate and prolonged process that is controlled by partisans and results in thousands of offices changing hands. In Britain, a change of government is swift and unceremonious: In 1997, moving vans arrived to remove the personal effects of the defeated prime minister from Number 10 Downing Street less than twenty-four hours after the election. The

changeover of personnel, however, is limited to ministerial offices and ministers' political staff, and the mechanics of the transition are handled by career civil servants. In Ontario, as in the Canadian federal government in Ottawa, transitions are somewhat slower and more politicized than in Britain, although there is no turnover of unelected officials comparable to that which occurs in Washington following a change of president or in most states following a change of governor.

The broad theme of this study is that transitions can be done well or badly, and how they are done has a major effect on an incoming government's performance—specifically, its ability to carry out its agenda. It would have been helpful if, at the outset, the authors had more clearly identified and ranked their criteria for evaluating transitions. Instead, it gradually becomes clear from their analyses of three transitions (1985, 1990, and 1995)—the first characterized as a modest success, the second as an abject failure, and the third as a brilliant triumph—that they attach primary importance to the quality of the working relationship established during a transition between politicians and senior public servants, since a transition "centrally involves taking control of the public service" (p. 151). In order to achieve a productive working relationship, they conclude, it is essential for opposition political parties with a chance of winning power to engage in thorough, systematic, preelection transition planning, and incumbent governments must authorize preelection planning exercises by the bureaucracy so that it, too, will be properly prepared if the election results in a change of government.

In assessing transitions, the authors rely heavily on material gathered through unstructured interviews (presumably, since the questions asked are not revealed). These were conducted on a not-for-attribution basis. Among the principal subjects were nineteen civil servants (ten deputy ministers and nine others of an unspecified lesser seniority) and thirteen members of the two opposition parties' transition teams (eight PC and five Liberals). Only three politicians gave on-the-record interviews: two minor PC ministers and the former leader of the Liberal party, Lyn McLeod, who resigned following her party's electoral defeat. It is not reported how many potential respondents were approached but declined to be interviewed. The lack of any explicit attention to methods is a serious weakness in a study of this kind. Nevertheless, the authors' key findings are amply supported by insightful (if also anonymous) quotations that only could have come from well-informed insiders.

In sum, they find that Ontario's NDP government of 1990–95 was handicapped from the outset by a disastrous transition, whereas the PC government that took power in 1995 benefited from an outstandingly well-executed transition. One of the most appealing features of the book is the detailed, incisive, and eminently readable account of these events.

The failed transition of 1990 is clearly explained and appears in retrospect to have been virtually inevitable. The NDP never expected to win the election and did no serious transition planning; it was confused over its goals and priorities; and some of the newly appointed NDP ministers and political staff were left-wing ideologues who were deeply mistrustful of what they saw as the inherent conservatism of the public service. When the premier appointed his notoriously partisan chief political advisor to the post of Cabinet Secretary (the head of the Ontario public service) and parachuted a number of other partisans into the higher echelons of administration, career bureaucrats understandably interpreted these moves as signals that the NDP was intent upon politicizing the public service. In one of the most interesting sections of the book, "Politicizing the Bureaucracy" (pp. 44–9), the authors come to the surprising verdict that the bureaucrats' case against the NDP is not proven. Nevertheless, as their study demonstrates,

the antagonistic atmosphere during the transition had a lasting and ultimately debilitating effect.

The 1995 transition, which is the main focus of the study, is dealt with in an equally illuminating and readable fashion. It is also an original contribution to scholarship in the field. The authors show how the PC, from the very beginning, approached the election with "a governing mindset." Transition planning was given a high priority by the party leader, Mike Harris, who as early as January 1994 put in place the core members of a capable and experienced team of transition advisors. Consequently, when the PC won an upset victory it came to power ready to govern. It helped that the party also had an unambiguous right-wing agenda that it was determined to push through, and that Harris took immediate steps to placate the bureaucracy. As the authors state, the PC transition team "viewed the incoming government's relationship with the public service as the most important element in taking power effectively, and it sought and received a good deal of advice from former public servants who knew the system and the senior players well" (p. 85).

The authors also give due credit to the Cabinet Secretary, whose appointment by the NDP had provoked outraged cries of "patronage" and "politicization." It is an ironic ending to the story (and to his career as a bureaucrat, since he was fired by the Conservatives immediately after the election) that he contributed greatly to the success of the PC takeover by doing a thoroughly professional, nonpartisan job of preparing the bureaucracy for a transition.

A concluding section, "Practical Lessons and Recommendations" (pp. 153-9), neatly sums up the authors' advice on transition planning. It is so wise and plainly stated that their book will almost certainly become essential reading for future transition teams in Canada, and it merits attention in other parliamentary democracies as well.

The book's obscure title is not explained until page 102. It derives from an arch joke by an unnamed PC transition planner. Upon entering the deserted Cabinet office the day after the election, he recalls, he had the eerie sense that bureaucrats were watching him from behind the potted palms. "I felt," he said, "like the first Viet Cong soldier cycling into Saigon after the Americans had left."

The First Chinese Democracy: Political Life in the Republic of China on Taiwan. By Linda Chao and Ramon H. Myers. Baltimore, MD: Johns Hopkins University Press, 1998. 371p. \$47.00.

Democratization in China and Taiwan: The Adaptability of Leninist Parties. By Bruce J. Dickson. Oxford: Oxford University Press, 1998. 276p. \$69.00

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These books address the same issue, the democratization of Taiwan, but use diametrically opposed analytic frameworks. The results are individually unsatisfying but, paradoxically, complementary.

The key question for Bruce Dickson is whether ruling Leninist parties can evolve toward democracy without collapsing? Working from the assumption that both the Kuomintang Party (KMT), which dominated Taiwanese politics until 2000, and the Chinese Communist Party (CCP) on the mainland are "Leninist" by nature, he presents a detailed comparison of their evolution. He concludes that the democratization of Taiwan suggests a positive response to this central question. There, a reforming leadership, aided by favorable international and domestic conditions, responded to growing societal demands by sharing political power. On the mainland, a party dominated by nonreformers who per-

ceived an unfriendly international and domestic environment choose to suppress societal demands. The recalcitrant record of the CCP leadership, Dickson concludes, offers little hope for its ability to adapt fully.

Linda Chao and Ramon Myers explicitly reject a depiction of the KMT as Leninist. They seek to explain the uniqueness of democratization in Taiwan when compared with other Chinese societies in the twentieth century. They present a typology of three different kinds of relationships between the political center and the rest of society: "a subordinated political center" and a political system that responds to society (democracy); an "uninhibited political center" and a society that "passively complies" with the political system; and an "inhibited center" that "does not control many of society's resources" and "gives more leeway to other loci of decision-making" (pp. 8-9).

Whereas Dickson argues that democratization was the result of KMT success in overcoming the limits of its Leninist origins, Chao and Myers view it as the result of the decision by the ruling elite to establish, from the outset, a non-Leninist basis for the party. Rather, they established an inhibited political center or limited democracy, characterized by a commitment to full democracy but without a timetable, and allowed limited oppositional activity. According to Chao and Myers, Taiwan followed a path from an inhibited to a subordinated political center, whereas the CCP has only recently evolved from an uninhibited to an inhibited political center. Thus, rather than ultimate failure to democratize on the mainland, the authors suggest that the two societies may gradually be converging as they follow similar developmental trajectories.

A review of this length does not permit a full assessment of the arguments. There is much that is useful and intellectually stimulating in both studies, especially the emphasis on the evolution of the electoral system on Taiwan. Yet, each fails in its response to the "big question" it poses because the premise from which each proceeds is flawed.

Although he recognizes differences between the CCP and the KMT and acknowledges that his comparison is inexact, Dickson does not make a convincing case for the KMT as a Leninist party. To be sure, at its founding in the 1920s and after its arrival on Taiwan, Leninist trappings were evident, but the KMT was a very selective and truncated version of a Leninist party. Specifically, what was largely absent was commitment to the central orthodoxy of Lenin's thought: the dogged insistence that the "consciousness" of the party/state should guide, not respond to, mass demands ("spontaneity"). Rather, the KMT was guided by a jumbled ideology (with a vague suggestion of political tutelege) and was an incomplete Leninist organization.

If one proceeds from this premise, it is not difficult to understand why democratization came to Taiwan and not the mainland. The ruling elite and their party organization in the People's Republic of China were/are fundamentally Leninist, whereas the system on Taiwan was Leninism *manqué*. In this light, Dickson's conclusion seems somewhat more modest than he suggests: Leninist states cannot adapt, and incomplete Leninist states can.

Similarly, the Chao and Myers depiction of the KMT as committed to democratization from the time of its arrival on Taiwan is very hard to accept. First, the authors never fully develop the content of the "Sunist ideology," which they claim was the basis of this commitment. Second, their use of Chiang Kai-shek's rhetoric to demonstrate a commitment to democracy is simply far-fetched. He made such statements even as his oppressive government decayed on the mainland. In the late 1940s and early 1950s this rhetoric became more intense as Chiang sought to gain American support for the idea of a "free China." More important, the

practice of “limited democracy” during his rule was certainly far more brutal and less accommodating than the sanitized version presented by Chao and Myers. One could equally—and perhaps more accurately—term this period limited autocracy.

These two very different books complement each other. Myers and Chao remind us that the KMT was always a divided party under foreign pressure and institutionally weak in all but its coercive capacities. It was certainly not as Leninist as the CCP. Yet, elements of Leninism were there, as Dickson suggests. Moreover, Dickson’s identification of the three elements that contributed, over time, to Taiwan’s democratization, when stripped of their Leninist assumptions and comparisons with the CCP, provides a far more sophisticated conceptualization of the process presented in great historical detail by Chao and Myers, but placed by them in the less useful paradigm of limited democracy.

Theories of Comparative Political Economy: New Paradigms.

By Ronald H. Chilcote. Boulder, CO: Westview, 2000. 316p. \$28.00 paper.

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Ronald Chilcote is a leading radical political economist, and this book is a fine illustration of his well-earned reputation. It is a comprehensive survey of most of the major writers and themes that have dominated the literature on liberal and radical political economy, organized historically and by schools of thought around key concepts: class, state, imperialism, and transitions. The core of the book focuses on these four conceptual areas.

In the chapter on transitions, Chilcote outlines the major shifts—from feudalism to capitalism and from capitalism to socialism. The scholarly debate ranges from Paul Sweezy’s notion that feudalism was largely a result of the emergence of a merchant class engaged in international trade; to Maurice Dobb’s theory that the transition was the result of internal class conversions; to Robert Brenner’s argument for the centrality of peasant-based class struggle. The diverse explanations posed by Chilcote highlight the complex reality of transitions and call into question the simplistic and schematic Rostovian stages of growth and Wallersteinian so-called world system approaches.

Chilcote’s own approach to political economy is most clearly revealed in his discussion of class analysis perspectives. He recognizes the limitations of “class reductionist” approaches but argues very persuasively for examining the interface of class structure, productive processes, and state institutions in order to understand long-term, large-scale change. An additional benefit is the annotated bibliography at the end of the chapter, which includes many of the major authors in the debate.

In the chapter on the state, Chilcote suspends his class categories and refers to the liberal state and the progressive state. He returns (p. 42) to class categories in assessing the dichotomy, but without elaborating on the distinction between regime and state—a problem endemic among many theorists. Chilcote’s summary of theorizing by Philippe C. Schmitter and Bob Jessop would have benefited from the extensive critical literature that highlights the many pitfalls of using the concept of corporatism to describe fascist, social-democratic, and nationalist-populist regimes.

The discussion of neoliberal views on the state accurately describes ideological principles (favoring a minimalist state) but does not analyze neoliberalism’s powerful statist thrust: the state role in subsidizing capital, bailing out banks, intervening in civil society to limit the role of labor, and so on.

Neoliberalism is most clearly a very current perspective and requires full exposition.

Chilcote provides a wide-ranging discussion and classification of the rich and sophisticated debates on the state by Marxist scholars. He evaluates and compares the Marxist, institutional, instrumental, regulation, and feminist perspectives. Surprisingly, there is no examination of the imperial state or of what some writers call the globalized state, although these topics are at the cutting edge of debate in the new millennium. The imperial state plays a major role in the world market by establishing new trading rules for expanding the influence of its multinational corporations and by directing international financial institutions to bail out crisis-ridden regimes and impose structural adjustments that facilitate privatization and buyout by Euro-American investors.

Chilcote’s exposition of the capitalist-democracy debate provides excellent background on the historical and theoretical disputes between liberals and Marxists. Once again, however, a clear distinction between regime and state would help explain the apparently contradictory position of U.S. policymakers toward democracy in Latin America. In transition from military rule to democracy, the United States supports regime changes when the apparatus remains intact, but it opposes democratic changes when the apparatus is dismantled and replaced by a revolutionary state. The pertinent question is not whether capitalism opposes democracy but whether the transition to democracy entails changes in property relations.

The chapter on theories of imperialism, as distinct from the imperial state, is probably the strongest in the book. Chilcote provides a highly informative comparative-historical discussion of imperialism from Marx, to Schumpter, to those he describes as the contemporary analysts of imperialism, including Paul Baran, Paul Sweezy, Harry Magdoff, and Eric Hobsbawm. Various schools have focused on issues of unequal trade, foreign investment, and/or the role of multinational corporations, whereas newer schools address the problem of the internationalization of capital (p. 235). Mention is made of globalization theory, but the burgeoning literature in favor of an interdependent world in which stateless corporations govern through international financial institutions is largely ignored. Although globalization theory is the hegemonic perspective, it does not lack for critics, and certainly its claims to have eclipsed the imperial paradigm are short on data and weak in accounting for the unipolar world that emerged after the collapse of the U.S.S.R. This highly informative chapter would have benefited from the inclusion of globalization theory.

The survey of debates and theories and the classification of schools of thought on the major historical changes in the nineteenth and twentieth century provide a useful starting point for developing new theories and approaches to the problem of imperialism. Moving from theory to current policies and structures of the international political economy, one discovers an enormous gap between abstraction and practice. For example, neoliberal or free-market doctrines circulate widely and are formally embraced by Washington and Brussels, but the actions of these imperialist powers more closely resemble mercantilist than genuinely liberal policies. Although the United States demands from its Latin American trading partners the elimination of trade and investment barriers, it upholds and enforces quotas on agricultural commodities, textiles, steel, and a host of others. In addition, it applies antidumping regulations in a very loose and haphazard manner, which leads many countries to suspect that these are a “non-traditional” form of protectionism.

Massive state subsidies to U.S. agriculture and technology (\$30 billion in the case of agriculture) allow U.S. multinationals and exporters to capture markets, but the U.S. government

pressures the International Monetary Fund to enforce budget cuts in Latin America directed at similar subsidies. The Bush administration's Free Trade Area of the Americas Treaty would consolidate U.S. economic dominance in the hemisphere based on the mercantilist relations currently in place. Neither Samir Amin's *Accumulation on a World Scale* (1974) nor Immanuel Wallerstein's world system approach provide any empirical or conceptual guidance in understanding the particular forms, policies, or effects of neomercantilistic imperialism. I am suggesting that Chilcote's book would have benefited from testing the various theories of international political economy with some contemporary case studies in order to allow readers to evaluate the explanatory power of the competing theories.

This book is a much needed antidote to conventional political economists who ignore the issues of imperialism, social class, and large-scale, long-term changes. When the next major U.S. military intervention takes place, I hope that the major professional journals will not ignore the imperialism hypothesis, as was the case during the U.S.-Indochina conflict. By broadening our analytical vision and introducing key concepts, Chilcote does a great service to the study of comparative political economy.

Israel and the Politics of Jewish Identity: The Secular Religious Impasse. By Asher Cohen and Bernard Susser. Baltimore, MD: Johns Hopkins University Press, 2000. 167p. \$36.00.

The Politics of Religion and the Religion of Politics: Looking at Israel. By Ira Sharkansky. Lanham, MD: Lexington Books, 2000. 161p. \$40.00.

Yael Yishai, *University of Haifa*

Religion has played a prominent role in Israel's history. The country was founded in 1948 with a declaration of being a Jewish state, not only a state for the Jews. This definition has been the source of two thorny problems. The first involves the relations between state and religion, the second the relationship between two communities, namely, the secular majority and the religious minority. The two books under review tackle these problems in a fascinating manner. They complement each other by presenting different aspects of the intricate religious issue in Israel. They also contradict each other by offering conflicting conclusions based on historical and contemporary analysis.

Sharkansky focuses on the relationship between religion and politics. His point of departure is comparative. Judaism is not unique, and politics in Israel is not exceptional. There are, according to his thesis, rules that govern the relationship between the two domains. His major argument is that religion and politics interpenetrate and actually resemble each other, having much in common. Neither can claim to have a monopoly of virtue or to be free of evil. Both religion and politics attract majorities, most of whose members remain loyal across time; both have their mainstream and their extreme elements. Leaders in both spheres tend to mix the promotion of spiritual and symbolic goals along with material payoffs for the faithful. Both are creative, inventing new doctrines and rituals, despite claims about absolute values; both attempt to distinguish themselves from their previous allies and current antagonists. In both religion and politics ambiguity is part of success and failure. The essence of the similarities lies in the policy arena: Religious issues are often the subject of political dispute. Religious demands, however, are not fully met because they come up against one another and because they encounter secular or antireligious opposition. The result is generally a stand-off in which nei-

ther religious nor antireligious activists can overcome the other.

This thesis is widely demonstrated by Israel, with its long history of a "thick" mixture of politics and religion. In a breathtaking journey through Jewish history and contemporary politics, Sharkansky demonstrates his thesis and shows that dispute over religious issues is a chronic phenomenon. At stake are problems of conversion, of personal matters, of observing Sabbath and holidays, and of the composition of religious institutions. Religious parties, described in detail in the book, exert demands that, if yielded to, may turn Israel into a *halachic* state, governed by religious law. This, however, has not been the case. Although religious issues are often on the political agenda, neither religious nor secular groups can overcome the other. The agenda of both proponents and opponents of religion is not translated into public policy.

According to Sharkansky, the reasons for this failure are grounded both in the universal attributes of the commonalities between politics and religion, on the one hand, and in the particular characteristics of the Israeli scene, on the other. The following reasons are prominent. First, although religious groups unite in seeking to advance their interests, they also compete with one another. Second, Israel as a state is so imbued with religious values that it is difficult to define the labels "religious" and "secular." Not only does a substantial share of the population consider itself "traditional," but also daily practices of individual secular Israelis are embedded in religious symbols, constituting part and parcel of the national culture. Third, the fact that no party has ever won a majority in the Knesset elections has served to mitigate the conflict between the secular and the religious. Fourth, surrounded by hostility, Israel is forced to maintain a degree of unity in order to survive. Politicians' pursuit of consensus and conflict minimization also contributes to easing the tensions. The result is that neither religion nor politics has gained total victory or encountered total failure.

Sharkansky's book makes an important contribution to understanding both the religion-politics nexus and the problems of religion in Israeli politics. The richness of data and the abundance of issues covered are overwhelming. In a brilliant exploration across time the author does not skirt any religious issue raised on the public agenda in Israel, be it trivial or major. The fit between the theory, captivating in its originality, and the case study is impressive. This is a thorough, innovative, and important work.

Cohen and Susser tackle the problem of religion in Israel from a different angle, focusing on the relationships between the two camps, the secular and the religious. Their major thesis contradicts that of Sharkansky. The picture they paint is not at all optimistic. In fact, they lament the decline of consociationalism in Israeli politics, implying that the secular-religious cleavage is in a new and unprecedented perilous phase. Not only are past animosities part of present realities, but also important changes have taken place in the character, context, and personal aspects of the religious-secular confrontation. In the authors' view, something qualitatively different has occurred in recent years, and there is a mounting potential for destructive crises along the religious-secular divide. "The language of consociational reciprocity, compromise, and mutual adjustment is quickly giving way to strident talk of total victory" (p. 139).

After elaborating the principles of consociationalism and the conditions that favor its emergence, Cohen and Susser adapt their conceptual framework to the Israeli scene by emphasizing the moderating and compromising effect of these arrangements, particularly in a society riven by deep cleavages. They then delve into Israel history, demonstrating the efficiency of consociational arrangements in the first

generation, from the state's independence to the change of power in 1977. Admittedly, even in that peaceful period there were thorny and highly disputed problems. The authors give a detailed account of these, including the drafting of the constitution, military service of religious students and women, observance of Sabbath and holidays, religious education, and the formidable problem of who is a Jew. These problems were dealt with in the past in line with the consociational spirit, namely, by mutual adjustments and accommodation.

From this point onward Cohen and Susser differ with Sharkansky. They maintain that the second generation witnessed an enhanced power of radicals, ideological transformations both in the secular and the religious camps, and most important, a growing correspondence between Jewish identity and the territorial issue; all of these have eroded consociationalism in Israel. The causes of breakdown are grounded in the political, ideological, and social domains. Succinctly put, the transition from a dominant-party system to a balanced two-bloc system with the growing empowerment of the religious, especially the *Haredi* parties, is the first reason. These parties are no longer minor groups with limited assets but pivotal parties able to determine the composition of the ruling coalition. Changes have occurred also within the religious camp, and there is growing dominance of the *Haredi* over the national religious party. The dramatic convergence of religiosity with militant views regarding the future borders of Israel has produced overlapping cleavages in Israel's public life. Ideology, according to these authors, has become more prominent.

Contrary to Sharkansky, Cohen and Susser believe that "religious values are understood to be absolute, and the tendency to present them in ultimative fashion is, therefore, only to be anticipated" (p. 41). Radicalization of demands, backed by deepening social rifts and by effective use of the political constellation, is the main reason for the crumbling of the consociational arrangements that have kept the religious-secular rift at bay. The broadening of judicial intervention in politics and the constitutional revolution have added fuel to the flames, making the two camps drift apart. In addition, demographic, cultural, and religious transformations have put consociationalism under pressure. Among the most noticeable are the growing influence of the media, the growing insistence on "good government," and the immigration of many non-Jews. Cohen and Susser have written an excellent book, well organized and highly systematic. They present their argument clearly and concisely and provide ample evidence to prove their point.

The two books have some omissions. Sharkansky overlooks the radicalization tendencies in the religious-secular rift. Assuming that religion and politics are alike does not mean that they cannot clash. Although, as he suggests, both invoke compromise, the process has its drawbacks and shortcomings. By presenting the grand design of the religious-politics nexus, he pays too little attention to undercurrents.

Cohen and Susser do the opposite: They rely on contemporary events to explain processes of a much broader scope. A good example is their reference to the peace process (p. 72), which has been nipped in the bud. Both books disregard civil society, whose activity has both invigorated the consensual spirit and fueled the flame of extremism. Cohen and Susser refer in passing to "grassroots initiatives that mobilize support across communal lines, [and] detour the normal political leadership" (p. 87), but activity appears to be flourishing. A plethora of movements and associations are active on the scene, which is no longer monopolized by political parties and elites.

Despite these slight oversights the two books are mandatory reading for anyone interested in Israeli society, which ex-

cellently exemplifies the relationship between the two leading forces in human history, politics and religion.

Transitional Citizens: Voters and What Influences Them in the New Russia. By Timothy J. Colton. Cambridge, MA: Harvard University Press, 2000. 324p. \$65.00 cloth, \$24.95 paper.

Unexpected Outcomes: Electoral Systems, Political Parties, and Representation in Russia. Robert G. Moser. Pittsburgh, PA: University of Pittsburgh Press, 2001. 183p. \$19.95.

Sarah Oates, *University of Glasgow*

As the Soviet regime recedes farther into the past, two types of scholars are now working hard to put the post-Soviet experience in comparative context. Some are those who built their careers on a study of the Soviet Union and have now significantly expanded on their work, while another group comprises scholars of the post-Soviet regime who completed their dissertations after the collapse of the Communist regime. These two books represent some of the best of both of these groups, and both are important in their scope in bringing Russian politics into one of the most important fields in politics, namely, that of elections, parties, and voters.

Timothy Colton's latest work on the former Soviet Union, *Transitional Citizens: Voters and What Influences Them in the New Russia*, breaks ground into new and exciting territory. He imports models of voting behavior, drawn mostly from the U.S. literature, into a postcommunist country. After a careful literature review and construction of a model, Colton applies the model to Russian voting behavior, using a series of three national surveys conducted in 1995 and 1996. The results of this methodical and groundbreaking work are engaging and fascinating and represent a major step forward in what social scientists can reap from studies of postcommunist states. The premise of Colton's book is that study of postcommunist politics has not always been informed by comparative theory and most work on voting behavior in Russia has not used a fully cross-national model.

Colton orders his research into several themes, including an examination of the socioeconomic determinants of vote choice; the development of partisan allegiances to parties and candidates; the impact of attitudes on vote choice; and what role personality, retrospection, and perspective evaluations play in elections. Throughout, he compares his results from the parliamentary elections to that of the presidential elections. Colton finds more similarities than differences in Russian voting behavior when comparing it with results in the West. For example, it is clear that socioeconomic characteristics such as age, rurality, and education have affected vote choice, particularly for the well-established Communist Party of the Russian Federation. In addition, there is compelling evidence for nascent partisan identification. Issues appear to have an impact on vote choice, as do evaluations of parties and candidates as well as evaluations of their past and future performance.

The book by Colton, who has written extensively on the Soviet Union and Russia, is a wealth of information for those who study Russian voting behavior, dealing with issues ranging from contact from campaign workers to the evaluation of parties on particular issues. The panel design of the survey allows Colton to make a convincing case of the stability of opinion on many issues. Colton's experience in studying Russia and his earlier work on public opinion and voting behavior are clear in the careful question design for the survey. For example, Colton makes a distinction among weak, moderate, and strong partisan identification, which is particularly important

in understanding the origins and strength of possible party identification in a postcommunist state. In his study of the impact of attitudes about the economy, he is careful to clarify that Russians are not dealing with relatively small issues of policy but are coping with global change in their polity.

Colton deals deftly and well with the bewildering range of Russian political parties, election laws, and candidates, yet the book raises several points that offer scope for further research. For example, his survey data indicated that those who were well educated and more attuned to politics were more likely to be influenced by the media in elections. This raises two interesting questions. Although Colton sees this as positive, it also suggests that the media is immensely powerful if it has more influence among the most politically sophisticated. In addition, the book does not differentiate between the highly polarized state and commercial television broadcasts. Colton notes that there is a lack of trust in television; further studies have found that there is a high level of trust in the prostate, state-run media but not in the commercial media, which championed the case of some contenders for power in the 1995 elections. Meanwhile, he does see media as particularly important in Russia (p. 60): "The question of media impact is more piquant in protodemocratic Russia than in a settled democracy because of the disproportionate leverage the state, and business interests affiliated with it, have over mass communications."

In *Unexpected Outcomes: Electoral Systems, Political Parties, and Representation in Russia*, Robert Moser uses Russia as a test case for hypotheses about the impact of electoral systems on party formation. Moser eloquently argues that Russia shouldn't be disregarded because the outcomes of electoral design were different from those predicted by theory. Rather, the "conclusion is simple, yet surprisingly absent from much of the neo-institutionalist research: context matters" (p. 4). Thus Moser makes a careful study of the Russian case to provide convincing evidence for his theory. Yet his argument is wider than a study of Russia or even the role of institutions in the formulation of democracy in postcommunist states. As he writes, it is "better to integrate the study of less developed democracies into comparative politics and develop hypotheses that describe and explain patterns of behavior found in these contexts" (p. 5). In this attitude, Moser joins many of the "new wave" of scholars of Russian politics in seeing the importance of integrating the Russian case into general comparative politics.

Why does Russia provide this laboratory? The irony is that Russia's electoral system is informed by political science research (as it now is a subject of the same). Faced with the need to create a party system in a matter of months after the revolt of parliamentarians in 1993, Russia's political advisers chose to implement a mixed electoral system for the 1993 parliamentary elections. Half of the 450 members of the lower house (the Duma) would be elected through a national party-list contest and half would be picked through first-past-the-post races in 225 single-member districts (SMD) across Russia. Moser considered this institutional design a perfect opportunity to study the impact of different electoral systems on party formation (and, to a lesser degree, on voting behavior). However, he is careful throughout the book to note that the Russian context also will inform the nature of the political "game." Moser does find several elements to the formation of Russian political parties that he considers somewhat unique. Although he feels that other third-wave democracies aside from Russia have a high electoral volatility, weak party identification, and little party organization (p. 31), what is distinct in Russia is the large number of independent candidates who contest and win elections. But rather than perceiving this as an argument for making Russia *sui generis*, he

writes, "Given that weak party institutionalization is a common feature of third-wave democracies, the experience of the Russian case may have broader implications for many new democracies."

In addition, Russia's party-list system defies the expectations of certain classic political theories, namely, Duverger's law and the Downsian spatial analysis of party preference. Rather than dismissing Russian politicians or voters as irrational or immature, however, Moser makes a careful analysis of possible rational preferences for an explosion of political parties by the 1995 elections. He finds that "party proliferation in the PR tier seems in part to be one of hidden rationality caused by the many competing incentives of the mixed electoral system" (p. 39) like nested games. Moser also argues that the proliferation of candidates in the SMD tier was caused by the weakly institutionalized party system. In Chapter 6, Moser tackles the puzzle of why Russian voters have twice elected a fairly reactionary Duma and a relatively reformist president a few months later. Moser argues that it is not irrational or even particularly unformed voting preferences; rather, the differing outcomes are the result of the institutional design of the electoral system. Moser uses an analysis of voting patterns for broad ideological camps to show that there was relatively little change from the Duma elections of 1995 to Yeltsin's election in 1996; what changed "were the rules of game that structured the vote choice and translated the vote into political power" (p. 97).

However, in this analysis some of the context does in fact matter, in that it is difficult to categorize particular party groupings, as Russian parties are often based less on ideology than they are on personalities or the needs of particular bureaucrats. Moser posits that the key factor in Yeltsin's surprising reelection in 1996 was the consolidation of reformist and centrist voters behind a single candidate. His arguments on the importance of ideology and the presence of rational voting behavior should, however, have taken more into account some of the distortions of the 1996 presidential campaign. Notably, Yeltsin enjoyed full control of the primary state and commercial television channel, as well as virtually unlimited access to funds and political "pork" as both a carrot and a stick for voters. While it is worthwhile to assess and measure voter preferences, it could be argued that there needs to be some additional compensation for the particularly unfair elements in the Russian political system, namely, the domination of the media, public funding, and public office by some political parties. These distortions have become more important over time, particularly in the most recent Russian elections (not covered in this book).

Both Colton and Moser's books provide an important contribution to the understanding of voting behavior and party formation in Russia. More importantly, they use comparative theory to inform the discussion about Russia and, in turn, illuminate points about this theory in general. Russia is a fascinating laboratory for political scientists, as parties develop in a sort of "Big Bang" rather than at the evolutionary pace that they did in much of the West. As such, it is possible to observe and measure phenomena such as the development of partisan identification and the relative effect of party-list structure on party formation in an accelerated manner.

Both Colton and Moser are guardedly optimistic about the development of democratizing patterns in voting behavior and party organization along Westernized lines. There is worrying evidence, however, that any movement toward a Westernized, liberal party system in Russia has stopped. As both of these books are based on studies of elections in 1996 or earlier, it was impossible to include in-depth analysis of the 1999 and 2000 elections in the volumes. Preliminary studies indicate that while studies of voting behavior and

party organization are still relevant, the ability of powerful state bureaucrats to use ephemeral political parties and the media to manipulate electoral results has increased markedly. Realistically, future studies of elections in Russia must now take into account theories of propaganda and authoritarian “staging” of elections that are more reminiscent of the former Soviet Union than holding promise for a democratic Russia. However, the way in which Colton and Moser have integrated the study of Russia into the larger questions of the discipline remains an example for other scholars to follow.

East Asian Democratization: Impact of Globalization, Culture, and Economy. By Robert W. Compton, Jr. Westport, CT: Greenwood, 2000 224p. \$68.00.

Cal Clark, *Auburn University*

Studies of Asian politics tend to follow one of two divergent analytic strategies. Area specialists emphasize the importance of Asian culture in shaping the politics and economics of those nations, whereas comparative theorists apply broader models of political behavior to the Asian context. Robert Compton seeks to integrate these two traditions, which generally speak past one another, by proposing a promising (albeit somewhat limited) model of the dynamics of democratization in East and Southeast Asia. The model explicitly incorporates the distinctive cultures of these nations as the central causal factor explaining the process of democratization and the characteristics of the democratic polities that have emerged in the region. Furthermore, in terms of normative questions, Compton uses this analysis to critique both sides in the current debate between advocates of democracy and critics who claim that democracy is incompatible with “Asian values.” Thus, *East Asian Democratization* should be of interest to a wide variety of scholars concerned with Asian politics or with theories of democratization.

Compton’s theoretical analysis is developed from a central empirical finding that party systems in the democracies of East and Southeast Asia are systematically different from those in the industrial societies in Europe, North America, and Oceania. In particular, the analysis focuses upon “competitiveness” and is based on an index that has two distinct components: (1) the fractionalization of the system as measured by the partisan dispersion of seats in the national parliament and (2) the variety in the system as measured by the difference in the proportions of seats controlled by parties representing the “power elite” of business, the military, and the bureaucracy and by those representing other, nonelite sectors of society. Statistical analysis demonstrates that the party systems in Asian democracies are considerably less competitive than in other industrialized nations. Moreover, increasing levels of industrialization and urbanization do not bring greater party competitiveness in Asia, thereby controlling (at least crudely) for their status as late developers.

One easily discernible explanation for these findings is that the emphasis in Asian cultures on respect for authority and community solidarity, as opposed to the assumedly paramount Western values of individual freedom and autonomy, shape political parties that differ significantly from their Western counterparts. Compton’s theoretical model, however, goes far beyond such a simplistic static comparison. Instead (and almost certainly appropriately so for cultural analysis), the author takes a much longer historical perspective and views political development in these nations as an integral consequence of their economic development strategy. During the postwar era, first capitalist East Asia and then Southeast Asia cast their fates to the winds of the international marketplace. Their development strategies,

however, were far different from Anglo-American laissez-faire practice and reflected instead a “Confucian” or communitarian capitalism led by developmental states that were generally authoritarian. The stupendous success of exported Confucian capitalism ultimately challenged these political regimes by expanding the educated middle classes, who pushed for political reform. The governmental elites, in turn, eventually responded with a “top-down” form of democratization that differed significantly from Western democracy.

In the developmental state model, as adumbrated by Compton, Asian governments, especially the economic and financial bureaucracies, created their developmental states by melding modern and traditional components in a very creative manner. In the economic realm, they were quite modern as they created internationally competitive industries. They essentially turned to the traditional culture, however, for their political legitimacy, which was based on a combination of three sources in Asian cultural values: (1) respect for authority and for government officials, who are seen as a “guardian class”; (2) patron-client relationships that are part of paternalistic authority patterns; and (3) a greater concern for community and family than for the individual.

The political cultures in East and Southeast Asia were largely constructed by the elites in these developmental states—bureaucrats, the military, corporate leaders, and (where elections were permitted) electoral politicians. Compton, to sum up, conceptualizes the economic and political development of Asian democracies as going through a series of stages defined by the relative power of these four elite segments. He then applies the model to three case studies—Japan, South Korea, and Thailand. The first two, despite significant differences in the ability of their developmental states to gain popular legitimacy, fit the model quite well. The fit is not quite so good for Thailand, but the hypothesized developmental stages can be discerned there, too.

This analysis also has implications regarding the quality of Asian democracy. Elites success in leading economic development and the nature of the prevailing political culture combined to limit participation in the Asian democracies that did evolve, in particular by continuing the near exclusion of labor, students, women’s groups, and environmentalists from positions of influence. In addition, the emergence of strong parties was prevented by several factors, including the strength of government bureaucracies, the prevalence of patron-client ties, and the marginalization (even after democratization) of groups outside the traditional power elite. Consequently, Compton challenges both sides in the democracy versus Asian values debate. On the one hand, the assertion of conventional modernization theory that Western-inspired capitalism and democracy are spreading around the world is clearly invalid; rather, economic and political development in Asia constitutes modernization without westernization. On the other hand, the fusion of economic and political elites based on Asian values creates problems as well. Corporate leaders have used economic power to subvert democracy, and the pursuit of economic gain by politicians delegitimizes the state.

Overall, Compton’s model is a valuable heuristic tool that should make us think more deeply about important aspects of the political economies in East and Southeast Asia. It needs to be extended in several important regards, however. First, the book provides very little direct description and analysis of democratic politics in the region. For example, the scores of the individual countries on the two components of the party competitiveness index are not given; and the case studies of South Korea and Thailand overwhelmingly treat the predemocratic era. Second, as Compton’s treatment of Thailand suggests, the nature of the state and its relationship

to the economy vary widely in the region. Third, this also suggests that a much more nuanced use of Asian culture would be valuable. For example, Lucian Pye's *Asian Power and Politics* (1985) presents a typology of political cultures that appears well suited for differentiating the political and economic structures in the region.

Discordant Comrades: Identities and Loyalties on the South African Left. By Allison Drew. Aldershot, Hampshire: Ashgate, 2000. 282p. \$74.95

Courtney Jung, *New School University*

Drawing on a wealth of new information made available by the opening of the Comintern archives, Drew sheds the light of hindsight on the relationship between the Communist Party of South Africa (CPSA) and, in turn, the Soviet Comintern, the South African liberation movement, and the white and black trade union movements in the first half of the twentieth century. This rich book makes a unique contribution to our understanding of ties between the Comintern and its satellite parties as well as the early history of the South African antiapartheid movement. There are only two other major books on this period of party history, and both are memoirs of party members who try to establish a particular version of the record. Drew contests the teleology of their accounts of communist party history and instead weaves a contingent narrative that identifies major turning points that narrowed the possibility for a radical reorientation of the party (p. 281). It was not inevitable that the party would split and finally dissolve in the way it did—other outcomes were possible, almost until the end. That they were not taken was the layered result of personal and ideological rivalries and party alliances that made socialism, and socialists, perpetually weak and vulnerable in the context of South African politics.

Drew argues that much of the internal weakness of the CPSA derived from its relationship with the Comintern. South African communists were always vulnerable to external influence, in part because most communists were recent immigrants from Europe with connections to European parties, and in part because South Africa had no socialist tradition of its own. As a result, the Comintern was exceptionally influential in South Africa, particularly in the late 1920s, and the CPSA had difficulty maintaining a steady party line and leadership because the Comintern vacillated among positions almost year by year. As ideological stances fell out of favor, so did the leaders most closely associated with them. Internal rivalries and splits were raised to the level of doctrinal challenges as conflicting sides turned to Moscow to confirm, and legitimate, their position. In a local reflection of the international division, those who fell out of favor were called Trotskyists, regardless of their ideological views. The split between (Stalinist) communists and Trotskyists was mapped onto racial politics in South Africa, as each side aligned with different black opposition groups.

Although the Comintern played such an important role in guiding the party, its ideological directives had little to say about the main problems facing South African communists: how to organize across racial lines when local conditions (racism as well as legal constraints) precluded such organization, and whether African nationalist organizations were legitimate engines of social change. Before 1924, the CPSA focused most of its recruiting efforts on organizing white labor. As the most skilled and stable sector of the working class, whites were expected by some within the party to be the vanguard of an urban proletariat. White workers consistently saw their interests as separate from and even conflicting with

those of black workers, however, and refused to align across racial lines to build a unified working class. The CPSA finally turned its attention to black workers in 1924, demonstrating some ability to respond to local conditions and to the growing significance of black labor. In the 1930s and 1940s, party members such as Ray Simons played a crucial role in building a black trade union movement through the painstaking work of organizing on the factory floor. That movement is one of the lasting legacies of the early CPSA.

What stands out most is precisely how small and isolated the CPSA was, considering the important role it later played in South African opposition politics through its influence on the liberation movements. At its peak in 1929, the CPSA had only 3,000 members. By the time it was banned through the Suppression of Communism Act in 1950, it had fewer than 2,000. Yet, many of the positions and rhetorical flourishes made familiar through African National Congress (ANC) discourse turn out to be early socialist positions—from which the much more conservative ANC quickly distanced itself.

The relationship between the South African communists and other oppositional organizations, such as the All African Congress, the anti-Coloured Affairs Department, the ANC, and the Non-European Union Movement, was complicated by “the race question,” which confounded CPSA ideologues and strategists for most of the century. How to handle race in the effort to forge a class struggle was a central problem for South African communists and was a decisive, and indeed divisive, problem between the CPSA and the Comintern. In 1929 Sidney Bunting, an influential South African communist leader, established the League of African Rights. It was meant to be a broadly based organization that included strands of democratic and national liberation as well as communist ideology. The league was growing quickly, particularly in comparison to the tiny ANC, when the Comintern ordered it disbanded.

Drew speculates that if the League of African Rights had survived, it could have offered the basis of a working class or social democratic challenge to the ANC (p. 278) Instead, the CPSA pursued a policy of tactical alliance with the ANC through the 1940s, even as both groups were careful to maintain an independent identity. The ANC considered the CPSA too radical, and the CPSA feared being tainted by ANC nationalism. When the CPSA was banned in 1950, party leaders unilaterally elected to disband completely, to the surprise of many members. The party was reconstituted underground three years later, but it was almost completely subverted within the ANC. Drew ends her account of the South African Left in 1950, perhaps because, “in the era of apartheid and the Cold War, South African socialism subordinated itself to nationalism” (p. 280).

This narrowly constraining perspective unnecessarily pits nationalism and Left as opposing paradigms. In the case of South Africa, as in many others, nationalism was harnessed to the project of the Left after 1953. For example, the 1955 ANC Freedom Charter included such provisions as “the land belongs to all who work on it.” The true legacy of the CPSA was its influence on the ANC, from within, and later also on the United Democratic Front. In the final decade of apartheid, the state was challenged by mass-based people power anchored in the black trade union movement. This also was the Left, speaking simultaneously in the voice of race and class and confounding the oppositions that long structured South African intellectual and academic discourse—is it race or class conflict? It was both, and leftist opposition expanded to occupy all the spaces of domination and marginalization. Today, the CPSA is one of very few communist parties in power anywhere in the world, as part of the Tripartite Alliance with the ANC and Congress of South African Trade

Unions. Yet, the alliance with the trade unions and the liberation movement has hardly strengthened the hand of the Left. In the postapartheid era, it has finally subordinated itself, not to nationalism but to governance and neoliberalism.

Women, Religion, and Social Change in Brazil's Popular Church.

By Carol Ann Drogus. Notre Dame, IN: University of Notre Dame Press, 1997. 226p. \$26.00.

Mala Htun, *New School University*

Historically, the Roman Catholic Church is seen as an obstacle to progressive social and political change in Latin America. Beginning in the 1960s, however, the Second Vatican Council and the growth of liberation theology prompted doctrinal and institutional changes in the church in Brazil and several other countries. From an ally of the conservative oligarchy and establishment, the church turned into an engine of mobilization for grassroots movements and a focal point for popular opposition to authoritarian governments. One of the more significant and widely researched changes in the “popular church” was the establishment of thousands of ecclesiastical base communities (CEBs) among the poor. The fact that the majority of CEB participants are women has received far less attention.

In this well-researched and engaging book, Carol Ann Drogus analyzes women’s participation in the CEBs of Brazil’s popular church with the intention of addressing two theoretical concerns. The first is the extent to which gender acts as a filter for religious experience and interpretation of liberation theology. Can the failure of CEBs to make a more radical contribution to Brazilian politics be attributable to the fact that they are comprised largely of women? The second question focuses on how women’s participation in the popular church has affected their attitudes toward gender. Have CEBs empowered women, and if so, has this led to development of a gender consciousness among them? Drogus’s findings are based on extensive fieldwork that consists of interviews and participant observation in seven CEBs in Santo Antônio, a low-income parish on the outskirts of São Paulo. The book contains rich descriptions of the daily lives of the women and their families (interviews with a core sample of 30 women who are CEB participants provide most of the data) and of the development of the popular church in Brazil.

Drogus found that, although the women face similar challenges in daily life and share a history of “long, intimate identification with the Church” (p. 78), they interpret the messages and symbols of liberation theology quite differently. Identifying at least three distinct religious personality types (liberationists, traditionalists, and Samaritans), Drogus argues that women as a group have neither “energized nor stymied” the liberationist project (pp. 81–107). Their political attitudes also vary. Liberationists are prone to link their personal struggles for survival with the status of the working class in general, and traditionalists are more likely to accept being poor as part of the natural order of things and to perceive strategies for upward mobility in individualistic terms. Despite their differential acceptance of liberation theology and class consciousness, however, the women share a positive response to the social movements endorsed by the church. They mobilize to participate in day care, sanitation, and land movements, which they see as mothers’ movements that enable them to work on behalf of children—“their own and all poor children more generally” (p. 142).

To what extent do the CEBs empower women and promote gender consciousness? To be sure, CEBs are not hotbeds of feminist activism. The popular church conforms to traditional Catholic doctrine in condemning civil divorce, abortion, and

birth control. Virtually every woman in the study endorses these traditional views.

Liberation theology, moreover, tends to subordinate gender to class oppression (p. 157). Yet, CEBs encourage women’s active participation in the public sphere and offer opportunities for women to assume new religious and political roles. In some cases, the fact that their domestic gender roles and family lives almost never change in parallel to their changing public role plants a seed of gender consciousness. Frustrated with the lack of change in men’s attitudes and behavior, some of the liberationist women in the study want more help around the house and also argue “for real *equality* and a division of labor that allows women to assume what they see as their *right* to participate” (p. 172).

The strength of the study lies in the compelling narratives about religion, politics, and daily lives. The fact that Drogus elicited this level of candor from her subjects is a testament to her skills. Yet, the research design permits the author to address only the second of the two questions posed at the beginning of the book. The data allow Drogus to advance claims about women’s reception of liberation theology but not the analytical significance of gender. An understanding of how gender filters religious messages requires a control group—in this case, men.

I also have some concerns about generalizability. The author claims to have chosen Santo Antônio as a “critical case” region, the idea being that if “liberation theology does not flourish in the conditions its proponents have deemed most hospitable, it is unlikely to do so elsewhere” (p. 20). The area had a “famously liberationist bishop” and “pastoral workers with a clear liberationist agenda” (p. 21), which created a particularly “propitious context” for CEB development. The parish participated in some of the major civil uprisings of the late 1970s that weakened Brazil’s military government. Because CEBs had only a limited effect on these women, I have the impression that in the rest of Brazil the effects of CEBs are small to nonexistent.

It may be significant that only liberationist women demonstrated a class consciousness and a desire to change gender roles. Given their underlying activist predispositions, one wonders whether they would have found modes of public participation—in neighborhood movements, for example—in the absence of CEBs. After all, women’s changing attitudes toward gender “have been largely an inadvertent byproduct of their participation rather than something with which the CEBs should be directly credited” (p. 179). This implies that the effects of CEBs on the attitudes of even liberationist women may have been largely epiphenomenal, a possibility that should be considered more seriously in the book. Incidentally, Drogus is too quick to endorse the proposition that gender characteristics account for women’s rejection of party politics (pp. 138, 184). When political parties fail to offer equal opportunities to women to participate as militants, to rise in the party ranks, and to stand for office, it is quite rational for women to avoid them.

The book should be of interest to scholars of religion and politics, Latin American politics, and gender and politics.

Brokers and Bureaucrats: Building Market Institutions in Russia.

By Timothy Frye. Ann Arbor: University of Michigan Press, 2000. 272p. \$65.00 cloth, \$24.95 paper.

Stefan Hedlund, *University of Uppsala*

Over the past decade, Russia’s attempted transition to a market economy has attracted a great deal of scholarly interest. Given the complexities at hand and the rather disappointing

outcome, it is natural that debates have been acrimonious at times. Some have argued that we are witnesses to a great success; others, that we are seeing a monumental failure. A book that promises to deal with “building market institutions in Russia” might harbor yet another contribution to such debates, but that is not the case. Timothy Frye is careful to note (on p. 12) that his book “does not attempt a holistic analysis of the process of economic and political reform in Russia.”

Leaving the overall assessment of success or failure for others, he chooses to tell a more specific story, and to use that story to advance our understanding of an important field in social science theory, namely, that of institutions and the problem of social order. The factual story, which has great merit in itself, covers the rise of three types of markets in post-Soviet Russia: currency futures, commodities, and corporate equities. The accounts are based on a combination of published materials and personal face-to-face interviews with brokers.

The aim of the five case studies presented is to explain differing degrees of success among brokers seeking to establish functioning systems of self-governance. It is also here that we may find the author’s claim to originality: The absence of state agents from existing theories of self-governance constitutes a hole in the literature.

In a broader sense, Frye’s undertaking represents a continuation of a large body of literature. Those who are not familiar with the field are given a very quick tour, ranging from Hobbes onward to Ostrom, Putnam, North, and new institutional economics.

The main thrust of his argument, however, is not to refine but to challenge the predictive value of previous economic and sociological theories of self-governance, arguing that his “political” approach is better suited to explain the cases at hand. Based on factors such as taxation and delegation, his discussion of the interaction between brokers and bureaucrats sheds valuable new light on the processes at hand. He deserves credit for highlighting the roles that may be played by state agents in promoting or hindering the emergence of institutions to support self-governance. But to what extent does he succeed in advancing the main body of theory? My problem here is chiefly one of presentation, of claims to have climbed mountains that really turn out to be hilltops.

From an institutional point of view, the role of the state as an outside enforcer is to promote such norms that we may achieve sustainable progress. If self-governance emerges in association with methods that go clearly against that longer-term goal of the state, then we have a mixed outcome indeed. By ignoring this dimension, Frye in some sense flushes the baby out with the bath water.

In a narrow sense, he presents an interesting analysis of factors that decide whether actors will rely on self-governance or turn to private protection, but he fails to note that in the broader picture even a formally successful case of self-governance will depend for its sustainability on the functioning of the surrounding state. More specifically, we must question the validity of Frye’s critique against those who have argued that the roots of social capital are “lost in the mist of history” (p. 107). While he is correct in saying (on p. 154) that “culture is too broad a variable,” he clearly goes too far with claims that his technical explanations of organizational competition offer a “more general explanation for the rise of the racket in Russia.” If it is true that culture cannot explain specifics, then it is equally true that exactly the same set of formal rules will have very different outcomes in different cultural contexts. From a policy point of view, these are important distinctions.

More generally, the political approach based on state agents can be held up as “better” than, say, the criticized economic approach only in a very narrow sense. By failing to incorporate the dynamic aspects of institutional change, as laid out by North and others, it has little to say about the broader issue of how the newborn social order will evolve. Thus it also has limited policy relevance.

In this sense Frye also greatly overstates when arguing (on p. 155) that his technical explanations are better suited to explain the weakness of the Russian state than arguments relating to the neoliberal ideology of the reforms. He is right only if we ignore completely the formation of social norms to support the formal rules of the reforms.

Turning to another problem, much emphasis is placed on the corporate equities market, as a prime example of successful self-governance. Again, the interplay between brokers and bureaucrats is given an interesting and certainly valid interpretation. But perhaps it might be better in this case not to speak of “state agents” at all, but of the interplay between foreign operators with clear money-making strategies and young Russian reformers eager to impress their foreign benefactors. The mechanics of formal rule setting would remain the same, but we would also capture all those problems of moral hazard that, *inter alia*, have led the U.S. government to file a law suit against Harvard University for activities undertaken by Harvard-employed advisors to the Russian government. What “state” did these “state agents” represent?

Throughout, the text also has a certain gloss of success. Chapter 8, in particular, which details the process leading up to the crash in 1998, studiously avoids any mention of bad behavior by Russian actors. I have no problem with this, but it does go against the declared ambition of staying clear of any broader assessment of success or failure.

My real problem with this book is that it ends up a halfway house of sorts. By neglecting to deal with some of the more unsavory aspects of Russia’s emerging markets, such as the protection of minority shareholder right, it fails really to come to grips with the broader story, which is a pity. At the same time, by neglecting to deal with the full scope of theory, such as the formation of norms and the consequent risk of institutional traps, it also fails to derive real benefit from the Russian example, which, perhaps, is even more of a pity.

Audacious Reforms: Institutional Invention and Democracy in Latin America. By Merilee S. Grindle. Baltimore, MD: Johns Hopkins University Press 2000. 269p. \$45.00 cloth, \$17.95 paper.

Brian F. Crisp, *University of Arizona*

Merilee Grindle addresses three questions: Why would rational politicians choose to give up power? What accounts for the selection of some institutions rather than others? What are the political consequences of the creation of new institutions? She studies cases of decentralizing political reforms in Venezuela, Bolivia, and Argentina. Her case studies are loosely guided by eleven hypotheses deduced from three schools of thought. The schools to which she refers in an introductory and concluding chapter are rational choice, comparative institutionalism, and new institutionalism (the latter has two subvariants: transaction costs and institutional design). The theoretical perspectives are apparently not equally useful across questions, as new institutionalism is not used to deduce hypotheses on why politicians would choose to reform, and rational choice is not applied to the reasons some institutions are chosen over others.

Devoting two chapters to each national case, Grindle marshals a variety of materials about how politicians and groups

in civil society justified their positions regarding the need for and content of political reform. In Venezuela the decision was made to elect directly state governors (previously presidential appointees) and mayors (an office that had not existed previously). In Bolivia the Popular Participation Law made it more difficult for national officials to rely on patronage, and it formally recognized the citizenship and participation claims of the large ethnic population. In Argentina the government of Buenos Aires, where 10% of the population lives, was granted greater political autonomy, including increased control over its own financial resources and the direct election of its executive (previously a presidential appointee). To assess the consequences of reform, the author draws on the opinions of those involved and traces trends in partisan identification, voter turnout, and parties' electoral support. In addition to a brief summing up at the end of each case-oriented chapter, she provides a concluding chapter that explicitly returns to the hypotheses deduced at the beginning of the book.

Given the complexity of the issues, the research design employed, and the unwieldy nature of several key concepts, it is inherently difficult to make compelling statements about whether support was found for a particular theoretical approach. Regarding the motivation for reform, Grindle interprets politicians' willingness to decentralize in an effort to recoup legitimacy as support for a rational choice approach. The lack of mobilized groups who champion the cause of decentralization undercuts the usefulness of comparative institutionalism as an approach. A related lack of overt conflict among organized factions regarding the content of reform signals a lack of support for the comparative institutional approach in relationship to Grindle's second question. Instead, the relatively consensual design of reforms by specialists who are motivated by explicit critiques of existing institutions is taken as support for the transaction costs and principal-agent variants of new institutionalism. Grindle concludes that politicians and parties did alter their behavior in predictable ways after the reforms, which supports both rational choice and comparative institutional approaches. The case studies yield mixed results regarding support for both variants of new institutionalism because transaction costs and principal-agents issues vary across actors—some benefited from reforms, and others found politics more difficult.

Readers interested in decentralization and its contribution to the further democratization of already democratic regimes will find empirical details of great interest here. The case studies provide insights into the motivations of several types of actors. Although never formally conceptualized, the arenas through which they come into contact (the previously existing institutional designs) are covered in some detail. Grindle points out that history, including previous institutions, helps explain the problems that provoke reform and the diagnosis of those problems but not the content or timing of reform. One wonders whether a pattern would emerge with the coverage of more cases, more explicit conceptualization of the (historical) institutional features hypothesized to influence the content of reform, or consideration of additional types of democratizing reform (e.g., electoral reform at the national level). Grindle's point about the inability to explain the timing of reform seems particularly troublesome for comparative (historical) institutional approaches. Recent literature on path dependency and critical junctures is not yet reviewed in *Audacious Reforms*, but the author's conclusion that we cannot systematically explain the timing of institutional change defines a challenge for institutionalists of all stripes.

Another challenge to scholars emerges through Grindle's accentuation of "leadership" as key to explaining many outcomes. For example, politicians decided to decentralize de-

spite the lack of focused pressure from below to do so, and specialists played a key role in determining the content of reforms. How does "leadership" fit into the theoretical perspectives tested in this book? Grindle notes that comparative institutionalism might provide the most promise in this regard, leading one to wonder whether there is a relationship between leadership as used here and preference formation as discussed in much of the literature on institutions. Attention to preference formation is supposed to be a relatively strong suit of historical approaches, and it may be that the stress on leadership is nothing more than recognition of the importance of preference formation in a different guise. Yet, the seeming importance of creative leaders may simply be an artifact of the research design. In the book, the dependent variable, the adoption of decentralizing reforms, is held constant. If it were allowed to vary, we might find equally creative leaders in other countries who did not choose to pursue decentralizing reforms or who preferred to do so but failed.

In sum, *Audacious Reforms* takes on an important topic and assembles a rich array of case materials. Those with an interest in decentralization or politics in Venezuela, Bolivia, and Argentina will undoubtedly find this a rewarding book. Both explicitly and implicitly, the book is very thought provoking on larger theoretical concerns that will be of interest to a wider readership.

Democracy and the Media: A Comparative Perspective.

Edited by Richard Gunther and Anthony Mughan. Cambridge: Cambridge University Press, 2000. 496p. \$85.00 cloth, \$29.95 paper.

Media and the Presidentialization of Parliamentary Elections. By Anthony Mughan. New York: Palgrave, 2000. 179p. \$65.00.

Margaret Scammell, *London School of Economics*

The themes of crisis and transformation have fueled a mini-explosion of research on media and democracy in the last decade. Researchers within or close to the "media studies" school have developed a burgeoning literature on questions of citizenship and the public sphere, in the context of deregulation, expanding media markets, and rising interest in the arguments of the deliberative democrats. Scholars more closely connected to political science have pursued an overlapping but different agenda. From the United States and western Europe, amid concern at signs of a crisis of citizen engagement, the focus increasingly is on media power to mobilize or demobilize voters. From Eastern and central Europe and Latin America there is an emerging corpus on the role of media in the transition and consolidation of democracy. Cross-cutting these various strands are the Internet revolution and the question of globalization and, more specifically, U.S. potency to lead or at least predict trends in political communication for the democratic world.

Mughan and Gunther's edited collection is a substantial contribution to these more identifiably political science themes. With the one notable, if understandable, absence of the Internet, the book brings together admirably the concerns of established and establishing democracies. A strong cast of scholars analyzes the contemporary contribution of media to democracy in 10 countries: the United States (Thomas Patterson), Japan (Ellis Krauss), the United Kingdom (Hollie Semetko), Germany (Max Kaase), The Netherlands (Cees van der Eijk), Spain (Gunther et al.), Italy (Carlo Marletti and Franca Roncarolo), Russia (Ellen Mickiewicz), Hungary (Miklós Sükösd), and Chile (Eugenio Tironi and Guillermo Sunkel). The various individual parts are knitted together

by Mughan and Gunther's introductory and concluding chapters. They establish the organizing principle, which is to combine micro-level analysis of media impact in specific circumstances and elections with a macro focus on media systems and their inferences for the distribution of power.

Comparative volumes usually work best when a clear overarching hypothesis is tested against the experience of the individual cases (for example, David L. Swanson and Paolo Mancini, *Politics, Media and Modern Democracy*, 1996). This book is not so tightly connected. Its research question is more open-ended and descriptive. It says, in effect, let us bring together macro and micro evidence in key countries and then assess it for patterns in media environments and the democratic character of political regimes. This approach pays some dividends. The chapters on the newer democracies, Russia, Hungary, Chile, and Spain, are especially fascinating reading. This is less because they tell us things we did not know—research on these countries is fairly readily available elsewhere—but because they are brought together, side by side, and amount to a riveting account of the importance of media in high politics. Clear cross-country patterns do emerge in totalitarian and authoritarian control over press and television and, equally, the crucial role of media in every case to the progress of democratization. Anyone who doubts the political importance of the media would do well to read these chapters and, indeed, the ones on Italy and Berluconi's adventures and van der Eijk's provocative account of the media's leading role in the depillarization of Dutch society.

The appeal of this book rests precisely in the quality of some of its individual chapters. Equally, the chapters that work best are those rooted most firmly in politics, especially the politics of transformation. This is not quite the same thing as saying that the political impact of the media is most interesting in those countries with the most intriguing politics, although that is a tempting conclusion. Rather, it is to say that the macro-level analysis of politics and media is the most valuable contribution of this work. The idea of micro-level analysis was a useful one in theory. It is instructive to know the state of media effects research in the various countries and the consensus verdicts about their impact on public opinion and elections at key times. However, this was generally the most variable and least satisfactory contribution of the country-specific chapters. Methodological approaches to the micro either were simply not comparable from country to country, or were not well elaborated, or were indeed detailed but too time-specific to add greatly to general arguments. Despite this, the editors' conclusion makes strong, too strong, claims for the direct impact of media in individual elections and, more broadly, for the "socializing" effects of media in central European countries. To take one example, it is simply not proven, or capable of proof from the evidence as presented here, that continued support for socialism in central Europe was the direct consequence of "four decades of government censorship and propaganda." Direct media effects are notoriously difficult to prove beyond doubt, and the editors simply did not need to overegg the evidence of their authors: There is abundant material here anyway for the political impact of the media.

One would not wish to quibble too much with editors, given the sheer amount of the work in this book and the general value of its contribution. Yet one cannot help being struck by the irony of some of their U.S.-centric assumptions, given the internationally collaborative nature of the work. One such example is their major conclusion, which is to challenge the "conventional wisdom" that the freer the media are from government, the stronger the contribution to democracy. This may be the conventional wisdom in the United States, al-

though they offer few references in support of it. However, it is simply not the conventional wisdom in Europe, where for more than 10 years media researchers have sought to protect key elements of public service systems precisely in the interests of a democratic public sphere. The free market/democratic media equation just does not hold as strongly in Europe. In fact, the editors unwittingly admit as much with their comment that European Union commitment to public service systems suggests that it is unlikely that many democracies will copy the U.S. system. A second example is their introductory comment contrasting the current wave of pessimism about media performance with earlier decades of apparent contentment. Again, this simply ignores the European critical tradition, the Frankfurt School, and the critique of sociology-grounded media studies. With these caveats, the editors deserve congratulations for a volume that is a sure-fire certainty on all political communication reading lists.

Mughan's solo book, *The Presidentialization of Parliamentary Elections*, is, in contrast, far less grand in its ambition. It is a careful and rigorous study of the theme for which he is most familiar in Europe, the trend toward "presidentialization" of parliamentary politics. He confines the study to Britain, which, he argues reasonably, is a fair test-bed. If the proposition is that presidentialism is a broad phenomenon of democratic politics, it should certainly be apparent in Britain, with its strong majoritarian electoral system. A key contribution of this work is Mughan's attempt to define the phenomenon of "presidentialism" and "presidentialization" and, more importantly, to deconstruct it into testable elements. Presidentialization is defined as a movement over time away from a system of collective to one of personalized government, within the parameters of an unchanging constitution. He tests two aspects of this. First, presentation: Has there been a move from predominantly party- to predominantly leader-focused presentation? Second, are leader effects becoming more influential on voting decisions? This is a helpful approach to a subject that is as ubiquitous as it is vague in the political communications literature. Personalization, we are told endlessly, is a defining feature of modern democratic politics. Rarely, though, are the core concepts defined and, even more rarely, carefully tested against empirical evidence. So in this sense Mughan's book is a welcome addition.

Beyond that, though, it is slightly disappointing. Mughan's test of personalization of presentation comprises primarily content analysis of *Times* editorials in election campaigns since the 1950s and Gallup opinion polls conducted for newspapers. There are two main difficulties with these data: one admitted, one not. The first is that since the Gallup surveys do not include a measure of party identification, there is no way of disentangling party and leader preferences. The second is the use of *Times* leaders as a proxy for party campaigns. It is neither uncommon nor unreasonable to suggest that campaigns have become more leader-centered, but it is not verifiable or otherwise from these data. One is left wondering quite what Mughan's content analysis adds to the existing body of research, which does indeed demonstrate increasing media attention over time to leaders. He concludes that there is a trend toward presidentialization consistent with the "threshold" model, that is, a substantial increase in personalization at threshold elections, followed by fluctuations, which never return to the prethreshold lows. However, beneath the claim for the trend, Mughan's conclusions are modest and predictable from preexisting studies: Leadership is still far less important than party and probably sufficient to swing results only in exceptionally tight contests. This is solid stuff from Mughan, but aside from his opening discussion of the core concepts, it is more safe than exciting.

Constructing Sustainable Development. By Neil E. Harrison. Albany: State University of New York Press, 2000. 175p. \$54.50 cloth, \$17.95 paper.

Rodger A. Payne, *University of Louisville*

Nearly 15 years has elapsed since the World Commission on Environment and Development—the so-called Brundtland Commission—popularized the idea of “sustainable development.” The phrase turned out to be unusually slippery, providing both political cover and ammunition for almost anyone engaged in debates about the global environment and/or development. Indeed, scholars and policymakers of all theoretical or ideological stripes found creative ways to employ the phrase “sustainable development” to support a wide array of arguments in these discussions.

Neil Harrison has written a clear and concise book that addresses important questions related to the ambiguous and multiple meanings. He analyzes, in the postmodern tradition, three dominant, yet often conflicting, policy narratives of sustainable development. These are efficiency, equity, and ethics. For each narrative, Harrison explains and evaluates the premises and arguments borrowed from various social, economic, or political theories. Then, over the course of five short chapters, he highlights logical inconsistencies that make viable policy goals literally impossible to achieve. The author reveals the conceit often hidden in these narratives and, in turn, promotes greater humility. His arguments are sharp, but his purpose is not mere deconstruction. In fact, Harrison often notes the elements of a narrative that might be made to work and that should be preserved in some fashion.

Still, Harrison concludes quite forcefully that “sustainable development is a Holy Grail that does not exist. It is a legend, a myth. . . . [The term] cannot be objectively defined, cannot be known” (p. 99). So what would people have to do to make development sustainable, which is the goal the author establishes on the book’s first page (p. vii)? The answer is certainly not found in traditional literatures on economics, politics, or environmental ethics. The chapters on efficiency, for example, describe and critique biases in the neoclassical vision of the market and then dissect alleged technological fixes, which are not likely to be developed in the current political-economic context. The chapters on equity likewise explain why neither redistribution of wealth nor international cooperation is about to occur. Finally, Harrison demonstrates that no society can embrace ecologically ethical policies without first embracing environmental ideals. This presents a bootstrapping dilemma, since ideas cannot be changed absent education policies that promote ecology.

Harrison offers his own recommendations in the final chapter. He borrows from the postmodern tradition to find meta-narratives that might be able to transcend barriers across the political, economic, and social divides he has identified (p. 101). From this perspective, the author derives a need for a far more participatory politics and an ecological view of science that values flexibility and adaptability. Data need to be accumulated, and education promoted, he asserts, so as to match the most appropriate policy initiatives to local needs. Perhaps most controversially, Harrison wants sustainable development to be “the central concern of political discourse” (p. 118).

If most of these prescriptions seem somewhat vague and (at least to the informed reader) mundane, Harrison is unapologetic. On the last page, he returns to an earlier admonition (Chapter 2) that policies supporting sustainable development (or virtually any policy goal) “are always stabs in the dark, best guesses in an uncertain world” (p. 118). Harrison’s broad challenge to “rational” economics, science, and policy-making, however, might tempt readers to question whether

his preferred choices are better than those he critiques. After all, the author warns that sustainable development seems to be “the ultimate ‘postmodern’ issue” and “can be interpreted to support any agenda, or objective” (p. 102).

Consider Harrison’s plea for education. While the author means his claims to be taken differently, it is difficult to imagine that college administrators will be persuaded to build their general education or liberal arts curriculums around the idea of sustainable development precisely “because it can mean everything to everybody” (p. 118). Harrison stresses that education should be sensitive to ambiguity and that teachers should take diverse perspectives into account when considering something as elusive as sustainable development. In practice, however, his warnings literally seem to imply that nothing is valid and that everything is valid.

Why should an idea like the precautionary principle (pp. 16, 111), for example, presumptively favor environmental goals? A probusiness advocate might argue that caution demands favoring jobs and economic well-being over “risky” policies to defend the environment. During the Cold War, “worst-case planning” meant spending hundreds of billions of dollars on nuclear weapons to promote “security.” No one should assume that environmental goals would come out on top if the most basic societal goals started to be compared. Harrison’s argument for community and participatory politics could even subvert environmental objectives. In actual debates about forest policies, loggers of the U.S. Pacific Northwest apparently prefer to retain their jobs to the preservation of owl habitats.

Anyone who has perused the right’s antienvironmental literature (e.g., see Ronald Bailey, *Eco-Scam*, 1993), quickly learns how postmodern insights and arguments can be turned against environmentalists. Scientists have often been wrong about past warnings of ecological or resource collapse, the skeptics assert, so why should anyone make costly policies based upon their latest warnings about global warming? The problem is magnified when a scientist or two challenges the environmental views.

Postmodernists, ironically, might fault Harrison for failing to embrace their project more fully. He acknowledges that his “approach is not specifically postmodern” (p. 112) and at times he seems to favor the employment of both material and instrumental measures. For instance, he advocates substantial increases in aid to poor countries, which would essentially bribe them to support sustainable development. He also supports higher taxes on consumption to influence consumer demand for resources. Yet material levers distort dialogue and would not necessarily promote an ecological mindset.

Harrison’s book seems most useful for educators who teach undergraduate or master’s-level courses about the environment. Students would benefit from the author’s succinct and lucid critique of prevailing economic, political, and ethical theories and from his application of postmodern theorizing. No doubt, Harrison’s arguments would provoke interesting and useful classroom exchanges.

Eroding Military Influence in Brazil: Politicians Against Soldiers. By Wendy Hunter. Chapel Hill: University of North Carolina Press, 1997. 243p. \$45.00 cloth, \$19.95 paper.

Craig Arceneaux, *California Polytechnic State University*

Writing at a time when the literature on transitions to democracy was fixated on the mode of transition, and when concepts such as authoritarian legacy, authoritarian enclave, or the shadow of the past dominated, Wendy Hunter’s 1997 book,

Eroding Military Influence in Brazil, went against the grain. In a direct attack on institutionalist approaches that emphasized the resiliency of military prerogatives in transitions from military regimes, Hunter argues that democracy itself would, over time, reduce the influence exerted by the armed forces. A solid theoretical orientation, rich casework, and insightful commentary on the dynamics behind the creation of civilian control all mark the book as a noteworthy contribution to the literature on democratic transitions, civil-military relations, and Latin American politics.

Guided by a rational choice perspective, Hunter argues that elected politicians answer first and foremost to their own immediate self-interests. Their aspirations directly impinge on the armed forces insofar as the retention of military prerogatives hinders the capacity of politicians to cater to the demands of the electorate and secure patronage to aid their future careers. Brazil is viewed as an exceptional case study because of the control exerted by the military during the democratic transition. Insofar as the military used its control to instill its advantages into political institutions, Hunter has a case that allows a test of her theory against that of historical institutional approaches, which hold a higher expectation of institutional resiliency. Hunter views institutions as the product of existing political desires and conflicts, while the institutionalist approach argues that institutions are not so malleable and, in fact, produce expectations, desires, and conflict independent of the interests and interactions of current actors.

The subtitle of the book, *Politicians Against Soldiers*, in fact obscures a more complex approach which recognizes the different pressures faced by executives, on one hand, and legislators, on the other. Both presidents and legislators can be threatened by military bullying, but legislators face a collective action problem that prevents them from feeding military desires to offset the threat—insofar as a response to the military demands a reduction in patronage resources, one legislator cannot be certain that his colleagues will follow. Hence, during times of crisis, only presidents answer to military intimidation. The second difference is the types of goals pursued by executives and legislators. Particularistic goals refer to the patronage distributed by officials to garner support from specific groups, while programmatic goals are those that cater more to the interests and principles of society as a whole. Both goals are important to politicians in a general sense, but smaller, regional constituencies lead legislators to be more concerned with particularistic goals, while presidents are more attuned to programmatic goals.

To illustrate the expression of legislative particularistic aspirations, Hunter devotes a chapter to budgetary politics and argues that the draw to patronage leads policymakers to withdraw funds previously devoted to military spending, although military pressure on the executive can offset some of this decrease. In another chapter, she uses advances in labor policy to illustrate the pursuit of programmatic goals by both legislators and presidents. The third case is devoted to Amazon policy. The immediate connection to democracy is not very clear in this third case. Changes in land-use policies and concessions to indigenous groups did infringe on military security interests, but pressure to do so came not from the Brazilian public but, rather, from international organizations, NGOs, and foreign governments.

While the discussion of eroding military influence distinguishes the book in a literature which largely thought otherwise, Hunter is not naively optimistic. In the conclusion, she recognizes that civilian control involves not only the reduction of military power, but also the establishment of mechanisms of civilian control such as greater civilian input in defense issues, education and training, the military justice

system, and other areas that do not conflict with politicians' quest for patronage resources and, in fact, often entail greater costs than benefits. Civilian supremacy demands not only the destruction of military influence, but also the construction of civilian oversight, and democracy does more to contribute to the former. Indeed, insofar as the short-sighted interests of politicians prevail, democracy may actually hinder civilian supremacy. This allows her theory to be more dynamic and to account not only for eroding military influence, but also reversals in democracy. Some comparisons to other South American cases are used to illustrate the applicability of the theory.

While the focus on democracy itself is interesting, and an important contribution, one cannot help but recognize that other, perhaps more important, factors are at work in the Brazilian case and elsewhere. In particular, Hunter seems too quick to dismiss the role of institutions in her effort to distinguish her rational choice approach from historical institutionalism. The Brazilian case was a controlled transition, but military prerogatives were not as institutionalized as in, for example, the Chilean case. Hunter herself notes that the armed forces very often expressed their interests through "entendimentos" (understandings) rather than codifying them into institutions, and this makes the case less of a "most likely" scenario for historical institutionalism. Indeed, institutions are largely responsible for the democratic dynamic in Brazil: Intense electoral competition is closely tied to Brazilian electoral rules (e.g., open lists, liberal state funding, ease of party creation); the lack of legislative influence over the placement of military bases and the insularity of defense production from civilian producers both remove areas that might typically tie military interests to legislative electoral interests; the 1988 constitutional changes gave the legislature more control over the budget; and the ruling that congress must work within the budgetary level set by the president constructs a zero-sum game between legislator and military budgetary interests. One need look no farther than the United States for evidence that congressional and military interests can coexist. Democracy comes in many forms, and its institutional configuration does more to set the battlefield for civil-military interaction than do individual interests. The indeterminacy of individual interests can be teased out of Hunter's theoretical discussion when she notes that presidents have the capacity to exert civilian control but lack the incentive (due to the greater fear of a military reprisal), and legislators have the incentive but lack the capacity (due to the collective action problem). How do we get from here to there? The answer rests at a higher level, within institutions, which rational choice approaches too easily dismiss as simple reflections of individual interest. Indeed, if institutions were so malleable and so directly reflective of rational individual interests, we would expect Brazilian politicians, who hold traditionally poor records of reelection, to concentrate first and foremost on creating an electoral regime to better secure their futures.

The second factor that would contribute to a better understanding of civilian empowerment after transition would be the military itself. The Latin American military has changed dramatically since the transitions to democracy—the post-Cold War world calls for new roles and missions, foreign actors scrutinize their actions as never before, and time itself under democracy has allowed the military to alleviate some of its traditional mistrust of politicians. Changes in military perceptions, beliefs, and attitudes have led the armed forces to be more open to civilian calls for reform in some areas. Tips in the civil-military balance are thus the result of not only civilian pushes, but also rethinking and acceptance on the part of the military—in some policy areas more than others, and in some countries more than others.

Eroding Military Influence forces scholars of democratic transition and civil-military relations to consider seriously democracy itself as an influential factor. Further research would do well to heed this advice, as well as to recognize the roles that institutions and the military itself play.

Policy, Office or Votes? How Political Parties in Western

Europe Make Hard Decisions. Edited by Wolfgang C. Müller and Kaare Strøm. Cambridge: Cambridge University Press, 1999. 319p. \$59.95 cloth, \$21.95 paper.

Michael Laver, *Trinity College Dublin*

This book is about the motivations of political actors. Many of the most commonly used models of party competition and government formation are grounded in explicit assumptions about the motivations of party strategists. These tend to assume one of three basic and interrelated motivations—the desire to fulfill policy objectives, the desire to control the perquisites of office, and the desire to maximize votes. While recognizing that living and breathing people may be driven by any or all of these motivations, among others, and that these may interact with each other in complex ways, most theorists ground their models in assumptions of policy-seeking OR office-seeking OR vote-maximizing by key political decision makers. Indeed this distinction between motivational assumptions is one of the most common ways to classify models of party competition. In part the grounding of models in a single motivational assumption is for the sake of analytical tractability; in part it is because the heuristic insights made possible by these approaches are enhanced if the models are kept simple and their relationship to core assumptions is kept straightforward.

The publication of Müller and Strøm's edited volume is timely because there has been a growing feeling in the literature that the time has come to move on. It has always been the case that, when analysts have applied formal models to real-world examples, they have explained anomalies by acknowledging motivations that go beyond those in their models. Thus the refusal of Communist or Green parties to converge on the center ground may be excused in vote-maximizing models of party competition on the ground that they are policy seekers. The willingness of ideological strange bedfellows to get into coalitions with each other may be explained in policy-driven models of government formation by the assertion that these particular parties care much more about getting into office than about enacting their policy program. Increasingly, these observations are leading theorists to try constructing models in which politicians have multiple motivations, which they must typically trade off against each other when making hard choices.

It is one thing to write down a model containing such trade-offs. It is quite another to apply it to the real world, an exercise that requires the analyst to provide at least a ballpark estimate about how a particular politician might trade off such-and-such a probability to getting into office against such-and-such a dilution of fundamental policy objectives. If my model tells me that you can increase your probability of being in government by 40% if you drop your long-standing policy objective of nationalizing the commanding heights of the economy, how do I know a priori whether or not you will do it? I need some realistic information about how you trade off office and policy motivations.

Müller and Strøm have provided us with the first book-length empirical study of precisely this problem. Essentially it is an edited collection of thickly descriptive case studies, topped and tailed by general chapters by the editors in

reviews that are sensitive to a range of the theoretical implications of this material. They set the scene in an opening chapter that does not contain a model or even a typology of trade-offs and hard choices, but that sets out to clear away some of the conceptual underbrush entangling these—a task that necessarily precedes the construction of such a model. We then have 10 case studies written carefully by well-established experts in their fields. Each of these takes a particular set of hard-choice trade-offs in a particular country, providing a detailed post hoc interpretation of this. The countries covered, in chapter order, are Ireland, Denmark, Spain, The Netherlands, Italy, Austria, Norway, Germany, Sweden, and France. This book was a long time in the making and the case studies are not wildly up-to-date, but that is not the point in this instance. What the case studies do is use a lens fashioned by the editors to throw serious light, pretty much for the first time, on the real-world trade-offs and hard choices that will be of critical importance to theorists who want to construct improved models of party competition based on more complex sets of motivational assumptions.

There is no space here to go into detail on the substance of each case study, but it should be said that the authors of the case studies have a sure touch with the detail of their material that should inspire confidence in those who might wish to build on these foundations. The main virtues of this book are thus the astute way in which the editors have identified a theoretically important matter that requires a preliminary empirical survey and the professionalism with which the authors of the case studies have set about their tasks. This is thus essential reading for those who have ambitions to build more complex models of party competition. It is also an attractive supplementary text for those teaching courses on party competition.

Shades of Citizenship: Race and the Census in Modern Politics. By Melissa Nobles. Stanford, CA: Stanford University Press, 2000. 248p. \$49.50 cloth, \$16.95 paper.

Howard Winant, *Temple University*

A thoughtful book on a subject that can be quite vexing, *Shades of Citizenship* benefits greatly from the comparative analytical framework employed. The central poles of comparative attention are the U.S. and Brazilian censuses, but Nobles also comments on a range of other national processes of census-taking and systems of racial classification employed; Germany and South Africa as well as other Latin American, African, and European countries are mentioned.

Censuses are treated as political in a dual sense. The obvious political logic of population enumeration is state centered: The census is a central instrument in the administration and organization of the nation/polity/society. It is a crucial tool of governmentality, which has all sorts of implications. The second political meaning is civil society based. In recent times the census has often been seen by groups as a vehicle for recognition; indeed, it has been viewed as a logical target of political mobilization by those excluded from the full benefits of citizenship or by those experiencing discrimination. Looking at censuses both in the United States and Brazil, and considering census politics both from the state and the civil society vantage points, Nobles develops a sophisticated comparative approach to this complex topic.

Race quickly becomes the touchstone of debates, the key issue, when we examine census politics. Why is that? Why are not classifications of social stratification/class, occupation, even education or health the central themes? The answer lies in the fundamental character of racial classification in many

societies (certainly in the United States and Brazil), the relatively intractable and ineluctable nature of racial identity, and the relationship, which is deeply rooted in national (and global) histories, between race and citizenship. To study census politics is in a fundamental way to study racial politics.

That is the overall rationale for this book, summarized here but well articulated by Nobles. The rationale is particularly important because the chief concerns of the project might easily be relegated to technical realms, might easily be dismissed, if the census were viewed as "simply" a means of information gathering. This is how many demographers would view the book, for example. But Nobles is explicit about framing her concerns differently, and that is the point of the extensive introductory chapter.

The fundamental concerns of the book are the historical evolution and fairly explicit racialism of the U.S. and Brazilian censuses as well as the way recent conflicts about and changes in procedures reflect those issues. Considerable attention is paid to state-centered political logic in framing and carrying out censuses. The author's crucial point is that strategies to demarcate racial categories and then locate populations in terms of them not only classify and measure, are not only investigative, but also are constructionist in both a social and political sense. This constructionism goes beyond the responsibility to create categories for empirical enumeration and analysis. It extends to the role of the state in "making" society or in this case race. This claim has large theoretical implications in many areas because it involves a certain view of racial formation; a political sociology, particularly of race and citizenship; and the historical treatment of ideas about race, a sort of sociology of racial knowledge or history of racial ideas. All this is presented with respect to the evolving census apparatus and practices in the two countries, from slavery to contemporary debates. I both commend the empirical work of Nobles and question the theoretical limits of the text. Although she offers a solidly grounded historical account of the evolution of census policy in both countries, there is very little interpretation. Yet, Nobles does signal her awareness of most of the interpretive questions I am raising, and she focuses her analysis on the question that most interests her: political struggles over racial meanings.

The key contemporary chapters present accounts of census-oriented popular campaigns in each country. In the United States the issue was framed (in the 1980s) around a "mixed-race category" and a proposal by the Office of Management and Budget (Statistical Directive No. 15) to incorporate such a category in the 1990 Census. In Brazil the issue was framed (in 1990–91) around "lightening" or "whitening": There was a limited but real effort on the part of a coalition of black groups to convince people to report their race "accurately," that is, not to pass for white. In each case nonwhite organizations demonstrated a much greater awareness of the political significance of census classification than ever before. As Nobles notes, the fact that these two campaigns emerged and prefigured future conflicts is itself a reflection of the changing dynamics of racial politics in the two countries.

In my view the situation was considerably more politicized in the United States than in Brazil: The U.S. civil rights movement and its legacy had no real equivalent in Brazil, and the Brazilian military regime eliminated race-focused questions from the 1970 and 1980 censuses (except for a limited household study). Thus, the reinstatement of race in the 1990 (or 1991, as it worked out) Brazilian count posed far more basic questions about race than existed in the United States. The comparative logic is limited as far as the details of the two campaigns are concerned. The chapter on the campaigns would have benefited from a more conceptual or more the-

oretically nuanced focus, which would have made the different political contexts more central. Despite this objection, Nobles's central concern—grassroots campaigns against the top-down politics of the census as framed by the state—is well addressed.

Although the book's conclusion is very solid, I want more. Nobles focuses on the (in)effectiveness of the census in capturing the meaning of race and its occlusion of, or even collusion in, the fundamental political questions that are the core of what race means, of its social-historical significance and so on. This she brings out very well, linking these themes to such issues as official classifications of race and methods of enumeration. That is very valuable. What I would like is a larger set of theoretical points about how and why these two states are constructing race in a certain way, about how racially defined minority movements understand these processes (and race itself) differently, and what this indicates for the comparative state of racial politics in the two countries. But that is a tall order. Notwithstanding these small criticisms, *Shades of Citizenship* is a very valuable, well-conceived, and well-researched work. It is important in several respects: as a study of racial politics (particularly on a comparative level), as an historical treatment of issues of race and citizenship, and as a contribution to our understanding of the census.

A Virtuous Circle: Political Communications in Postindustrial Societies. By Pippa Norris. Cambridge: Cambridge University Press, 2000. 398p. \$59.95 cloth, \$21.95 paper.

Stephanie Greco Larson, *Dickinson College*

Scholars of media and public opinion in Western Europe and the United States will find plenty of useful information and much to argue about in *A Virtuous Circle*. The data are rich and broad, and the conclusions drawn are provocative and relevant to some of the major debates in the field. The central question addressed is whether the news media discourages political engagement.

Pippa Norris challenges the increasingly popular idea that cynical news and passivity of television viewing create a disengaged public. Instead, she characterizes the relationship between news media and political attitudes and actions as a healthy and reciprocal one. This relationship is called a virtuous circle in which "watching the news activates existing predispositions to vote and, simultaneously, the predisposition to vote prompts people to seek out more news" (p. 264). Political interest and knowledge also stimulate attention to news, which results in more interest and knowledge. In addition, people who lack interest give news too little attention or trust to be adversely effected by it. Therefore, the news media is judged to have a positive to benign influence on civic life.

The evidence in support of these claims is cross-national and ample but, as the author admits, falls short of proving causation. Nevertheless, the conclusions resonate with long-standing minimal and conditional consequences theories of media effects and resurrect voting behavior studies that came before *The American Voter* (1960). The book reminds us that any rush to scapegoat the media not only overlooks other culprits but also ignores much of what we know about attitude change and information processing (e.g., selective perception, reinforcement, and the knowledge gap). Even those who find videomalaise (Michael Robinson, "Public Affairs Television and the Growth of Political Malaise," *American Political Science Review* 70 [June 1976]: 409–32), *The Spiral of Cynicism* (Joseph N. Cappella and Kathleen Hall Jamieson, 1997), and the notion that television culture has left Americans bowling alone (Robert Putnam, *Bowling Alone*, 2000) more

compelling than the “virtuous circle” (either rhetorically or substantively) will still need to consider this book’s critiques. The comparisons of media systems and public opinion in numerous democratic nations and the close look at the timing of trends in media use and political behavior raise serious concerns about the media malaise thesis.

Another contribution of this book is its careful and comprehensive discussion of how media have changed over time. The five chapters in Part 2 describe trends in political communication and the structural differences in news environments of 29 Organization for Economic Cooperation and Development (OECD) nations. The complexity of the current communication systems is evidenced by an expansion of news from diverse sources and formats, which makes a broad range of news available to growing audiences. New media supplement old media, rather than replace them. For example, Internet users still read newspapers and watch television. The special attention to the Internet audience (in chap. 2 and 12) raises concerns about how many people are fully engaged in the virtuous circle, since only 4% of Internet users contact groups and officials about politics or have political discussions online (p. 270).

Chapters 7 and 8 will be of particular interest to those who study the media and elections. Norris describes an “evolutionary processes of modernization” (p. 137) in campaign communication that entails three major stages: premodern, modern, and postmodern. This discussion judges the role of the media in contemporary campaigns less harshly than do most critics. “The postmodern campaign can be seen to represent a new openness and tolerance for alternative views and multiple forms of understanding, as well as a source of anxiety and disorientation as the familiar standards are swept away” (p. 178).

In addition to building the virtuous circle argument, chapters 9 and 10 stand alone as a comprehensive study of news coverage and public attitudes about the European Union. The extensive content analyses of 189 newspapers in 15 member states from January 1995 to autumn 1997 and 10,000 hours of television coverage from six countries is impressive and illuminating. It reveals that coverage of the European Union was minimal and focused primarily on monetary policy and development. Overall, the direction of this coverage was moderately anti-Union (especially on television). This tone coincided with a low level of public support for Union membership and the euro.

A Virtuous Circle is likely to stimulate research that supports, expands, and challenges it. More attention needs to be paid to the content and consequences of entertainment media, since this book points out that heightened political participation of television news viewers coincides with lower participation rates among heavy entertainment television viewers. In addition, the one panel survey used in this book suggests more questions than it answers. Therefore, laboratory and social experiments should be conducted to get a better sense of the causal mechanisms at work. Conclusions about how media environments, political institutions, and public opinion interact could also be enriched by studying non-Western nations. The effect of news on civic life in Latin America would be an interesting contrast to Europe, given the author’s brief discussion of their media and campaign systems. Finally, some scholars will likely follow the author’s advice to look to systemic problems to understand political disengagement. “Blaming the news media is easy, but ultimately that is a deeply conservative strategy, especially in a culture skeptical of regulation of the free press, and it diverts attention from the urgent need for real reforms to democratic institutions, which should have our undivided attention” (p. 319).

Uncivil Movements: The Armed Right Wing and Democracy in Latin America. By Leigh A. Payne. Baltimore, MD: Johns Hopkins University Press, 2000. 328p. \$42.50.

Martha Huggins, *Union College*

Leigh Payne greatly enriches our knowledge of Latin American transitions from authoritarianism to democracy. *The Armed Right Wing* focuses on the role of violent right-wing groups and government responses to them in three Latin American countries, with application elsewhere. Explaining that uncivil social movements “use political violence . . . to promote exclusionary objectives . . . as a deliberate strategy to eliminate, intimidate, and silence political adversaries” (p. 1), Payne contrasts these movements with “civil” social movements. They employ rule-breaking (and violence) to “expand [rather than curtail] citizen rights and freedoms” (p. 1).

Payne defines uncivil movements in the first chapter and in the second chapter develops her analytical approach. In three following chapters she presents three Latin American “uncivil” right-wing movements: Argentina’s promilitary *carapintada* after the “Dirty War”; the powerful Brazilian landowners’ Rural Democratic Union (UDR); and the Contra movement in Nicaragua after the civil war. This ambitious research project, which involved open-ended interviews and secondary research in three countries and three languages, uses carefully crafted structured-focused comparisons to explain the emergence, evolution, and demise of uncivil social movements.

Payne makes six central claims. Uncivil movements are a unique blend of civil-institutional and uncivil-mobilization politics. Political threats are necessary but not sufficient for the emergence and growth of uncivil movements. Leaders “sell” perceived political threats to potential members by using culturally available myths and images. Uncivil movements’ inherent contradictions weaken them over time, but not before they have negatively affected the democratic process. Democratic governments can exacerbate or lessen these movements’ negative effect. Uncivil movements can emerge in “consolidated” and transitional democracies.

Payne’s discussion of the difficult ethical, personal safety, and methodological problems faced by scholars who study secrecy and danger resonate with two recently completed projects of my own. How does one conduct research when groups’ activities are either totally or partially clandestine? How does one obtain a satisfactory sample of respondents when informants’ names are difficult to uncover? To address these problems, Payne started with each uncivil movement’s most visible leaders and used their trust to obtain interviews with others.

Once interviewees were identified, nagging ethical questions emerged: How does one strike a balance among protecting anonymity, disclosing illegal and/or human rights violations, and protecting one’s own personal safety? Payne’s solution was to disclose only the names of already well-known movement leaders—even though the possible cost was (and is) violence against her—and withhold the names of less powerful voices associated with or outside the movement. Anyone interested in social science research methods, especially the problems associated with interviewing powerful or not-so-powerful violence supporters and perpetrators, should read Payne’s fascinating explanation of her research methods.

The author skillfully incorporates into each case analysis a wealth of synthesized political science, sociological, and social-psychological thinking on social movements, and she rigorously weaves social movement variables into each case analysis. Payne astutely incorporates and revises political science institutionalization and social group mobilization

theories to bring out their policy implications for a government's handling of uncivil social movements.

Consider, for example, how Payne weaves interview data into social movement theory in her analysis of how uncivil movements develop political power. They grow by leaders "framing" and "cueing up" culturally resonant images to support their activities and by creating "legitimizing myths" and selling these to their publics. How this process unfolds and whether it uses sufficient culturally viable images in part shape the success or failure of an uncivil movement. Payne, for example, illustrates how Argentina's carapintada military officers and Brazil's landowner association successfully promoted their power by drawing both on older autochthonous cultural cues and by developing new ones. In contrast, she argues, the 1990s Nicaraguan Contras were unsuccessful as an uncivil social movement precisely because they were unable to frame their movement as distinct from, superior to, and more legitimate than other political groups.

A particular challenge for the leaders of uncivil social movements is to expand the appeal beyond the hardliners who make up the movement's core. Payne finds that an "entrepreneurial" charismatic figure is required to manage the process. A new framing and operational style develops out of these conflicts and adjustments.

For example, in order to convince "pragmatists," which uncivil movements need in order to grow, that the movement is legitimate, the charismatic entrepreneur must present himself as different from the movement's earlier authoritarian leaders. The fact that these new leaders—urban, educated, and urbane—are in fact different from their predecessors is not enough. Leaders have to "frame" and "cue up" the movement's new persona: Argentina's carapintada needed to be seen as not self-serving, ruthless military *caudillos*; Brazil's UDN could not be merely a reincarnation of the older rustic and uneducated landowner *coroneis*; Nicaragua's new Contra leaders were not merely brutal "Somosista" National Guardsmen. How and to what extent these changes are "sold" to pragmatists and a wider public will shape the course and operational success of each uncivil movement.

Factors inherent to these movements can promote or undermine them. If the goals are highly specific, then achieving success puts the movement out of a job. The original leader's charisma poses a destabilizing succession problem. The personal ambitions of these charismatic leaders often make them easy prey to those in government who subscribe to institution-building theories about democratically shaping and incorporating uncivil movements. Yet, political institution theorists will discover from Payne that incorporating these leaders or their agendas into democratic institutions can backfire: Lending them power and legitimacy disguises their violence, builds membership, and fosters their goals.

Payne recognizes the importance of timing: President Alfonsin of Argentina may have moved too soon and too strongly against the military. "Without the [military] trials the carapintada would not have emerged" (p. 97). This created conditions for the attenuated military coups. Governments may enter into negotiation with carefully selected movement factions, but Payne recommends they investigate uncivil movements and prosecute their violence. Transition governments must build up and reform judiciaries.

Payne's term "uncivil movements" should be discussed and debated. Some will ask why she did not use the more traditional designation "counterrevolutionary" in the usual continuum: "conservative," "reformist," and "revolutionary" social movements. Cannot the Contras—"contra" for "counterrevolutionary"—best be described as a counterrevolutionary movement? Is it questionable to define uncivil movements as only right-wing? Have there been un-

civil (exclusionist, violent) movements on the Left? Payne would argue that leftist movements struggle for the systemically disadvantaged, whereas uncivil movements protect, foster, or seek to restore the powers of the previously (or currently) advantaged. Every reader will find Payne's definition and designation of uncivil movements theoretically and analytically useful and thought provoking.

Another source of lively debate arises in Payne's last chapter, which compares uncivil movements in Latin America to those in other societies. In particular, she focuses on three consolidated democracies—Le Pen's Front National in France, Meir Kahane's KACH Zionist movement in Israel, and militia movements in the United States—and one in a transitional government, South Africa's AWB Afrikaner Resistance Movement. There will be debate and analysis about the movements left out of Payne's "uncivil" categorization.

Payne raises and addresses most questions, however, and just enough in her extraordinary analysis is left unstated to generate new and alternative questions and explanations. The literature on social movements, on politics, and on Latin America is significantly advanced by Payne's careful research and scholarly analysis.

Disaffected Democracies: What's Troubling the Trilateral Countries.

Edited by Susan J. Pharr and Robert D. Putnam. Princeton, NJ: Princeton University Press, 2000. 348p. \$65.00 cloth, \$19.95 paper.

Markus M. L. Crepaz, *University of Georgia*

A specter is haunting the Trilateral Democracies—this specter is called civic malaise. It has visited these countries before; rearing its head for the first time a quarter-century ago, proclaiming the demise of democracy due to the inability of governments to respond to the onslaught of waves of new forms of participatory democracy and political action. (Michel Crozier, Samuel P. Huntington, and Joji Watanuki, *The Crisis of Democracy: Report on the Governability of Democracies to the Trilateral Commission*, 1975). Too much democracy, as it were, may be too much of a good thing. Fortunately many of these dire predictions have not materialized, perhaps partly because they were highlighted a quarter-century ago. The sequel, *Disaffected Democracies*, celebrates the silver anniversary of the original *Crisis of Democracy*. This successor volume is similarly concerned with the fate of democracy in rich countries. These democracies are "troubled" (p. 7), so the argument goes, because their public institutions are undermined by declining confidence in government and sagging interpersonal social trust. The authors of this edited volume situate the sources of these "disturbing" (p. 13) developments squarely in the political sphere. In other words, the origins of the decline in confidence in political institutions is not explained by a frail social fabric but, rather, by failures of government and politics themselves. Despite a tight focus on the temporal (the last 25 years) and spatial (the Trilateral countries) parameters of this edited volume, it is refreshing to see so many diverse and innovative diagnoses as to what is ailing the rich democracies.

Of the 13 essays that compose the text (bracketed by a foreword by Samuel Huntington and an afterword by Ralf Dahrendorf), three are single country studies: two on Japan, by Hideo Otake and Susan J. Pharr, and one on the United States, by Anthony King. Even though King's contribution is entitled "Distrust of Government: Explaining American Exceptionalism," what all three of these essays have in common is that they locate the sources of civic erosion firmly in the untrustworthiness of politicians, for once making the United States NOT exceptional. Similarly, Donatella della Porta's

three-country comparison of Italy, France, and Germany finds that confidence in government was highest in Germany and lowest in Italy, where the lowest and highest levels of corruption were found, respectively. France took a medium position. The contributions by Alberto Alesina and Romain Wacziarg and by Kenneth Newton and Pippa Norris take a wider view, each looking at a larger number of countries. Both research teams conclude that the culprit of low public confidence may have to be found in the subpar performance of government policies and the institutions designed to implement these policies. The somewhat more "philosophical" contributions by Russell Hardin and Fritz Scharpf speculate on the decreased need for the state in modern societies leading to reduced trust in the state (Hardin) and on the quality of discourse between elites and masses: A more realistic discourse between these groups should prevent wishful thinking and bring the desires of the electorate closer to the actual capacities of governments (Scharpf). Pippa Norris' essay on the effect of watching television on social capital does not unequivocally find that the former undermines the latter, thereby reducing confidence and trust in government. It simply depends on what you watch. Peter Katzenstein looks at the lessons from smaller democracies and finds that neither the end of the Cold War nor internationalization is a valid predictor of the already much less accentuated civic malaise that has befallen the smaller countries of Europe. They are characterized by well-developed welfare states and inclusionary political structures that cushion their citizens from exogenous shocks, with the effect of creating a less polarized society with a citizenry that is not turned off by government.

Just when the reader's sense of impending doom in the rich democracies is becoming unbearable, along come the two essays by Russell Dalton and Sidney Tarrow, both of which put a more cheerful spin on this dire topic. Dalton argues, refreshingly, that value change is an ongoing process, driven by changes in the skills and knowledge of citizens in post-material societies, that institutions and political parties are constantly challenged, and that democratic regimes simply must adapt to these processes. For Tarrow, new forms of political activism have arisen, leading to short-lived and shifting coalitions with little likelihood of sustaining high levels of confidence in government.

As this description of the individual essays indicates, there is a rich diversity in explanations regarding the origins of the decline of public trust and confidence in government. This very diversity, however, has the effect of connecting the various essays rather tenuously with one another and, while they are superbly written and argued, encourages even more speculation as to what is actually disquieting the democracies of the world. Perhaps the liberal democracies have advanced "too far," i.e., the toxic mix of radical individualism, hyper-consumerism, and economic individualism makes "public" institutions look anachronistic. Perhaps people today are simply more educated, so they criticize more. Perhaps their lack of trust and confidence in governmental institutions can be interpreted as a maturation process, or a process of emancipation in which citizens take a more alert and cautionary stance against political institutions—indeed if this were the case, there would be no need to be "troubled." Or perhaps the reasons for the civic malaise are to be found in the decreasing capacity of the state to integrate ever more atomized, detached, and fragmented citizens? Is it conceivable that rising inequality, particularly in the United States, is reducing the pool of people who still trust government and have confidence in their political institutions? This book will certainly leave you with a strong desire to explore these fascinating topics further.

However, perhaps the most crucial question of all is, how seriously should we take these observations that seem to show that citizens are upset? With the advantage of hindsight we can say that the original message of *Crisis of Democracy* was unnecessarily alarmist. The democracies have adapted and become even stronger. Today's concern with the troubled democracies may again turn out to be nothing but a tempest in a teapot, 25 years hence. After getting closer to the substance, we may realize that this specter was nothing but a mirage after all.

The Anatomy of Public Opinion. By Jacob Shamir and Michal Shamir. Ann Arbor: University of Michigan Press, 2000. 320p. \$52.50.

Barry J. Balleck, *Georgia Southern University*

What is public opinion? In this aptly named book, authors Jacob Shamir and Michal Shamir attempt to address their perceived deficiencies in public opinion research by posing a new theoretical framework for the study of this important subject. Though hundreds of books, articles, and monographs have addressed public opinion, the authors contend that current theories of public opinion are too deterministic and that they fall short of explaining the full range of public opinion possibilities. Existing studies attempt to interpret public opinion on the basis of the observed outcomes—i.e., Why was a particular opinion expressed? What does it mean in the context of the instrument constructed to measure that opinion? The authors believe that to understand public opinion, one must come to understand the role of the information environment in which that opinion is located. In other words, Shamir and Shamir are not interested simply in the static outcomes of public opinion but in the environment in which that opinion is constructed. To this end, they propose a new theoretical construct by which to interpret it.

According to the authors, "Public opinion lies at the juncture of society, communication, and the individual; of the public and the private domains; of civil society and the state; of citizenry and politics; of masses and elites; of social control and rationality; of norms and events" (p. 2). Current theories are inadequate in that they have not captured the various constituent elements of public opinion, particularly the nuances and motivational factors by which public opinion is constructed and maintained. To correct these deficiencies, Shamir and Shamir propose a theory that lies at the confluence of communication, social psychology, social cognition, political science, and political psychology. They base their theory upon four premises.

First, public opinion is a social construct and, as such, is "perceivable, exposed, shared, and relevant to all citizens as well as to government" (p. 3). Public opinion is not static, but rather an "organic entity" which emerges and is continually recreated through societal discussion and debate. Public opinion is the product and reflection of the society in which it is found. However, it also helps to construct society in its continual expression. Public opinion thus maintains a symbiotic relationship with society.

Second, public opinion is multidimensional and cannot be reduced to a single expression. Current theories of public opinion concentrate on measuring attitudes and what these attitudes mean in reference to policy issues and/or political candidates. Such attempts do not assess the values of individuals or society, or the various dimensions that underlie these values. The authors attempt to assess public opinion in all of its facets while recognizing the various guises in which public opinion can manifest itself—i.e., symbolic, verbal, individual, group, electoral, etc.

Third, public opinion is dynamic and evolutionary. Shamir and Shamir contend that there are multifaceted divergences and convergences in public opinion. One such divergence, that of the concept of the "silent majority," is indicative of the discrepancy that exists between the evaluative and the expressed facets of public opinion. By coming to understand the silent majority and if it, in fact, exists, the authors hope to answer the "nagging political and theoretical question" of which public opinion the government should heed in making its policy choices.

Finally, public opinion consists of an "information environment" whose nature must be studied and understood in order to determine the processes that the expression of public opinion will set in motion. Previous studies of public opinion have included little about attitude distributions and the component elements of public opinion. The authors intend to study public opinion not simply from an observational standpoint, or from a point "outside the box," but from an interactive standpoint by climbing into the box and dissecting public opinion and then recombining it through meaningful interpretation.

With these four premises as their theoretical impetus, Shamir and Shamir set out to decipher the information environment in which public opinion is formed. Their task is a formidable one. Indeed, the authors recognize that their study is broad in its scope and that their efforts are but scratching the surface of a full understanding of public opinion. However, they are quite successful in laying the foundation for an interesting and innovative approach to public opinion research.

Using as their case study Israeli public opinion on issues of peace and territories before and during the first Intifada, Shamir and Shamir combine an innovative use of survey instruments and "thinking-aloud protocols" to help decipher the cognitive elements which comprise public opinion. Their research is guided by a rich and detailed survey of public opinion literature, though they rely upon the works of Elizabeth Noelle-Neumann to an unusual degree in their research. Despite their lengthy discussion of such terms as "pluralistic ignorance"—which is the gap between "aggregate distributions of opinion and their perceptions by the public at large" (p. 1), and which comprises the entirety of Chapter 5—the most interesting aspect of the authors' approach to understanding public opinion is their use of "thinking-aloud protocols."

Pioneered by Anders Ericsson and Herbert Simon (*Protocol Analysis: Verbal Reports As Data*, 1984), thinking-aloud protocols are designed to provide clues to the cognitive processing of individuals as found in their verbal reports. By utilizing the thinking-aloud methodology, Shamir and Shamir provide rich insights into the "information environment" of Israeli public opinion. Indeed, their interpretations of their subjects' responses to questions concerning peace and the return of territories during the Intifada period are crucial to understanding the multidimensional facets which make up the information environment as well as the variability of that environment.

By virtue of their methodology—particularly the thinking-aloud protocols—the authors conclude that estimates of public opinion are an exercise in everyday reasoning—not formal reasoning as evidenced by exercises which measure problem-solving abilities. Thus, people use cognitive shortcuts and heuristics when assessing public opinion, but the process is far from infallible. In fact, citizens still make mistakes in their assessments—such as those manifested by pluralistic ignorance. These mistakes occur because the cognitive shortcomings of individuals do not allow for adequate information processing to take place.

Believing that typical public opinion research has ignored motivations, the authors delve into how citizens construct their opinions, what information they rely upon, and how well they discern the overall environment of public opinion. Thus, a major issue for the authors is the role which values play in the formation and maintenance of attitudes. These values can be assessed in light of the "climate of opinion" which exists in any society. This climate in which the values of the individuals are found is a socially derived phenomenon which is collective and shared. The climate itself is the result of societal processes and interactions, such as those manifested by the direct relations of individuals (one-on-one interactions), small groups, various social networks, and large aggregations of individuals and groups (a community or state). Shamir and Shamir find that individuals base their opinions on their own personal experiences, as well as those of their family, friends, and colleagues and the groups of which they are a part. These findings are interpolated from the thinking-aloud protocols and reinforce the authors' contention that public opinion is not simply a number manifested by a poll or a sampling of attitude distributions. Rather, it is a dynamic phenomenon which is created by the information environment in which it is found, but is also re-created at various times and shapes the opinions of those who find themselves in that information environment.

The Rise of the Agricultural Welfare State: Institutions and Interest Group Power in the United States, France, and Japan. By Adam D. Sheingate. Princeton, NJ: Princeton University Press, 2001. 279p. \$45.00.

Grace Skogstad, *University of Toronto*

This is a helpful addition to the growing body of historical institutionalist literature that demonstrates the influence of macro- and sectoral-level institutions on policymaking. The central arguments, examined here with regard to agricultural policy, are two. First, institutional relationships among state and nonstate actors may facilitate one policy objective but impede other policy goals. Neither novel nor inconsistent with the literature, this proposition is advanced through elaboration of how various interrelationships between political parties and interest groups shape governments' policy capabilities. Second, Sheingate argues that the American institutional framework of dispersed authority and pluralism does not necessarily render governments incapable or subject to interest group capture. In advancing this proposition, he seeks to put paid to popular depictions of American agrarian politics as constituting iron triangles and all-powerful farm groups.

To elaborate these arguments and demonstrate the particular effects of American institutions on agricultural policymaking, Sheingate adopts a long historical perspective—from the 1860s through the close of the twentieth century—and contrasts developments in U.S. agricultural policy over this period with those in two countries with quite different institutional structures: France and Japan. The first objective is to demonstrate the interrelationships among the respective American and Japanese/French institutional structures of dispersed versus concentrated political authority, pluralist versus corporatist farm group representation, and separated versus aligned political party and farm group relations. The second goal is to show how these institutional relationships have affected governments' abilities to develop the "agricultural welfare state."

Sheingate argues that four phases are common to all three countries and present "a distinct policy task that requires different relations between government and farmers" (p. 21):

the era of promotional policies (1860s to 1910s), market intervention and production controls (1920s to 1940s), a mature subsidy regime (1950s and 1960s), and retrenchment (1970s onward). U.S. institutions, he maintains, facilitated the first and last phases and frustrated the second and third. The policy effects of the French and Japanese institutional frameworks were just the opposite, enabling market intervention and a mature subsidy regime but handicapping retrenchment policies.

This historical and comparative analysis is well executed. Although the national agricultural policy expert is unlikely to learn anything new—the discussion of Japanese and French agricultural policies draws heavily on secondary sources, and English sources alone in the case of Japan—the book makes a contribution by virtue of its historical sweep and the quality of research. Analyses are well documented, with good use of summary tables and figures to capture distinctive expenditure patterns, structural transformations in agriculture, and agricultural representation in national legislatures.

The Rise of the Agricultural Welfare State is not without limitations. In contrast to other historical institutionalists who seek to examine the role of ideas in policy making, as factors in their own right and in interaction with institutions, Sheingate makes no such attempt. The analyses here are resolutely structural, save for an occasional nod to the role of exogenous factors, such as the state of the economy and international developments. The author dismisses the possibility of ideational differences between the U.S. and Japan/France regarding agriculture's contribution to the public good (p. 36), and he makes no effort to examine the role they play in agricultural policy outcomes across the three countries. Institutions bear virtually all the explanatory weight for policy outcomes and government policy capabilities. To carry the burden, they are sometimes miscast (e.g., the relationship between French farm groups and the state is described as more corporatist than the reality of plural and competing farm groups warrants), and their importance is overplayed. An example of the latter is the claim regarding the significance of links between French conservative parties and the dominant farm group. There is evidence that the power of the French farm lobby transcends the ideology of the governing party, which suggests the need for an argument that goes beyond the organizational strength of French farmers and their linkages to political parties. It requires recognition of the role that ideas about the multifunctionality of agriculture play in agrarian politics in France and the European Union more generally.

On occasion, theory-building and empirical data are not fully compatible. Sheingate constructs a careful theoretical argument on how the U.S. institutional framework facilitated retrenchment of agricultural policies whereas the French and Japanese frameworks thwarted it. Retrenchment policies, he argues, require frameworks like the American system that facilitate blame avoidance, venue change, and entrepreneurship. But the argument is problematic empirically, undermined by expenditure data—Sheingate's chief indicator of retrenchment—subsequent to the 1996 agricultural reforms, which demonstrate sharp escalations in government transfers to American farmers. Expenditure data are only a partial measure of a government's capacity to effect radical reforms, and Sheingate's undue reliance on them undermines the strength of his own claims about the policy autonomy afforded governments by the American agricultural institutional framework. To his credit, Sheingate stresses the contingency of radical policy reform in the U.S. institutional framework and the role of exogenous factors, but he remains adamant that interest group "capture" is impossible in this institutional context (p. 239). The reservation of that appelle-

lation to state-farm group relations in Japan and France is probably not unwarranted. At least in the instance of Japan, however, it lacks the nuance of a relationship under stress as detailed by Aurelia George Mulgan, a leading authority on Japanese agrarian politics (*The Politics of Agriculture in Japan*, 2000).

These examples illustrate the book's capacity to provoke. They do not diminish its theoretical and empirical contribution to comparative public policy and to our understanding of the effect of institutions on policymaking and policy reform.

The Politics of Institutional Choice: The Formation of the Russian State Duma. By Steven S. Smith and Thomas F. Remington. Princeton, NJ: Princeton University Press, 2001. 180p. \$42.50 cloth, \$16.95 paper.

John Londregan, *Princeton University*

Politics of Institutional Choice is an important contribution to the literature on legislative institutions. The authors' backgrounds complement each other to good effect. The result is a study that is both conversant with the literature on legislative politics in the United States and Western Europe and solidly grounded in the politics of contemporary Russia. The collapse of the Soviet Union and the adoption of reformed legislative institutions by the Russian Republic in 1993 left the newly elected representatives with the need to devise a working set of parliamentary institutions for the newly formed bicameral legislature. The "building materials" out of which these were fashioned—legislative committees, party caucuses, rules allocating agenda control to leaders—resemble those of the U.S. Congress and Western European parliaments, but the institutional structure was adapted to the needs of Russian politics.

Smith and Remington use the choices made by Russian legislators as they crafted their new parliament to learn about the motives of members of the Duma in particular and about legislators in general. The authors are interested in measuring the relative weight of what they call "policy incentives," "electoral incentives," and "party incentives." To do this they examine three attributes of legislators: policy preferences, measured on a one-dimensional continuum from Left to Right; mode of election, that is, some Duma members were elected from single-member districts (SMDs), and others were chosen through a system of proportional representation (PR); and faction membership. These three characteristics create a taxonomy of Duma members.

The authors link the three characteristics to "incentives," but this is problematic. For example, knowing that PR and SMD deputies behave differently does not directly measure the strength of reelection incentives (often referred to in other literature as "office motivation"). Differences in what it takes to be elected from SMD and PR constituencies may lead office-motivated deputies from SMD constituencies to behave differently from their PR counterparts, but this is only one of many possible explanations. Another is that ideologically motivated independent deputies with no interest in reelection may view competing on an artificial party list as an infringement on their independence, whereas ideologically motivated members of major parties find competing on PR lists more congenial. The link between incentives and a characteristic such as a deputy's mode of election is not airtight.

Smith and Remington use the multitude of institutional choices made during the mid-1990s by Duma members to gauge the effect of their three measured characteristics. They also look for, and find, evidence that preferences toward

institutional arrangements evolved over time as deputies learned. The taxonomy is both a strength and a weakness. It enables the authors to ask concrete questions that can be answered, but this comes at the cost of not being able to address fundamental questions about legislatures directly. For example, how important is the "information processing" role of committees relative to their role as gatekeepers for political parties or special interests? To what extent do legislators join parties because of their policy preferences, and to what extent are their preferences the result of their party affiliation?

The bulk of the book uses several important decisions reached during the Duma's first session as "natural experiments." The authors look for statistically significant effects of mode of election, policy leanings, and membership in a legislative faction. They skillfully draw on a range of evidence, including roll-call votes, popular election returns, organizational data, and the results of two detailed attitude surveys of Duma members. The result is a useful array of what might be called "stylized facts." For example, party discipline was highest among the communists and was generally better explained by faction membership than by location along a Left-Right continuum.

The book is organized around a core of substantive chapters, each of which begins by setting forth a set of propositions. Various relevant roll-call votes and survey responses are then used to test the propositions. Those that survive become the book's stylized facts, such as proposition 2.2, which states that SMD deputies were more opposed to allowing parties to expel PR deputies who bolted their party. The authors could do a better job of tying the hypotheses to the literature on party control and mode of election. Some of this is done, but many propositions are introduced on the basis of their intuitive appeal, and the opportunity to draw wider connections seems not to be fully exploited.

Perhaps Smith and Remington should have noted more often the relationship between legislative politics and the executive in the Russian Federation. For example, they present convincing evidence that the parties on the Left tend to prefer more centralized parliamentary control that would allow the majority to legislate more easily, whereas the "reform" parties of the Right oppose such measures. Readers unaware of the proreform president's ability to bypass the legislature using his decree powers will find this affinity of proreform legislators for the status quo puzzling, even in an opposition controlled legislature. A modicum of extra background material would have made this important book more accessible to scholars who do not specialize on Russia.

The authors are conscientious about describing their data, and they use logit models that treat their explanatory variables, such as faction membership and mode of election, as "risk factors" for voting a certain way or expressing a certain opinion. This offers a useful window on the behavioral links in their data. There is a technical problem, however. We can think of the dependent variable in a logit model as taking on two values, "success" and "failure," and the model then estimates the probability of success conditional on the values for the explanatory variables. For example, Smith and Remington analyze a vote taken in the Duma in January 1994 on whether to require any group of deputies who want to organize into a faction to have at least 50 members.

In this case, "success" corresponds to voting in favor of the amendment, "failure" to voting against it, and the explanatory variables include indicators of the faction to which a member belongs. In Table 2.2 (p. 41) the authors report that every member of the Liberal Democratic Party of Russia (LDPR) who participated in the roll-call vote was in favor, but no member of the Party of Russian Unity and Accord (PRES) voted for the amendment. An explanatory variable

such as membership in LDPR that perfectly predicts success (in this case, a "yes" vote) corresponds to a logit coefficient of "positive infinity." Likewise, a "no" vote corresponds to a logit coefficient of "negative infinity." This is tantamount to removing all the LDPR and PRES members from the dataset before estimating the remaining coefficients.

A classic mistake is to include a variable such as LDPR or PRES, estimate the logit model via maximum likelihood, and stop the computer algorithm when the likelihood function ceases to register large improvements. The results will typically be an implausibly large but statistically insignificant positive coefficient estimate for the variables that perfectly predict "success" and an implausibly large negative coefficient estimate for variables that perfectly predict "failure." This is just what we see in Table 2.3 (p. 43), and it is not an isolated event. The same problem arises at least seven other times in the same table and at various other points in the book (e.g., the "DPR" coefficient in all three columns of Table 2.5, the "NRP" coefficient in both columns of Table 5.6). The authors need to be sure that removing these hidden "perfect predictors" from their data and reestimating their model does not affect the remaining coefficient estimates.

Assuming that the substantive implications of this methodological problem are minor, the book hinges on the authors' energetic pursuit of stylized facts that link behavior with their taxonomy of member characteristics. Their approach has the advantage of posing concrete questions and finding answers: Do SMD deputies exhibit lower party discipline than their PR colleagues? "Yes," barely. But the connection with members' motives is unresolved: Do the high rates of party discipline on roll-call votes mean that members join factions that reveal their policy preferences (Smith and Remington think so), or do they reflect party whipping (the authors observed no welts during their interviews with legislators)? Yet, this is a generic problem faced by any study that is primarily empirical. The theoretical controversies in legislative politics remain controversial because they tie in ambiguously to objective measures.

The authors make a serious effort to address these wider questions. Although they do not set any of the major controversies to rest, their results bring the workings of the Duma into clearer focus.

The Left's Dirty Job: The Politics of Industrial Restructuring in France and Spain. By W. Rand Smith. Pittsburgh, PA: University of Pittsburgh Press, 1998. 363p. \$50.00 cloth, \$22.95 paper.

Michael Loriaux, *Northwestern University*

W. Rand Smith compares socialist policies of industrial retrenchment in France and Spain during the 1980s and 1990s. Both governments sought to adapt their national economy to change in the global market, through investment incentives and labor policies, in a way that would avoid sectoral crisis or even collapse. They sought to achieve an "orderly exit" of labor from redundancy-plagued industrial sectors, notably steel and automobiles, through job retraining, help in establishing small businesses, relocation incentives, and improvements in the job market, not to mention such standard support mechanisms as severance payments and preretirement systems that supported the incomes of unemployed workers. There was a distinct convergence between French and Spanish policy around this kind of adaptive policy. Neither country after 1983 resisted global market trends through price controls or subsidies or trade protection, and neither government embraced market adjustment through more liberal policies of deregulation of capital or labor markets.

International structural factors do not explain this convergence, since those factors could just as well entail a more liberal response than the one the two governments adopted. A number of domestic factors do explain it. Weak industrial capacity is one. French industrial firms were in dire financial shape in the early 1980s, and Spain's legacy of backwardness and protectionism made its industry even more fragile. A second factor is the capacity of both states to intervene decisively to manage adjustment. Both had a powerful executive and centralized administration. Many of the industries that needed restructuring were state-owned. Furthermore, both governments enjoyed strong electoral and parliamentary majorities. The Spanish socialists were able to deploy policies that their more conservative predecessors had tried to implement but failed. The French socialists were able to effect a complete reversal in policy approach without major loss of parliamentary support. Finally, both governments informed policy with the same ideological preference for reconciling market efficiency with the activist state.

Smith devotes much of his analysis not to convergence but to differences between the two experiences. The Spanish elaborated strategies of adjustment that were more coherent than the French but in the end less effective. The author explains the greater incoherence of French policy with reference to the greater need in France to build a governing coalition among the disparate forces of the political and labor Left. The French Left that assumed power in 1981 was an alliance of a rather Stalinist Communist Party, a splinter of the old anticlerical liberal party, the Radicaux de Gauche, and a Socialist Party that was itself a coalition of four more or less independent political forces, ranging from the far left Centre d'Etudes de Recherche et d'Education Socialistes of Jean-Pierre Chevènement to the modernizing right wing of Michel Rocard's Parti Socialiste Unifié. Incoherence in policy reflects efforts to keep this wide-ranging coalition together.

The Spanish socialists were able to govern with minimal concerns about alliance politics. Felipe Gonzalez had solidified the Socialist Party by eliminating internal factions and by centralizing control over the party apparatus. The Spanish Communist Party was in decline, and the socialists were not compelled to enter into electoral alliance with them.

Smith explains the greater effectiveness of French policy with reference to the greater interventionist capacity of the French state and the greater weakness of French labor unions. Divisions among the unions facilitated the implementation of policy in France, whereas labor unity occasionally threatened implementation of policy in Spain. This dynamic was most apparent in the steel industry. Organized labor in neither country was able to deter the government from adapting steel to a slow market, but Spanish unions mobilized more effectively and extracted more concessions. Despite many protests in French coal and steel regions there was little effective action, in part because of divergent analyses among the unions regarding the nature of the crisis and the appropriate response. The independent Confédération Française Démocratique du Travail sympathized with the need to adapt the industry to the slow market, but the procommunist Confédération Générale du Travail sought to subsidize investment and retain labor.

Smith eschews moncausal explanations and provides a rich, textured political analysis that he lays out with great clarity. He returns again and again to the importance of coalition building and the politics that informs it. In so doing, he reinvests agency in the actors who make policy and demotes the importance of international structural constraints. This is a good, rewarding book.

Unemployment and Government: Genealogies of the Social.

By William Walters. Cambridge: Cambridge University Press, 2000. 240p. \$64.95 cloth, \$19.95 paper.

Stephen McBride, *Simon Fraser University*

William Walters probes understandings of the concepts of unemployment that developed and, to an extent, succeeded one another over a hundred years in British history. The British case has its own unique trajectory but parallels developments elsewhere, so the book's interest is not confined to specialists in British politics or social history. Walters applies a Foucauld-inspired governmentality perspective to unemployment in order both to overcome the routine and familiar understandings that have become attached to the concept and, more broadly, to contribute to what he terms a genealogy of the social. The latter refers to a particular sphere of governance as exemplified by the practices, techniques, and institutions devised to govern processes and problem populations, in this case the unemployed. Genealogical approaches are suspicious of generalization and systematization and hold that "we can learn from the particular and the contextual" (p. 8). Thus, Walters argues, the government of unemployment will be instructive for understanding social governance generally.

Walters charts this route into the unemployment question and presents a valuable account of many of its facets. The discussion of the transition of unemployment from a moral problem (based on individual failings), to a social problem, with unemployment just one of the risks inherent in the operation of an industrial economy (and one sufficiently predictable that it could be insured against), to an economic problem (lack of aggregate demand) that appropriate Keynesian measures could counteract and control is nicely done. Similarly, the examination of administrative practices and the way they link to and reinforce certain concepts of unemployment is insightful.

The duality of purpose, in which the genealogy of the social looms as theoretically most important, leads Walters to a series of disclaimers throughout the study that seek to define what the book is not. It is not concerned with the historical experience of unemployment or its causes, and it is not a history of unemployment or of the welfare state. Rather, it is a historical sociology of the governance of unemployment. This technique serves to situate the book but perhaps is overdone and certainly lends a defensive tone to the discussion. This is compounded by the determined avoidance of confronting or testing the interpretation against those from other perspectives.

At times the disclaimers "protest too much." Walters obviously does not want the book to be considered simply as a history of ideas; yet, in pursuit of his own project, he provides a very good history of the idea of unemployment. Notwithstanding his emphasis on the institutional construction of unemployment, or techniques for governing unemployment and the unemployed, it is the ideas (definitions, conceptualization, and so on) that come through most prominently. The institutions are enmeshed in the production of ideas, new forms of knowledge about unemployment, new ways of categorizing and thinking about the subject. Conceived in this way an institution ostensibly set up to deal with unemployment "does not simply find unemployment already there. It produces unemployment.... In a sense, we are talking about the invention of unemployment" (p. 47).

The author does not consider unemployment a preexisting condition; rather, it is constructed—through discourse and through the practice of government agencies, such as the labor exchanges of the early twentieth century, established to deal with the recently defined phenomenon. This analysis contrasts with social policy or structural accounts of unemployment. But Walters does not confront that literature

directly; his account evades it and is offered as another way of looking at the unemployment issue, a supplementary account perhaps.

The social conditions that made it possible to "invent" unemployment are alluded to, and it is recognized (p. 15) that changes in economic and occupational structure played a role in developing the notion of employment (and unemployment). But the failure to discuss adequately the maturation of a capitalist labor market and its first real crisis, the Great Depression in the 1870s, weakens the plausibility of this account of the arrival of the concept of unemployment in that period. The view that concepts reflect reality rather than constitute it, and institutions respond to problems rather than invent them, has much to commend it; avoidance is not an effective strategy for dealing with that position.

With Keynesianism, Walters observes, the seemingly natural link between governance of unemployment and simultaneous government of the unemployed was temporarily broken. "Temporarily" because recent conceptions of unemployment have returned to the notion of individual culpability for the experience, as expressed, for example, in explanations that rely on deficiency of skills and poor attitudes among the unemployed. On this point, Walters presents a case study of New Labour's "New Deal," a work for welfare program. The initiative is presented as steering a path between the neoliberalism of the Thatcher Conservatives and the statism of old Labour.

The case rests on the view that the New Deal is partly an ethical and moral project and thus "is shielded from the criticism that training and jobseeking schemes are somewhat limited in tackling persistent unemployment because they do little to affect the aggregate supply of jobs; it is about the ethical as much as the economic" (p. 132). Others claim that the New Deal creates few jobs, perhaps 80% of which would have been created anyway, and from which 60% of the participants drop out, and might regard it as an exercise in the politics of manipulation rather than morality. But addressing such claims is beyond the defined scope of the book, which consistently side-steps confrontation with other perspectives.

Ultimately, therefore, the genealogy of the social perspective, with its emphasis on discourse and the "invention" of social phenomena, limits itself by its failure to engage with interpretations that see institutions as responding, in various and often contradictory ways, to social problems that are, however, extant rather than invented.

Power and Wealth in Rural China: The Political Economy of Institutional Change. By Susan H. Whiting. Cambridge: Cambridge University Press, 2001. 348p. \$59.95.

Benedict Stavis, *Temple University*

While this book does not quite cover the broad range promised by its title, it does offer a sophisticated analysis of the privatization of rural industry in China, thick in social science theory and rich with empirical data.

Whiting poses an important question: Why did the Chinese communist system endorse the expansion of private ownership of rural industries, rather than emphasizing collectively owned enterprises? What were the administrative links and feedback mechanisms among central policymakers, local administrators, and China's entrepreneurs and rural workers that led to privatization? For decades, a primary presumption in the scholarship of socialist systems was that communist bureaucrats had such deep vested interests in state ownership of economic enterprises that they would never convert. But Whiting reaches the startling conclusion that it was easier for local officials to tax and control private enterprises

than socialist enterprises that were embedded in the state structure.

The tax system is crucial to her analysis. Most tax revenue came from the turnover and income taxes on rural industry. Taxes went up to the central government, but a share was retained locally, so local officials had an interest in expanding local industry. At the same time, local officials were given extensive local obligations, including education, health, etc., so they needed more revenue. Thus, they needed to increase rural industry and other sources of discretionary local income. They would be rewarded with promotions and higher salaries if they succeeded. Whiting's key point is that local officials were eager to increase their revenue, and higher levels agreed on this (Chapter 3).

In regions where the central government had earlier encouraged rural industry in the collective sector (rural Shanghai and Wuxi provide case studies for Whiting for this pattern of development), further development of collective industry was the easiest solution. Local leaders already had much influence over the collective sector and could easily get information to obtain tax revenues from them. However, and this is one of Whiting's key discoveries, local collective industry deliberately kept profits low by inflating expenses. They overstated the number of employees and expanded entertainment costs to shift expenses to tax deductions. As enterprises avoided taxes, local officials benefited indirectly from better economic and social conditions, but, at the same time, their tax revenue for themselves and higher levels was limited. (This subterfuge may also have resulted in Chinese and foreign analysts overstating the inefficiencies of the collective system.)

In areas where the central government had not helped to develop collective rural industry, private rural industry got a head start. (Whiting's case study work on this pattern is in Yueqing, near Wenzhou in southern Chejiang.) Rural officials had essentially no way of knowing the financial accounts of private industry for tax purposes, so they developed a far simpler institutional environment. A new form of enterprise, the "share-holding cooperative enterprise," was created. It was a private firm, with the restriction that it had to reinvest 50% of profits and contribute at least 25% of profits to public accumulation funds (p. 160). Moreover, a simplified sales tax of about 9–10% of gross production replaced a complex set of taxes that required far more intrusive information to compute. Both private entrepreneurs and local officials gauged this a reasonable deal. The government got reasonably high tax revenues fairly easily. Moreover, if competition and over-production started to push prices down and banks refused to loan money to failing firms, the local officials had no financial obligation to private firms if they went into debt and possible eventual bankruptcy. At the same time, private entrepreneurs got their private investment protected with a new, safe legal status (p. 177).

Thus Whiting arrives at an important and counter-intuitive insight, that local officials could do their jobs and please their superiors more easily by dealing with private enterprises than with collective ones. By the late 1990s, private ownership was becoming better accepted and protected and was expanding as a part of China's economic system.

At a macro-political level, her insights show that Chinese citizens do make inputs into the development of state policy. The modality is not through an institutionalized formal democratic process of elections between competing parties; rather it is by withholding resources from the state by locally sanctioned tax minimization and forcing the central government to adopt new policies that encounter less resistance.

From a methodological perspective, Whiting's book is a wonderful model. Her use of public documents is superb. She

has found a wide range of yearbooks, government directives, and collections of articles on both local and national levels that provide a very full image of rural industrial development. She has supplemented this with 252 interviews, so that she can convert cold laws and statistics into living social, economic, and political processes. Quantitative analyses of economic and financial data further confirm some of her hypotheses. She proves that one does not need to take the risks of internal (*neipu*) documents or secret materials to do in-depth research in China.

While Whiting's argument is persuasive, it is not necessarily complete. I wish that she had explored more systematically whether officials encourage private economic development because it gives them more opportunities to help their children, other relatives and friends, and maybe even themselves to become successful businessmen and leap above the financial constraints imposed on public servants. She tantalizes us with hints, “[L]ocal party secretaries and township executives . . . translat[ed] their political power into ownership of a relatively large number of individual shares in the most valuable firms, often at highly favorable prices” (p. 156), but does not treat this issue with the same rigor that she does her other hypotheses.

All in all, this is a fine contribution to our understanding of the internal dynamics of China's policy process as it continues to reform the economic system. It reminds us of the central importance of public finance institutions and policies to sustain a state. It is clear that the Chinese Communist Party understands this requirement.

Continuities in Poland's Permanent Transition. By Harald Wydra. New York: St. Martin's Press, 2000. 257p. \$65.00.

Arista Maria Cirtautas, *University of Virginia*

In studying and analyzing the postcommunist “transition” countries, it is becoming ever more apparent that what calls for explanation is not only change, as expressed in the trajectory of liberal capitalist reforms, but also continuity. Even in the face of profound institutional transformation, mentalities and behaviors associated with the past are not readily giving way to new modalities of thought and action. Accordingly, one way of approaching the problem of explaining the variability of regime outcomes in the former Soviet bloc is to focus on the particular institutional and discursive forms that the interplay of change and continuity has produced in each country. In his work on Poland, Harald Wydra encourages us to analyze this interplay from a particular point of view—one that is rooted in the lived experiences of the populations involved, as opposed to how countries measure up with respect to progress toward liberal capitalist outcomes. Consequently, he argues that even as discontinuity and change have characterized the “first reality” of post-1989 institution-building, important continuities mark the “second reality of images, myths and mentalities” (p. 26). In turn, this “second reality” provides “fundamental reference points in post-1989 Poland and Eastern Europe” (p. 25) that infuse the new institutional realities with unique, culturally determined, content and meaning.

As Wydra sees it, neither elites nor publics have been autonomous or free to respond to changing circumstances according to detached rational cost-benefit calculations. Instead, their responses have been driven, at the unconscious level, by inherited structures of thought that generate collective patterns of interpretation and “processes of situational adjustment” (p. 22). These patterns, while fluid and indeterminate as to how specifically individuals or groups might combine elements of the inherited structures, are

nonetheless predictable within the parameters of a society's or group's given cultural *habitus*. Although these inherited mentalities have most often been characterized as part of the negative legacies of communism, as in the “*homo sovieticus*” syndrome, Wydra remains refreshingly free of such value-laden terminology. Nor does he assume that the less educated social groups are the sole carriers of inherited mentalities. Indeed, his empirical focus is on Poland's political elites and how their choices and actions have been impacted by continuities at the level of the second reality. This is consistent with his theoretical aim of “detaching the concept of second reality from a functional meaning in the service of the antagonism of state vs. society” (p. 17). For Wydra, it is too simplistic and ultimately misleading to reduce the continuities of the “passions and identities” of contemporary Eastern Europe to a single faultline that pits unreconstructed societies against “modernizing” state elites. In contrast, Wydra directs our attention toward a more generalizable condition that continues to structure mentalities and situational responses regardless of social class. This condition is characterized by the perception of “the menacing dissolution of order.” Hence, in the particular circumstances of Poland, “second realities should be regarded as subjective-emotional expressions of desire to leave an unwanted or unsatisfactory situation” (p. 17) and return to order or normalcy. According to Wydra, this desire has continuously shaped Polish political culture from 1945, and even before, to the present day.

Central to the sustainability of the “pre-political identities” inherited from the communist and precommunist past has been the condition of liminality or “permanent transition” in which Poland has found itself for most of the 20th century. Or, to put it more precisely, Polish elites have consistently interpreted their country's condition as one of transition from an undesirable state to a more desirable one. In this regard, the post-1989 political establishment is no different from the post-WWII communist leadership. The empirical substance of Wydra's work, and the bulk of the chapters, elaborates on the three main consequences of this worldview: (1) A utopian public discourse that is often couched in terms of mirror images or “mimetic conflicts;” (2) the organized expression of this discourse in the “anti-structures” of “moments of nationwide communities” (p. 25), most dramatically illustrated by Solidarity; and (3) the “backwardness” of Polish political elites resulting from “their imitation of blurred images of democracy and capitalism and their recurrent reference to images of the past” (p. 30).

The major contribution of Wydra's work is not, however, his empirical findings, since he relies almost exclusively on secondary sources. The greater value clearly lies in Wydra's efforts to analyze, on the basis of the Polish case, the interactions between the objective socioeconomic conditions of backwardness and the “subjective-emotional” interpretation of these conditions, and how these interpretations become consequential in their own right. This mode of analysis draws our attention to the fact that although the Central and East European countries share many objective structural attributes of backwardness and late development, the ways in which these attributes have historically been interpreted and internalized have varied: Variances which in turn have contributed to the differential outcomes of the post-1989 period. Thus, while Poland, Hungary, and the Czech Republic currently seem similarly poised to escape the debilitating “in-between” state of backwardness by becoming fully consolidated democracies en route to European Union membership, the conflicts and cleavages that inform the political struggles and experiences of elites and citizens in these countries remain quite distinct as a result of different political traditions

or, in Wydra's terms, the continuities of differing "national and social habitus" (p. 192).

Ultimately, however, Wydra's contribution to a cultural theory of change and continuity in postcommunist polities is undermined by inconsistencies in the logic of his argument, an underelaborated analytical framework, and the lack of serious engagement with alternative approaches. There is, for example, an apparent glaring inconsistency between Wydra's assertion that his "understanding of transition should be separated both from the fashionable 'transition to democracy' and from transitional post-communism as an abnormal state of affairs" (p. 202) and the implicit reference point of Western practices and mentalities employed by the author. Polish politics is, in the final analysis, evaluated according to its "considerable distance from Western democracies in terms of political trust, social cohesion, and stable political identities" (p. 204). Given such an underlying comparison between a Polish "national and social habitus" characterized by the pursuit of (unrealizable) utopian principles and the (unrealistic) perpetual demand for change, as contrasted with the author's own idealized understanding of the "normal" habitus of Western democracies, it is at times rather hard to see the difference between Wydra's approach and those that explicitly treat "transitional post-communism as an abnormal state of af-

fairs." Such inconsistencies become even more problematic in the absence of a fully developed analytical framework. Wydra suggests that his analysis is guided by insights derived from the work of Norbert Elias, Arnold van Gennep, Victor Turner, Rene Girard, and Bruno Latour (see, especially, page 25). Little effort, however, is made to relate these insights to one another in a systematic and coherent way that would help the reader understand the author's claims, where his work fits into the existing literature on cultural theory and how, in substance, his approach might deviate from alternative approaches to the study of postcommunism. Surprisingly, Wydra does not refer to the growing body of literature on the cultural aspects of postcommunist transition. As a result, when the author claims that "continuities in Poland's transition are neither evolutionary nor linear, but repetitive and ritual-like" (p. 191), not only is the contrast itself opaque, but it is unclear how his overall analysis of the continuity of "second reality" images and myths differs from other culturally oriented studies of contemporary Eastern Europe.

In short, this work should be read both with a sense of appreciation for the author's efforts to recast our analytical perceptions of the transition and with a critical sense of how this project might be improved upon.

International Relations

German Unification and the Union of Europe: The Domestic Politics of Integration Policy. By Jeffrey Anderson. Cambridge: Cambridge University Press, 1999. 240p. \$59.95 cloth, \$21.95 paper.

Beverly Crawford, *University of California, Berkeley*

German unification presents conceptual puzzles of which comparativists dream. Has this monumental change, which boils down to full German sovereignty, growth of German power, and the emergence of new domestic political interests, altered Germany's relationship to Europe? Is Germany withdrawing from or dominating European institutions? Does the new Germany still tread its well-worn postwar path of the model "European"? The questions are important for our understanding of the sources of policy change and continuity as well as the process of regional integration in general and the course of European integration in particular. In which issue areas has Germany's postunification policy broken with the past? Is the break caused by changes in domestic politics or the increase in the power of a unified and fully sovereign Germany? Have policy changes impeded or enhanced the speed and character of European unification? Are important continuities evident? If both policy continuity and change are present, why the variation? These are the questions Jeffrey Anderson tackles in this timely and important volume.

In the decade since unification Germany's European policy has been characterized by both continuity and change. Situated securely—but not exclusively—at the domestic level of analysis, Anderson's explanation relies on the particular interaction of institutions, ideas, and interests before and after unification. He makes an important distinction between the constitutive and regulative dimensions of the European integration process, arguing that Germany has exhibited seamless continuity in constitutive politics because German "identity" was so entwined with Europe. Furthermore, as European integration deepened during the postwar period, Germany

participated in shaping European institutions and securing its domestic interests within the European Union (EU).

It is within regulative policy that the real puzzle is revealed. In some areas (trade and internal market affairs) Germany's postunification policy remained constant but in others (environmental and energy policies) exhibited subtle shifts, and in still others (structural funds, state aid, and agricultural support) it underwent dramatic changes. The key to continuity and change rests with domestic political interests, their response to the unification process, the degree to which domestic actors' desire for policy change resonated with long-standing German economic beliefs, and the availability of domestic and EU institutions to translate actor demands into policy choice. When interests conformed to prevailing ideas and when institutional channels were available, domestic actors were able to push through their policy preferences. When their interests contradicted prevailing ideas and when few institutional channels were available for their voice to be heard, their policy demands were thwarted. Entrenched ideas and the institutions that support them are of crucial explanatory importance in cases in which the demands of domestic actors pose a challenge to prevailing policy; they are of little direct importance when they support—or at least do not clash with—the interests demanding policy change.

Anderson provides solid empirical evidence for this argument. Unification put the entire German economy under considerable stress and affected the interests of all economic actors. Eastern firms, labor unions, and state governments clamored for trade subsidies to enhance competitiveness, but these contradicted prevailing liberal beliefs. Bonn soundly resisted domestic pressure from the East for policy change. In stark contrast, domestic actors from the new *Länder* successfully pushed for radical policy departure that would garner for themselves more EU aid, subsidies, and development funds. Successful policy change rested on an institutional structure in these sectors that permitted eastern domestic interests to be "partners" in the policymaking process. And the ideas supporting previous policy could be stretched enough to incorporate and justify the policy shift.

More nuanced changes occurred when both German federal and EU institutions favored the demands of German domestic interests, even when these demands appeared to fly in the face of prevailing ideas. These more subtle changes were evident in the environmental regulatory sector. Strict national regulations, exporters had always argued, hampered Germany's competitive advantage. Before unification Germany ignored this objection had led the effort to strengthen EU environmental regulation as German domestic regulations stiffened. Ideas about environmental protection prevailed over interests. But when German export competitiveness sharply declined after unification, German officials were less eager to see increasingly strict environmental policy and attempted to slow the EU process as the German process slowed.

In the energy sector, liberal ideas had favored deregulation, but hard coal interests and postwar decisions to provide a sure and cheap energy supply made Bonn drag its feet on deregulation. After unification, the cost-benefit calculations changed: Energy-intensive industries, favoring gas and oil over coal, were always eager to deregulate. Now their lobbying efforts paid off, especially in Brussels, where the European Commission also favored rapid deregulation; Bonn, hampered by hard coal interests and institutional constraints, favored a slower approach. But the economic crisis triggered by unification provided the impetus to speed it up. A coalition between German exporters and the European Commission provided the needed push to accelerate energy deregulation at the European level. That push finally gave Bonn the upper hand over coal interests and brought more rapid energy deregulation in line with liberal ideas. In these two issue areas, it was the speed of European policy change that was affected, rather than Germany's overall policy stance. Prevailing ideas about environmental protection and liberalization remained intact.

This convincing argument makes an important contribution to the scholarly debate over the sources of states' foreign policies. Some analysts behind that national policy changes result from external pressures—growing or declining power in the international system or the increasing role of international institutions in shaping states' preferences. Others look to domestic forces in the policy process as the primary cause of change. Anderson makes a convincing case for domestic explanations of policy preference. Yet, he rejects mono-causal accounts, and his careful analysis shows precisely how ideas, institutions, and interests—at both the domestic and international levels—interact to influence policy choice. Unlike arguments that bundle ideas and institutions together in their explanations, Anderson gives institutions independent explanatory status, apart from the ideas that gave birth to them.

Will this argument stand the test of time as unification pressures recede? Germany is increasingly less likely to face the same mix of domestic pressures in the future, and Anderson's argument relies heavily on unification and its domestic consequences as a cause of change. Nonetheless, his analytic framework will continue to be useful under other conditions that affect one or more of the three variables.

Can the lessons learned from these seven cases be generalized? Indeed, the case of dramatic postunification changes in German export control policy seems to defy Anderson's account. Before unification, Germany's regulations controlling the export of commercial technology with military usefulness to potential adversaries were liberal, lax, and minimal, and the Germans consistently opposed the stricter control policies of European partners and the United States. The logic of export regulation was in direct conflict with Bonn's liberal trade ideas, and tight policy networks between government and in-

dustry in favor of loose controls ensured lax enforcement and minimal regulation. Institutional forces favored exporters' interests; indeed, the major industrial association virtually wrote Germany's export control regulations. Clearly, ideas, interests, and institutions combined to ensure minimal export control in the face of strong external pressures.

After 1989, however, despite heated protests from industry, Germany's export control system underwent its most dramatic transformation in history. It moved from a reluctant controller to a fervent proponent of tight controls, and its laws were among the strictest in the world. Within the EU, Germany successfully pushed for a common export control policy, and initial EU guidelines mirrored the new German legislation. Institutional changes in Germany favored tighter controls and stricter enforcement. Neither ideas, nor institutions, nor interests can explain this shift; their interaction had long worked to ensure a liberal export control policy. And exporter interests in lax controls only grew stronger after unification as competitiveness declined. Perhaps in this case, analysts must look to external pressure associated with growing German power and growing responsibility for regional security.

This objection, however, does not undermine the significance of this important book. Anderson has gone far in strengthening both the argument for domestic explanations of foreign policy preferences and arguments about the domestic sources of EU policy. Scholars will benefit greatly from the use of his analytic framework, and the argument will long continue to pose a significant challenge to those who claim that Germany's foreign policy will now be shaped primarily by external forces and its growing power on the European and international scene.

Stay the Hand of Vengeance: The Politics of War Crimes Tribunals. By Gary Jonathan Bass. Princeton, NJ: Princeton University Press, 2000. 402p. \$29.95.

Arthur W. Blaser, *Chapman University*

This excellent book is a worthwhile acquisition for anyone and any library, but it is an essential one for those concerned with international law, international organization, and war crimes. Bass combines the best of his scholarly political science training with his experience as a former correspondent with *The Economist*.

Bass offers comparative case studies of war crimes trials, including successes and failures. There are separate chapters on the aftermath of the Napoleonic wars, the trials at Leipzig following World War I, the trials at Constantinople after the massacre of Armenians, Nuremberg, and the Hague Tribunal on the former Yugoslavia. Along the way Bass makes points about other trials, such as the Rwanda tribunal and the Tokyo tribunals after World War II. These chapters are sandwiched between first and last chapters that offer propositions and arguments about war crimes trials.

This is a contribution to international relations that acknowledges extensive reliance on a work in political theory, Judith Shklar's *Legalism* (1986). As Shklar argues about Nuremberg, Bass claims that legalism, or "due process across borders" (p. 20), makes a difference. It has not made for perfect solutions, but it is preferable to the alternative, vengeance. It is the "least awful alternative" (p. 304). Furthermore, Bass's argument is important with respect to three related divisions in the study of world politics. His words will not be the last about these divisions, but they are eloquent and well supported by evidence.

First is the idealist-realist distinction. Bass belongs to the former group, although he is careful to point out that legalism

is a product of liberalism, not democracy. Indeed, it may work at its best when it operates to limit democratic decisions. Bass also is careful not to claim too much for the tribunals; he is anything but a utopian. Even the great success at Nuremberg, he reminds us, was achieved at great odds and was not unblemished. His work reminds me of psychologist William James's distinction in *Pragmatism* (1907) between the "Tough-Minded" and "Tender-Minded." When Bass offers countering opinions to those of Henry Kissinger in *A World Restored* (1957), for example, he is careful to provide plenty of evidence. For this section it comes from extensive documentary research; in contemporary cases, such as the chapter on the Hague (Yugoslavia) tribunal, Bass includes interviews with many of the principals.

Bass argues that tribunals may offer punishment that vengeance cannot. He describes the legalist rationale: "For public attitudes to shift, criminal leaders must be tried—their aura of mystery shattered by showing their weaknesses and stupidities" (p. 288).

Although I suggested in the first paragraph that this book is essential reading for those interested in international law, Bass specifically states this is one of three subjects he does not cover. The other two are domestic transitions to democracy and international institutions (p. 34). His concern is international war crimes proceedings. Although Bass does not get mired in abstract discussion of how states ought to behave according to international lawyers, he does describe how they have behaved, and how rules can and have affected this.

The second distinction is between traditional and descriptive approaches to international relations as opposed to more scientific and quantitative ones. Bass's work exemplifies the strength of the former approaches. The book is comparative history at its best and is very readable. There is some conceptual discussion (useful terminology to some readers, needless jargon to others) in the footnotes at the end of chapters.

Bass's argument is easy to follow owing to effective use of headings and subheadings. Figures or tables would have been useful; I found myself constructing some to clarify the attributes of some tribunals but not others. Bass acknowledges that he does not offer precise typologies of liberal and illiberal states and that there is great variety within the categories he writes about, but he points out that for his case studies a simple dichotomy is sufficient.

Finally there is the distinction, about which Bass convincingly argues that many analysts have been much too rigid, between international relations before and after World War II. He points out that the phrase "crimes against humanity" was used by Soviet Foreign Minister Sergei Sazonov to describe the massacre of Armenians and by others to describe conditions during the 1910s.

Most of my quibbles concern the book that might have been written. Bass could have said more about Rwanda and the Tokyo trials, particularly about Justice Pal's lengthy dissent. (Bass easily discredits self-serving Soviet statements at Nuremberg; Pal's Tokyo dissent would have made interesting fodder for refutation.) Although Bass acknowledges that liberal states have shown lots of self-interest, he convincingly argues that they are the only ones to have created war crimes tribunals. They also have a record of human rights atrocity, including slaughter of indigenous populations, slavery, forced relocation, and investment strategies that induce dependency (in many accounts linked to war crimes). Bass argues the merits of a universalistic worldview (p. 23); critics of Western states' policies charge that such a worldview has brought disastrous consequences to those who do not fit it. Bass comments on the new International Criminal Court (ICC) but could have offered more. His thoughts include: "If there

is to be, despite American objections, a serious permanent war crimes tribunal—the ICC—then liberal governments will have to make a far stronger commitment to international justice than they have in the 1990s" (p. 282).

By offering comparative case studies, this book fills a void. Other recent works are comparable in some ways but not others. Steven Ratner and Jason Abrams (*Accountability for Human Rights Atrocities in International Law: Beyond the Nuremberg Legacy* (2d ed., 2001), provide many useful points of comparison but do not offer the historical breadth of Bass's book. That is important because, as Bass reminds us, "the Leipzig war trials were a disaster" (p. 80). As he observes, it will be to our peril if we remember Nuremberg but forget about Leipzig and other precedents in designing policies toward war crimes in the former Yugoslavia and elsewhere. By placing the Nuremberg tribunal in the context of others, Bass allows for more accurate appreciation of that tribunal. The book contains telling anecdotes about U.S. foreign policymakers (including Henry Stimson, Madeline Albright, and William Cohen). It is suitable for course use and is accessible to upper division undergraduates and graduate students.

Cambodia Confounds the Peacemakers, 1979–1998. By Macalister Brown and Joseph J. Zasloff. Ithaca, NY: Cornell University Press, 1998. 272p. \$39.95.

David W. Roberts, *University of Ulster*

This book takes the reader through the complexities of the search for peace in the first chapters and through the provision of the UNTAC operation up to 1993. It concludes with an overview of the 1998 elections, and in between there is a section dedicated to the period between the two polls. The first section is reasonable, but the text is repetitive and it repeats what several others have already written (Trevor Findlay, *Cambodia: The Legacy and Lessons of UNTAC*, 1995, and various works by David P. Chandler, Ben Kiernan, and Michael Vickery). The analysis reflects that by several of the recognized Khmer scholars, and is useful to read, but while the analysis is insightful regarding Western peacemaking processes and the Khmer resistance at various points, it does not question assumptions regarding the overwriting of Western peace paradigms onto Khmer political culture. This has a distinctly different historical evolution and an equally dissimilar set of contemporary values regarding the notions of opposition, power sharing, and social harmony. The limits of this type of analysis are present throughout the work and are probably best illustrated by the statement that the violence of July 1997 "transformed UNTAC's enterprise from what may have been termed a 'limited success' to a failed rescue of a failed state" (p. 265).

First, Cambodia was not, in 1991, a failed state. I found that most of its primary organs worked reasonably well, other things being equal. There was no anarchy, no brutal police state, and no war by October. Trade was conducted at all levels, fuel was available for commerce, and capital city hospitals were functioning at varying levels. Second, shortcomings in the operation were not UNTAC's fault, but the fault of the Paris Peace Agreement. This vessel carried peace not for Khmers in the first instance, but for the Perm-5's concerns, specifically the United States and China, regarding their relations with Viet Nam. It was not UNTAC's fault that they could not conduct an unfeasible (in practice) mandate. More importantly, the statement reflects a simplistic analysis of the peace process in and above Cambodia and seems to reflect a notion of Western superiority and the poverty of Khmer politics. This is quite noticeable when the writers discuss how a

Japanese proposal, with European backing and support, provided for Prince Ranariddh to reenter Cambodia for the 1998 election after he had run away prior to the July 1997 fighting. They state that "once again, members of the international community helped provide a pathway to open Cambodia's deadlocked politics" (p. 303). But it equally can be argued that there was no domestic deadlock. A new prime minister had been appointed, the Assembly continued in its deliberations, and stability ensued in Phnom Penh, where it had been absent before. Some might view the writers' perspectives as patronizing.

These perspectives are also reflected in the title of the work. It seems that Cambodia has "confounded" the "peacemakers." That is, the West, irreproachable for all its valiant efforts at peacemaking, has been undermined by the efforts primarily, if the book is to be believed, of an arch-strongman, Prime Minister Hun Sen. It must be considered that the virtuous West ignored Cambodia in its darkest years and then created a peace framework ordained to ensure the inclusion of the Khmer Rouge in a political settlement.

That framework also rejected domestic peace overtures, such as those between Sihanouk and Hun Sen in 1987 and 1989, which would have dispensed with the need for foreign intervention. One senses a certain set of values that that are "offended" by Cambodian actions that undermine a Western "rescue," when in fact the Khmer elite were prevented from undertaking their own peace plans. The man most responsible for either steering Cambodian decision making or resisting Western planning is clearly Mr. Hun Sen, and he is the target of uncritical and partisan analysis. There is a strong sense that Hun Sen has been responsible, mainly, for the demise of the West's valiant attempts to "save" a nation. This is strongly evident in the section on the July 1997 "coup." This term is repeated frequently throughout the book in an uncritical manner that does not even consider the possibility that Hun Sen was forced to defend himself against Ranariddh's illegal practices or, indeed, that any other explanations exist. Since July 1997, a raft of evidence has been provided, some by the Khmer Rouge, that Ranariddh was involved in private deals with the former guerrillas aimed at militarily changing the balance of power against his coalition partner.

The treatment of sources is also problematic in this context. For example, Brown and Zasloff subtly challenge the veracity of Ranariddh's smuggling of private weapons disguised as machine parts by stating that Hun Sen "accused" Ranariddh of the above crime (p. 262). Similarly, in the Epilogue, the authors conceal through omission that Ranariddh was proven guilty of smuggling weapons, etc. They write that Hun Sen "contended" that Ranariddh had been guilty, and that Hun "postured" various things (p. 302). In fact, Ranariddh confessed to the act, and the papers were full of the story at the time. Given this, and given the authors' considerable use of local papers from Phnom Penh, this has been curiously avoided or neglected.

Indeed, the book leaves the impression that Hun Sen is the bogeyman of Cambodian politics. Again, such analysis is uncritical. Identifying him as authoritarian is right in some respects, but most scholars of this subject, including those who are diametrically opposed on many issues, agree that both Ranariddh and Rainsy are equally authoritarian in nature; however, this is not addressed. Indeed, Rainsy seems to have grown a halo in some parts of the book, most notably in Chapter 8, which reviews the period 1993–1997. The authors do, however, redeem themselves in recognizing Rainsy's virulent racism against Viet Namese people during and after the 1998 elections. Indeed. This section (Epilogue) contains much to commend it. It offers a clear review of the polling and, because the authors were present, recreates the atmosphere

of the time, reminding even of the 1993 election. It also avoids the tedious accusations that the election was critically rigged by the Cambodian People's Party (CPP) and neutrally considers the arguments concerning the role of the CPP-biased National Election Commission in the preelection period. But once again, omission is evident. Concerning the period after the July fighting, the authors note "extra judicial killings" and then lend legitimacy and credibility to that statistic by attributing it to the U.N. Special Representative for Human Rights (no less). But there is no mention of the fact that four of those "dead" miraculously reappeared alive, undermining the credibility of the Human Rights body and embarrassing the Special Representative. Equally, no mention is made that some of the killings were undoubtedly score settling and turf wars, not extra-judicial political acts blamable on the CPP elite.

Clearly much effort was involved in the writing of this book, and the authors have studied the politics closely and visited the country. However, their analysis is often weak and evidently biased against a key political actor, undermining the neutrality and credibility of the book. The work also does not criticize the peace plan for propriety in the context of Khmer conflict management culture and does not discuss whether the shortcomings in the peace plan of a cultural nature led to more, but different, conflict within Cambodia. As a U.S. Ambassador once noted, "In 1991, we settled the Cambodian war. In 1997, the Cambodians settled the Cambodian war." Read with caution.

Sanctions as Economic Statecraft: Theory and Practice.

Edited by Steve Chan and A. Cooper Drury. New York: St Martin's, 2000. 258p. \$75.00.

Kim Richard Nossal, *McMaster University*

Edited collections usually have relatively clear provenance. Most often they are brought to life by colleagues who are intrigued by an issue or a question; they then organize a conference (or a collection) around that theme and seek a publisher for the resulting collection. Such works are commonly marked by the putative expectations associated with an edited book: There is an attempt by the editors to present the unifying theme desired (if not demanded) by most university presses and the academic reviewers whose reports determine which way the thumbs go. Usually the editors attempt to herd the cats they have invited to participate—and ask that the contributors address unifying themes or take desired approaches. Even if the cats do no more than tip a ritual bow in the direction of the unifying theme, the editors normally use the introduction and conclusion to craft at least the appearance of unification.

This collection of essays on international sanctions, published in the widely respected Macmillan series on international political economy, has none of these signposts to its origins. The editors provide no preface that might give the reader some sense of where the collection came from—or why. There is no rationale offered for the topics included or excluded. There is no attempt to provide a unifying theme or puzzle, and there certainly is no central argument. To be sure, there is an introduction, but it bills itself as an "overview" and lives up to that title, for it is no more than a brief outline of what is to come. The nine chapters that follow clearly indicate that their authors have had no herding. The only commonality among them is that they focus on international sanctions. In short, this is a collection that just is.

Taken for what it is—nine essays on sanctions between ridiculously expensive Macmillan/St. Martin's cloth covers—the book provides a useful and intellectually stimulating

reflection on this tool of statecraft. First, several essays employ methods that depart from the common approaches in the international sanctions literature: A. Cooper Drury uses statistical analysis of U.S. presidential decisions on sanctioning; William H. Kaempfer and Anton D. Lowenberg prefer public choice; Valerie Schwebach uses formal modeling; Daniel W. Drezner uses Boolean analysis to explore Russian economic statecraft.

Second, this collection offers a number of analytically rich case studies that will appeal to traditionalists who might be less than taken with the results of the above approaches. Jason Davidson and George Shambaugh focus on incentives as a tool of economic statecraft, a theme picked up by Curtis H. Martin, who examines the all-too-often ignored case of the 1994 agreement between the United States and North Korea as an example of the differences between sanctions and inducements as instruments of foreign policy. Daniel W. Fisk's chapter on the American sanctions against Cuba does an excellent job of locating the Helms-Burton sanctions of the mid-1990s in a broader political perspective, looking beyond the usual arguments about the domestic roots of American efforts to sanction Cuba. Steve Chan chronicles the seemingly endless debate in the United States over whether China should receive most-favored-nation (MFN) status—or “normal trade relations,” as some Americans have unilaterally decided this centuries-old European convention should now be called. Of course, the debate has continued long after MFN status was granted, although it now focuses on whether MFN status should be withdrawn over such matters as the Chinese government's crackdown on the Falun Gong movement or Beijing's initial responses to the air collision near Hainan. David M. Rowe revisits the Rhodesia sanctions of the 1960s and 1970s and examines the domestic political effects of the boycott on Rhodesia's important tobacco industry.

This collection features an interesting mix of methods and case studies, but there are some significant holes in the coverage of contemporary sanctions. For example, there is no discussion of sanctions against South Africa in the 1980s or against Iraq since 1990, even though those cases remain crucial episodes. There is no exploration of the various ways in which the international community has sought during the post-Cold War era to sharpen and smarten sanctions. (Perhaps that is not surprising, since the book tends to limit its focus to the national level, rather than explore sanctions from a multilateral perspective.) Perhaps most important, there is no extended discussion of the normative implications of sanctions, an issue that has increasingly engaged both policymakers and academics over the last decade.

International Relations—Still an American Social Science?

Toward Diversity in International Thought. Edited by Robert M. A. Crawford and Darryl S. L. Jarvis. Albany: State University of New York Press, 2001. 394p. \$88.50 cloth, \$29.95 paper.

Timothy Dunne, *University of Wales, Aberystwyth*

Early accounts of the development of the discipline of international relations (IR) attribute causal significance to changes in the “real” world. In this respect, historiography was a reflection in history's looking glass, such that World War I created idealism, and World War II prompted the revival of realism. The editors of *International Relations—Still and American Social Science?* remind us that the identity of the discipline is also a reflection of geopolitical and cultural circumstances. The sixteen essays seek to reawaken the question of the identity of the discipline and how this has been transmitted and contested. There is no doubt that the book

will be widely read and is likely to find its way onto many postgraduate course lists. It is also likely to find critics and supporters in fairly equal number, which is reason alone to applaud the labors of the editors.

Stanley Hoffmann once referred to the United States as the “heartland” of IR. Given the importance of his essay, “International Relations: An American Social Science,” to the forging of the dominant self-image of the discipline, the editors were right to publish it as the lead contribution to the volume. Many readers will welcome the opportunity to read once again Hoffmann's finely crafted argument and fluent prose, which qualities are less evident in many of the subsequent contributions.

Two main themes underpin the book. The first is the extent to which IR continues to be bound to the cultural and economic interests of the United States. The second, dealt with below, is whether the recent proliferation of approaches enhances the standing of the field. Regarding the former, there is broad agreement among the contributors that America continues to dominate the study of IR. I use the term “broad” because the editors are unwilling to furnish a compelling criterion for what counts as dominance. As Robert Crawford writes in the Introduction, “this volume offers no consensus on how ‘American intellectual hegemony’ should be conceptualized, measured, and defined, nor do our contributors agree on the degree to which nationality in any guise ultimately matters” (p. 6). Kim Richard Nossal's chapter is not so reticent. His survey of fourteen IR textbooks shows “how an image of world politics is constructed and thus world politics understood, and how this is reproduced from one generation to the next in a hermetically sealed Americocentric vacuum” (p. 183). The argument would be much stronger if Nossal had demonstrated that textbooks produced in other national contexts are not guilty of ethnocentrism.

Concerns about methods resurface in A. J. R. Groom and Peter Mandaville's essay. To sustain their argument that there is an emerging “continental” disciplinary identity, the authors conducted a survey that they describe as “dirty and very unprofessional” (p. 159). They should be applauded for being so candid about the limits of their method, but this surely provides grist to the mill of those who believe that much of what passes for scholarship outside the United States lacks rigor and precision. More substantively, there is no doubt that Groom and Mandaville are right to argue that IR in Europe is gaining in strength, but does this generate a European perspective on the international?

This relationship between national (or regional) community and world view is hotly debated elsewhere in the volume. The case of Canada is instructive here. Tony Porter contends that nationality is only one variable influencing how IR is taught and researched. Mark Neufeld and Teresa Healy support this contention through their engagement with “minority currents” in Canadian IR. Exponents of these critical perspectives share the following two propositions. The first is a disinterest in policy advocacy (which is not meant to imply a lack of concern for concrete political practice), and the second is a commitment to critical pedagogy. Here we read that realism and idealism offer only two versions of the same dominant discourse. This is contrasted with “Critical IR,” which apparently succeeds in connecting with students who are concerned with issues of social justice. These observations are interesting but beg many questions, such as how this teaching strategy works with students (a majority?) who would rather work for Microsoft than join the brigade of anticapitalist protestors. At a more specific level, it would have been useful if Neufeld and Healy had told us more about the details of their pedagogy. For example, does it work best with case study teaching? Should reading be directed, or is

it better to allow students to roam more freely into other disciplines and explore other mediums?

Roger Epp's chapter shows us that critical theory is more convincing when the authors "just do it" rather than talk endlessly about its virtues (while parodying the positions of so-called uncritical theory). Epp's interest is in indigenous peoples and what their neglect tells us about IR. The one theoretical approach that has engaged with them is the English School, even though its narrative of the expansion of international society has relied on "a domestication of aboriginal peoples" (p. 312). Even a rewriting of such narratives to include aboriginal diplomacy risks seeing aboriginal cultures only insofar as they were recognized by European sovereign states. Epp concludes his essay with a warning that pluralism is limited unless there is a genuine commitment not just to represent but also to listen to the other.

Feminist thinkers have long made this argument. In her chapter on transcending national identity, Jan Jindy Pettman reminds us of the gender inequalities built into both academic IR and global political practices. As a critique, this is powerful stuff. It is less convincing in the claim that there is a transnational community of women based on the "near-universal ways in which so many women experience identity conflicts and different kinds of violence" (p. 261). In support of this claim she quotes Virginia Woolf's famous contention that, "as a woman, I have no country. As a woman I want no country. As a woman my country is the whole world" (p. 261). Only a few years after Woolf wrote these words, women showed that they did want their country and made significant sacrifices for its survival. Pettman is aware of the importance of location and the fact that there are many tensions within the world of women along race and class lines. But this raises the question of how much diversity the feminist project can allow while maintaining its commitment to the emancipation of women as a transnational identity based on shared values and experiences.

D. S. L. Jarvis's chapter on identity politics has a great deal to say about feminism. It is highly critical of standpoint feminists for privileging the gender variable over other identities. Postmodern feminists are dismissed for retreating from the main IR agenda. The main contention is that identity tribalism is a blind alley: We are different in many important ways, Jarvis argues, but what matters is what we share, such as a repulsion against violence and poverty. Jarvis's answer to the second thematic question underpinning the book is that the recent proliferation of approaches does not constitute theoretical progress.

More is not better for Kalvi J. Holsti either. He mobilizes a polemical critique of those who inhabit what he calls Identityville—the "postmodernists, poststructuralists, critical theorists, feminist standpointers of unlimited hues, hermeneuticians, empiricists, historical sociologists, symbolic interactionists, and post-Marxists, just to name some of the more prominent" (pp. 81–2). Interestingly, one of Holsti's criticisms of the inhabitants of Identityville is their lack of tolerance and respect for other approaches. There is no evidence that these qualities are present in Holsti's reading of critical theory. Traditionalists in glass houses should not throw stones.

Those who fear theoretical diversity might, ironically, gain some sustenance from Chris Brown's argument. In an engaging chapter, Brown argues that the discipline was founded upon Anglo-American liberal principles, and "the modern discipline has not yet escaped from its liberal internationalist past" (p. 217). In other words, the gap between traditionalists and postmodernists may not be as wide as either side would like to think.

A telling critique of the framing of *International Relations: Still an American Social Science?* is that all the authors are

located in institutions of higher education in Western nations. To show convincingly that there is diversity in international thought, the book should have included a greater focus on the research and teaching of IR in Africa, Asia, Latin America, and so on. There is diversity out there, but not enough of it is represented in this otherwise lively collection of essays on the state of the discipline.

State Learning and International Change. Andrew Farkas. Ann Arbor: University of Michigan Press, 1998. 208p. \$50.00.

Erik Gartzke, *Columbia University*

Learning would seem to be a natural topic for the academic study of international politics. Interest in dynamic processes, however, and in learning, in particular, has been uneven, while the impact of contributions to date has been mixed. *State Learning and International Change* provides a thorough, thoughtful application of a complex biological learning model to international politics. The text is engaging and well written. Modeling and philosophy of science issues are addressed with aplomb and humor. For example, the author refers to the residue from social Darwinist applications of biological models to social science as "pesky," discusses stochasticity and pleiotropy with reference to striped and spotted cross-eyed bandersnatches, and mentions "deranged mutant-killer-monster snow goons" as a possible method of biological selection, all in the space of a few pages (pp. 59–67).

The core of the text is an evolutionary learning model, intended to "explain how states arrive at foreign policy decisions" (p. 1). In the model, agents (policy elites) are initially randomly assigned policy positions. Policy evolves as the weighted average of the recommendations of policy elites succeeds or fails and the weighting of recommendations is adjusted in response to performance. A later version of the model allows elites to shift their own recommendations in response to success or failure. Propositions derived from the model include the insight that extreme policy positions may improve policy (p. 101) and the finding that multiple advocacy can be counterproductive (p. 149). These propositions are in tension, however, if including extreme positions involves more policy advocates.

Evolutionary metaphors are often used to justify key "as if" assumptions of rational theory. Noting that selection generates "fitness," the quality of being successful by appropriate evolutionary criteria, Farkas is able to show that even automaton can approximate optimizing behavior. This is an appealing finding, and one that emphasizes complementarity between the rational and the biological approaches. Unfortunately, Farkas presents the evolutionary model as an alternative to rationality.

Farkas appears to criticize a particular kind of rationality, ignoring opportunities to address his concerns, while adopting practices or attributes that parallel rationalist approaches or are equally subject to criticism. For example, Farkas challenges the plausibility of assumptions used in rational choice models but assumes that agents in his game all have heterogeneous preferences over goals. While defensible on tractability grounds, it is clearly not true (Stalin and Trotsky had different goals) and it alludes to the fundamentally problematic nature of arguing over assumptions. More to the point, policy elites in the evolutionary model are egoistic and purposive, seeking to enhance their influence over policy outcomes by adopting recommendations that they believe will be successful. The critical differences, then, between the evolutionary approach and an explicitly rational theory are that elites are initially randomly assigned policies rather than being able to choose

policies, that elites update recommendations based solely on past performance, and that selection is exogenous.

There are more minor lapses in the text that perpetuate common errors. For example, in the introduction Farkas asks how people "who individually do not act as if they were rational" (p. 2) can behave as such collectively. Rationality, of course, is not what Farkas is addressing. Since rationality is judged by conformity to a postulated set of objectives (derived from a preference ordering and constraints), we can say that a particular formulation of rationality is wrong, but not typically that people are not rational. Prospect theory, for example, offers a different utility function, not an assertion that individuals do not have preferences or that they fail to seek preferred outcomes.

A more important set of issues has to do with evolution and politics. As the text points out, "The evolutionary model of state learning . . . depends on an analogy between beliefs and genes and policy and positions and phenotypes" (p. 62). At the core of the evolutionary model is the selection mechanism that adjusts the influence of policy elites. In biological systems, selection may be an exogenous process, but in politics seeking to manipulate the criteria by which one succeeds or fails is an important part of the game. Whether selection occurs in the consciousness of the leader (where contrary to claims in the text it would involve complex calculations), or in the institutions and norms of the state, an agent that can influence the way weights are assigned is evolutionarily advantaged. Since this requires cognitive skills (one must anticipate events and the actions of other agents), it is evolutionarily desirable in politics not to be an automaton. The assertion that the assumption of rationality is overly restrictive then appears somewhat more restrictive than allowing actors to think.

Another concern with, or potential expansion of, the model is that it assumes that objectives do not change. Policy evolves in the learning model to respond better to existing policy concerns. This is problematic if, for example, one is dealing with nuclear weapons. The ability to think and act strategically may be superior if learning from experience is dangerous or costly. Similarly, whether policy elites view the world through the insights of predecessors depends on whether these insights continue to be germane. If relevant processes remain substantially the same over time, then decision making based on inherited heuristics is sound. If, instead, environments change rapidly (as apparently occurs in modernity), then the value of inherited heuristics declines and individuals have an incentive to depend on other sources (peers, their intellect) for insight. This implies that the environments that are most ripe for the evolutionary perspective are those that do not change much or often.

In natural or sexual selection, as the rate or intensity of change increases, individuals or groups with traits that bias toward survival or desirability will increasingly predominate. However, in politics, the rapidity or intensity of change affects the *salience* of the evolutionary metaphor. As the rate or intensity of change increases, individuals or groups that depend on inherited knowledge will tend to do less well than those who reason. If allowed to exist, rational agents will predominate. After all, human cognitive abilities appear out of natural selection. Thus, the irony is that it would be at odds with the evolutionary metaphor to advocate the current evolutionary model of learning. In systems where knowledge is likely to be time-bound, adopting inherited heuristics biases against selection. The approach with greater fitness is to avoid heuristics and adopt rational choice.

The text emphasizes that the validity of a model depends on how much it explains. "Careful readers who are concerned with the misapplication of biological models would do well to ask how accurately a model captures the behavior it purports

to represent and whether the predictions made by the model are supported by the data available to test it" (p. 67). The text offers many anecdotes, but there is no systematic assessment in the context of international relations. Further, there is substantial overlap between the evolutionary approach and existing rationalist explanations for international behavior. "The evolutionary model of learning produces outcomes that are identical—or nearly so—with those predicted by rational-choice models" (p. 76). I am willing to believe that the model has empirical leverage, but let me suggest at least one framework for a more general test.

Why not assess implications of the evolutionary approach for government institutions, rules, SOPs, etc.? There is little discussion of institutional and informal constraints on decision making in the text. This is striking because rules and other constraints are well suited to the propagation of knowledge. It is clear why incumbents seek to impose their will on future decision makers through institutional constraints. What is less immediately obvious is why subsequent decision makers would defer to such constraints. Rules carry knowledge of the relationship between policies and outcomes and so impact decision making. The evolutionary perspective suggests why and how rules develop and when they are maintained. Further, these are easily observable properties of states, so that tests of implications of the evolutionary approach should be manageable. Finally, this is an interesting aspect of politics that rationalist theories seek to explain, but not without some difficulty.

One learns from *State Learning and International Change* that there is much still to learn about learning in international politics. Farkas forces readers to examine conventional assumptions about foreign policy decision making. Rationality is not necessary to generate behavior that is consistent with the claims of rational models, nor need one adopt rational theory to engage in interesting, informative research. However, while contrasting rational and evolutionary approaches is of considerable value, of even greater value would be an exploration of the boundaries between, and synthesis of, rational and evolutionary models. Farkas offers a promising glimpse of the value of an exploration of learning in international politics and a hint at what the synthesis between these two perspectives can achieve. This alone should encourage readers to have a look.

Public Attitudes Toward Immigration in the United States, France, and Germany. By Joel S. Fetzer. Cambridge: Cambridge University Press, 2000. 272p. \$54.95 cloth, \$19.95 paper.

Gallya Lahav, *State University of New York at Stony Brook*

Joel Fetzer is to be congratulated for a serious attempt to bring a public opinion approach to comparative immigration politics. His book represents an ambitious step toward bridging the gap between policy input and output in the immigration equation of advanced industrialized democracies. Its occasional choppy organization and underdeveloped data analysis tend to distract from the import of the work and leave the reader yearning for a deeper and more substantive discussion.

Although the title promises to tell us about public attitudes toward immigrants and immigration, the focus is on *negative* attitudes, or what Fetzer calls "nativism," an American term that can be equated with xenophobia in the European context (see chap. 1, note 1). Fetzer never goes beyond a broad definition of the term, but he is careful to isolate the complex renditions of this dependent variable along three dimensions: immigration affect, policy preferences, and support for

anti-immigrant movements. The author's stab at compiling extensive comparative data is impressive, especially given serious constraints in obtaining cross-national data.

The exploration of causal attitudinal factors ventures into a major polemic in our understanding of tolerance of minorities and immigrants. What explains immigrant rejection and hostility—economics or culture? Attitudinal research has yielded contradictory results over ecological and contextual factors, and theoretical advances are inconclusive. Ironically, Fetzer rarely speaks directly to a whole host of budding scholarship in the immigration field, which has made significant headway on subjects he addresses. These include the rise and consolidation of the extreme Right parties, restrictive immigration policies, and immigration politics. To these studies, Fetzer's book uniquely adds rigorous empirical data testing that relies on theories exported from social psychology, economics, and cultural anthropology.

Fetzer works in "threes." He tests three theories in three separate nation-states employing three different modes of analysis: historical, time-series, and cross-sectional analyses. The first question to be asked is how the United States, a traditional country of permanent immigration, fits into a comparative scheme with such temporary labor countries as France and Germany. More might have been said about the comparative framework, the rationale of case selection, and the convergent findings.

The prevalence of each theory—marginality, economic self-interest, and contact—is tested against models of the three forms of nativism/xenophobia. Marginality theory, whether cultural, economic, religious, or gender-based, poses that people who are considered outside the dominant cultural template of a society are *ceteris paribus* more likely than their native counterparts to empathize with oppressed individuals (i.e., incoming immigrants). It suggests that marginalized groups will not be socialized into the dominant norms of the society, as Fetzer argues is the case with American Jews, who maintain a culture distinct from the dominant Protestant culture of the United States (p. 9).

Economic self-interest theory includes both the typical labor market variant and the use-of-service type. It may involve relative deprivation or sociotropic effects. In terms of immigration, the theory posits that individuals who believe their economic well-being is endangered by an influx of cheap labor will be more likely to feel an anti-immigrant effect than will people who believe they are secure in their job and income position. There is a tendency here to treat immigrants as a monolithic group and to overlook the threat that the rapidly growing and important skilled labor force poses to highly educated natives (p. 21).

The contact thesis (both individual and aggregate) involves the amount and type of exposure to immigrants. Much of this theory stems from the work of Gordon Allport (*The Nature of Prejudice*, 1979), who identified two main types of contact: "casual contact," which promotes prejudice, and "true acquaintance," which generates more tolerance. Fetzer attempts to conduct analysis on measures that he argues represent both forms of contact. The analysis seems to fall short of effectively measuring what would allow us to distinguish between casual and true acquaintance contact. To some degree this problem can be ignored, because all the measures used to test for the presence of contact theory are not systematically proven, but it does beg some questions about theoretical utility.

Fetzer concludes that a number of assumptions of both economic and marginality theories are upheld by the data. When they contradict each other, the marginality theory has a slight advantage, especially when it is formulated in cultural terms. These conclusions are rather clear, but the explana-

tory power of each theory alone is somewhat compromised by the failure to account for interaction effects. Many of the theoretical streams overlap (i.e., economic marginality and economic self-interest) or appear to be at loggerheads (i.e., sociotropic versus self-interest), which undermines their predictive value. One might take issue with these competing paradigms in favor of a more interactive approach, such as that used by Jeannette Money in her analysis of anti-immigrant sentiment at the local level (*Fences and Neighbors*, 1999). Her study reveals that the interactive part of job competition, for example, comes not from the mere presence of migrants but their presence when unemployment rises (which is what creates labor market competition). This perspective underscores the need to identify and delineate more systematically these contextual variables so as to predict conditions under which rejection explodes.

Fetzer is much more successful at isolating the causes of xenophobia than he is in his prescriptive trajectory. Although he builds a compelling case for policy-relevant attitudinal research, his policy prescriptions do not flow from the data. Fetzer never clearly states how public opinion relates to policy articulation and elaboration. He corroborates conventional postnational arguments made by such scholars as Yasemin Nuhoglu Soysal (*Limits of Citizenship*, 1994) and David Jacobson (*Rights across Borders*, 1996) about the narrowing of boundaries, but there is some tautology in his assessment that the lines between immigrants and dominant cultures fade. In fact, this claim seems to contradict his important finding that minorities continue to ally with their "cultural" kin.

The use of multilevel modes of qualitative and quantitative analysis is commendable. The longitudinal rationale and breakdown of timeframes are not necessarily obvious, however, and the historical analysis is often anecdotal. Fetzer runs into some typical problems with cross-national measures of foreign-born populations (see tables A1.1, 4), which vary with regard to geographic or citizenship criteria. The comparative operationalization of the dependent variable is also somewhat uneven. Intuitively, Fetzer captures an important distinction in the literature between immigration and immigrant policy preferences, but his measurement of these concepts often confounds the two. Does support for Le Pen or the *Front National* strictly capture anti-immigrant sentiment in France (p. 85), and is it really analogous to American attitudes toward immigration levels? Furthermore, is the right to asylum tantamount to immigrant rights, as Fetzer conceives (p. 111), or does it represent the admission-type of policy areas?

Fetzer's bold foray into the critical "micro-macro question" (p. 148) is a thrilling opening to those stuck in that scholarly divide. The discussion of the two components of Allport's contact theory is a thought-provoking and welcomed alternative to the linear thinking on immigrant effects. Ultimately the book fails to answer directly the important question of individual-aggregate discrepancies, because it is not certain that it truly tests for the latter form of contact and because it defers to the promise of "further research" (p. 81). To be fair, Fetzer does not make this question the pivot of his study. In fact, his goals are much more humble and modest.

If research contributions are evaluated by the extent of questions and further research they inspire, then Fetzer's book is a promise of success. Are the conclusions generalizable? Cultural marginality does not seem to explain why, in immigrant-importing countries of the Middle East, temporary labor is recruited specifically from Asia and other non-Semitic countries. What is the role of international relations as an intervening variable (p. 59)? What is the role of political elites and the media, and how do they relate to

mass attitudes? More generally, how do these attitudes relate to policy outcomes? My own comparative work on elite and public opinion has revealed far greater convergence than Fetzer (p. 149) and others project. As Fetzer readily concludes in explaining the German outlier (pp. 132, 149), leaders shape the immigration debate and therefore public reaction. If so, do public attitudes matter? My findings imply that they do, and, thus, Fetzer's work is illuminating.

By embracing the normative component, Fetzer helps fill a critical void in comparative immigration scholarship, and he makes a unique advance in the scientific inquiry into such an emotionally charged issue. Despite the disappointing array of evasive inferences, the book serves as an important prelude and invites more in-depth studies of attitudes. It will be widely cited.

Deterrence and Security in the 21st Century: China, Britain, France, and the Enduring Legacy of the Nuclear Revolution. By Avery Goldstein. Stanford, CA: Stanford University Press, 2000. 356p. \$49.50.

Lawrence Freedman, *King's College, London*

Clausewitz once observed that in war everything is simple, but the simplest things are very complicated. This seems to apply doubly so to nuclear deterrence. The principle is very simple: A potential enemy is persuaded not to do anything rash by the prospect of devastating retaliation. But it soon gets complicated. What difference does it make if the idea is not only to protect the homeland but also allies? As potential enemies acquire their own means of devastating retaliation, issues of preemption arise, and this requires close attention to the details of force structure. How varied, overwhelming, and surprising need the attacker be, especially if there is little interest in preemption? How much need the defender disperse, conceal, or protect forces, or develop antimissile defenses, just in case the other side is contemplating preemption? What happens if both sides are contemplating preemption at the same time? Can understandings, tacit or negotiated, between potential enemies ease the dangers of a crisis getting out of hand?

Questions such as these kept busy generations of nuclear strategists and led them to develop a substantial literature. Although this was largely concerned with the U.S.-Soviet relationship, the literature nonetheless was often abstract and theoretical, trying to work out what rational actors might do in hypothetical nuclear predicaments before worrying what the actual states involved might do. At the same time, because much appeared to depend on the hardness of concrete shelters or the accuracy of a missile warhead, this high theory had to be combined with some quite technical analyses of the properties of particular weapons. And then there was always the political dimension, concerned with the tenability of alliances and the dynamics of crisis management, followed by demands to come up with sensible proposals for force structure and arms control negotiations.

By such processes do simple problems become complicated. The deterrence industry grew even as the superpower relationship itself stabilized. The stability was the result of the practice being a lot simpler than the theory. Politicians understood the essential point. If you were going to risk nuclear war, you had to be really sure of your calculations. The consequences of getting it wrong were huge, and the inherent uncertainties were substantial; prudence was always the order of the day. This was especially so after the tense moments over Berlin and Cuba in the early 1960s.

The problem of deterrence for lesser nuclear powers appeared to be far less demanding. Robert McNamara, when

Secretary of Defense, claimed that small nuclear forces would be "dangerous, expensive, prone to obsolescence and lacking in credibility as a deterrent." Either he was right, in which case the disincentives to getting into the nuclear business would be profound, or he was wrong, in which case proliferation could be expected as more states concluded that at quite modest numbers a nuclear deterrent could be both survivable and useful.

Avery Goldstein, who takes the second of these views, considers the Cold War experience of China, Britain, and France and judges its relevance for the post-Cold War world. He examines not only the rationales for their acquisition of nuclear forces but also what they thought was necessary to achieve their deterrent purposes. He finds that they felt able to do this without attempting to match the exacting standards set by the United States and the Soviet Union, at least so long as they were limited in their ambitions and confined themselves to preserving the status quo. The trouble is that in developing this position goldstein demonstrates himself to be a complicator. There appears to be not a single issue of deterrence theory that he does not attempt to address, and he even includes his own little "expected utility model." The result is a dense read, with the more interesting material often tucked away in footnotes, and few general propositions are reached on the viability of alternative deterrence strategies that are not context-dependent.

Fortunately, Goldstein also seeks to develop his argument through case studies. His previous book was on China, and by far the best sections of this book are those that deal with the history of China's nuclear force. He is on top of all the best scholarship on Chinese foreign policy, from the entry into the Korean War to the Sino-Soviet dispute and the opening to the United States, and he has interesting things to say on why the force structure took the form it did. The case studies of France and Britain are less successful. Goldstein's neorealist position leads him to argue that the core motive behind acquisition was the lack of confidence in a superpower protector. That has some validity with France, as with China, but for Britain that is a much smaller part of the story. Of course the "standing alone" scenario was part of the rationale, but the basic impulse behind Britain's nuclear capability was, first, its initial status as one of the "big three" and indeed as the first country to demonstrate the possibility of a nuclear bomb and, second, its constant belief that a national nuclear capability provided an entrée into American policymaking.

Whether these three cases tell us much about the future is unclear. One might have thought the Israeli, Indian, and Pakistani cases, to which Goldstein devotes a few pages, are more relevant. As for future proliferation he points to the importance of the degree of insecurity afflicting individual states, the availability of nonnuclear means of addressing such insecurity as does exist, and the economic and technical capacity to develop the nuclear option. Goldstein's candidates are those that neorealists identified just after the end of the Cold War—Germany, Japan, and Ukraine—although in none of these cases is a military nuclear program on the immediate political agenda. The candidates that most bother American politicians—Iran, Iraq, and North Korea—are dealt with only in a cursory manner.

The underlying philosophy appears to be an expectation that the international system might one day conform to a neorealist model, that is, return to the great power politics of the old days, and then governments might be very grateful for any nuclear insurance that they had taken out. This indicates the basic problem with the title. Of course, the nuclear revolution remains important, and who knows what challenges might develop in the future, but to understand deterrence and security in the twenty-first century it is

necessary to make sense of ways in which the state system is already changing; new forms of insecurity are developing, and therefore new forms of deterrence are required. This book is welcome for the historical analyses of the smaller nuclear powers but, contrary to the title, has nothing to say about security and deterrence in the twenty-first century, other than to suggest that it may be like the twentieth century.

Unipolarity and the Middle East. By Birthe Hansen. New York: St. Martin's, 2000. 288p. \$59.95.

War, Institutions and Social Change in the Middle East.

Edited by Steven Heydemann. Berkeley: University of California Press, 2000. 372p. \$60.00 cloth, \$24.95 paper.

"Pariah States" and Sanctions in the Middle East: Iraq, Libya,

Sudan. By Tim Niblock. Boulder, CO: Lynne Rienner, 2001. 241p. \$49.95.

Robert O. Freedman, *Baltimore Hebrew University*

Scholars of Middle Eastern studies in the last decade often were preoccupied with two major problems. First, the democratization that has spread over most of the globe seems to have missed the Middle East. Second, there appears to be a growing gap between international relations and comparative politics theory, on the one hand, and Middle East studies, on the other. In seeking to explain why, some point to the highly politicized scholarship that can still be found in Middle East studies. Others argue that the theorists simply have not tried hard enough to fit the special nature of the Middle East into their theoretical models, or that Middle Eastern scholars have not tried hard enough to deal with theory. Two of the three books under review, by Hansen and Heydemann, do a great deal to narrow the gap between theory and reality in the Middle East. The book by Niblock is an example of the kind of highly politicized scholarship that is still found too often in Middle Eastern studies.

International relations theory meets the Middle East in Birthe Hansen's *Unipolarity and the Middle East*. In an admirable attempt to adapt Kenneth Waltz's theory of neorealism to Middle Eastern events in the 1990s, Hansen discusses a number of key developments during that period, including the Gulf War, the unification of Yemen, and the Israeli-Palestinian peace process, from the perspective of what is depicted as the "unipolar world" that emerged in the 1990s. Hansen offers some useful insights, but the problem, as with some of other international relations theorists who seek to deal with a region in which they do not have first-hand knowledge, is that a series of factual errors and questionable interpretations mar the rigor of analysis. Nonetheless, Hansen's discussion of unipolarity has strong theoretical merit and can be usefully refined by other scholars, including those with a greater knowledge of the region as well as international relations theorists, to reflect more closely Middle Eastern realities.

Essentially, Hansen argues that in the unipolar world that emerged in the 1990s the states of the region had to adapt or suffer the consequences. Syria, which did adapt, was able to reinforce its position in Lebanon by "flocking" (p. 65) to the side of the United States during the Gulf War. By contrast Iraq, which did not adapt, suffered a major defeat. Similarly, Hansen argues, Yemen unified because the northern and southern sections of the country could no longer benefit from the superpower "zero-sum game" competition for their support. Morocco was able to consolidate its control over the former Spanish Sahara because Algeria no longer had the Soviet Union to support it, whereas Morocco continued to have the support of the unipole, the United States.

A number of other interpretations of these events and others cited in the book are possible. To take but one example, it can be persuasively argued that the bloody civil war in Algeria, rather than the loss of Soviet support (which, in any case, diminished during the 1980s [p. 164]), was the primary reason Algeria acquiesced in Moroccan control of the former Spanish Sahara. Another questionable assertion is that the Cold War ended in September 1989, when Moscow made concessions on strategic arms to the United States (p. 78), rather than in January 1989, when the USSR withdrew from Afghanistan, or, as most observers contend, when the USSR acquiesced in both the fall of the Berlin Wall in November 1989 and the subsequent unification of Germany.

In addition to some questionable assertions, there are some factual errors. Syria intervened militarily in Lebanon for the first time in 1975, not 1976 (p. 141). Following the Hebron massacre in 1994, foreign observers were sent to Hebron, not Jericho (p. 169). There is also a misreading of Soviet support for Iraq during the Iran-Iraq war (after flip-flopping several times, Moscow tilted to the side of Iran in 1987) (p. 109) and a downgrading of the increasing Soviet opposition to the U.S. war effort against Iraq in January and February 1991. Hansen overestimates the influence of the Arab Cooperation Council in the 1989-90 period.

One also may question the theory of unipolarity itself. If the United States is indeed the unipole in the system, how does one explain Iraqi and Iranian opposition to the United States from 1991 to 2001, and the Palestinian rejection of U.S. mediation efforts in July 2000 (the Camp David II talks) and December 2000, during the Al-Aksa Intifadah? Perhaps it might be more useful, from a theoretical perspective, to call the 1990-2000 period one of limited unipolarity and to restructure the theory accordingly to explain the behavior of Iraq, Iran, and the Palestine Liberation Organization. Such a restructuring also would add more credence to the other case studies in the book. Despite its faults, the book is well worth reading because of Hansen's contribution to international relations theory.

Whereas Hansen asserts that the unipolar world led by the United States, with its emphasis on the spread of liberal democracy and free market capitalism, is basically a positive development, Tim Niblock describes the new world order established by the United States after the Gulf War as a "malign hegemony" (p. 6). A strong strain of anti-Americanism pervades his book, which purports to show "the view from the underside" (p. 7). Niblock asserts that this perspective "does not imply disregard for international law but rather a determination not to allow international law and international institutions (such as the UN) to be used to promote Western/U.S. interests" (p. 6). He cites numerous interviews in both Libya and Iraq in his footnotes, and he clearly sympathizes with both countries, which weakens the analytical quality of the book. In the case of Libya, he asserts that one effect of sanctions, corruption, reflects the more general commercialization of society that is clearly associated with wider global processes that have spread Western values (pp. 76-7), and he also blames sanctions for the spread of Islamism in Libya (p. 90).

If Niblock takes a rather gentle view of Kaddafi's policies in Libya, he is even more sympathetic to Iraqi leader Saddam Hussein. Time after time Niblock supports Iraq's resistance to UN efforts, from ascertaining the status of Iraq's holdings of weapons of mass destruction (WMD) to the UN distribution of food to the Iraqi population, as justified because of Iraq's sense that its sovereignty was being challenged. Niblock frequently simply restates Iraqi arguments without critically evaluating them, such as the statement that the UN WMD inspection efforts were "simply a cover for creating an imbalance of power in the region favorable to Israel" (p. 104).

In sum, if one is looking for an *apologia* for the Libyan and Iraqi regimes, and a strong denunciation of the U.S.-led sanctions against them, this is the book.

By contrast, the edited book by Steven Heydemann is an example of first-rate scholarship. Heydemann asked a group of scholars to examine the thesis of Charles Tilly and others in Tilly's edited book, *The Formation of National States in Western Europe* (1975), to see whether their perspectives on the role of war in the expansion of state capacities, the emergence of new patterns of human and economic mobilization, and the organization of extractive institutions could be usefully compared to the formation of states in the Middle East. There are a number of excellent chapters in Heydemann's book, many of which not only add a new theoretical perspective to the dynamics of statebuilding in the Middle East but also offer new information about the evolution of many states in the region.

One of the best chapters is by Heydemann and Robert Vitalis on the role of the Middle East supply center in Cairo during World War II. The story of the center has been told before, as Roger Owen notes in a very helpful concluding chapter, but Heydemann and Vitalis cover new ground by convincingly demonstrating how Egypt and Syria, once they became independent states, had a much greater role for the state in their economies than they otherwise might have, due to the effect of the supply center on the two countries during World War II.

The chapter by Reem Saad on the experience of the Egyptian peasants during the 1967 and 1973 wars, based on detailed interviews in a single village, provides some fascinating insights on peasant attitudes toward their government and leaders in time of war. Although loyal to the state, they were very angry at Nasser, hitherto a hero for his land distribution policies, whom they blamed for lying to them at the beginning of the conflict about Egypt's great "victory" (p. 242), although they felt he was misled by his advisers. Interestingly enough, there was little lasting emotional feeling about the 1973 war. Saad speculates that this may have been due to the exhaustive Egyptian state propaganda about the "victory" in the 1973 war.

Another very strong chapter deals with the formation of the militarized regime in Iraq under Saddam Hussein. Written by Isam al-Khafaji, it demonstrates how, through a combination of external war and internal terror, Saddam built loyalty among Sunni Muslims while marginalizing both Shia and Kurdish Iraqis (p. 282). Al-Khafaji also analyzes Saddam's use of "Arabism" as a device to secure loyalty not only from Sunni Iraqis but also from Arabs throughout the Arab world. According to Saddam, as their champion, Iraq deserved, indeed, was owed money by the Gulf Arabs (al-Khafaji calls this "strategic rent extraction," p. 273) to support its war against Iran (despite the fact that Saddam started the war by invading Iran).

A central theme in the Heydemann book is that unlike the states of Europe from the 17th to 19th century, which extracted taxes from their own populations to build up their military power and, in some cases, became more responsive to their populations as a result, a number of states in the Middle East avoided conflicts with their populations over taxes by becoming rentiers. This group included not only the oil-rich states of the Persian Gulf but also Egypt, Syria, and Iraq, which received military equipment from abroad either gratis or paid for by other Arab states. Needless to say, the end result was a very different relationship between rulers and ruled in the Middle East than in Europe. The Heydemann book goes a long way toward explaining why, in countries such as Syria, Egypt, and Iraq as well as in the Gulf states, genuine democracy has yet to develop.

Empire. By Michael Hardt and Antonio Negri. Cambridge, MA: Harvard University Press, 2000. 478p. \$36.95 cloth, \$18.95 paper.

Manfred B. Steger, *Illinois State University and University of Hawai'i—Manoa*

Hailed by sympathetic reviewers as the "*Communist Manifesto* for our time" and condemned by scathing critics as an "impenetrable work of absolute abstraction," *Empire* has exploded on an unsuspecting international academic scene. Indeed, the tremendous appeal of the book has transcended the narrow walls of the ivory tower, drawing to its authors a glaring public spotlight that only rarely shines on political and literary theorists. In the ongoing public debate over the virtues and flaws of the study, even its most intransigent detractors can hardly deny that *Empire* represents a powerful neo-Marxist contribution to the rapidly emerging field of "globalization studies."

Like this new area of research, Michael Hardt and Antonio Negri's best-selling study is unapologetically interdisciplinary in character. Its authors are willing to paint in broad strokes, in the process eagerly digesting a plethora of materials drawn from legal studies, politics, economics, philosophy, anthropology, cultural studies, comparative literature, and critical theory. Franz Kafka and Herman Melville are made to rub shoulders with Baruch Spinoza and Karl Marx; the postmodernist ideas of Michel Foucault, Gilles Deleuze, and Félix Guattari are interspersed with the modernist insights of Hans Kelsen, Otto Bauer, and Max Weber.

Mixing passionate revolutionary appeals with abstract philosophical investigations and detached sociological analyses, *Empire* constitutes a fascinating tapestry of innovative ideas, brilliant interpretations, flamboyant provocations, shameless exaggerations, and bold generalizations. The 400-plus pages of text pulsate with creative energy, taking the reader on an intellectual roller coaster ride that too often relies on dense language and esoteric terminology. The book incorporates Negri's long-standing sympathies for the central themes of the "autonomist movement" within 1970s Italian communism: skepticism of "big government, big business, and big labor" (p. 350); the "quest for workers' autonomous self-government" (p. 349); and "the multitude's right to self-control and autonomous self-production" (p. 407). Hardt's influence is most obvious in the study's poststructuralist emphasis on the biopolitical (re)production of social life under conditions of late capitalism, in which the economic, the political, and the cultural increasingly overlap and invest one another (see, especially, pp. 22–41).

At the core of the study, one finds the authors' insistence that "Empire" represents a radically new paradigm of authority and control—a "new global order" composed of a series of national and supranational organisms that supersede old, nation-state-centered forms of sovereignty. For Hardt and Negri, "Empire" is not merely a metaphor but a promising theoretical concept signifying a "political subject that effectively regulates these global exchanges, the sovereign power that governs the world" (p. xi). In their view, this single logic of globalization operates in all spheres of social life, perhaps most visibly in the regulation of human interaction, the formation of global markets, the creation of new technologies, the expansion of vast circuits of material and immaterial production, and the generation of gigantic cultural flows. No longer opposed by an extrasystemic "outside," this new form of sovereignty constitutes a regime that effectively encompasses the "spatial totality" of the entire globe (pp. 353, 413). Wielding enormous powers of oppression and destruction, Empire neither establishes a territorial center nor relies on fixed boundaries, but "manages hybrid identities, flexible

hierarchies, and plural exchanges through modulating networks of command. The distinct national colors of the imperialist map of the world have merged and blended in the imperial global rainbow" (pp. xii–xiii) As a regime "with no temporal boundaries and in this sense outside of history or at the end of history" (p. xv), Empire thus dwarfs any particular imperialist project previously undertaken by single nation-states.

Indeed, one of the major debates in globalization studies revolves around the question of whether the current "spatial reorganization of production" (James Mittelman) and the apparent "compression of time and space" (David Harvey) really constitute an entirely new phenomenon. On this issue, Hardt and Negri clearly side with other "hyperglobalizers," such as Susan Strange, Kenichi Ohmae, William Greider, and Martin Albrow, who argue that globalization defines a new epoch of human history, a fundamental reconfiguration of the framework of human action in which nation-states are becoming increasingly less important actors (see, especially, pp. xi, 5–20, 336). Unfortunately, however, Hardt and Negri never discuss in sufficient detail contrary empirical evidence provided by such globalization skeptics as Paul Hirst, Graham Thompson, and Linda Weiss, who conclude, first, that current world flows of trade, investment, labor, and ideas are by no means historically unprecedented and, second, that national governments seem to possess enduring powers to regulate economic activities. This omission points to a systemic flaw that prevades *Empire*: its authors' reluctance to engage fully the rapidly growing, diverse body of literature on the subject of globalization.

After a descriptive analysis of the present features of Empire and a long interpretation of the historical transition from imperialism to Empire, Hardt and Negri conclude on an optimistic note, suggesting that the creative forces of the exploited and subjugated producers that sustain Empire are also capable of autonomously constructing a "counter-Empire"—an alternative political organization of global flows and exchanges fueled by the multitude's "will to be against" (p. 210). Struggling for global mobility, global citizenship, a guaranteed social wage, and the right to the reappropriation of the means of production, this subjugated multitude is poised to invent new democratic expressions and new forms of constituent power.

In this context, Hardt and Negri's discussion of the subversive effects of a political demand for global citizenship is simply superb (pp. 396–400). A direct response to a globalizing world, mass migration and increasing mobility contain a truly revolutionary potential to undermine economic inequalities and asymmetrical power relations that are rooted in an anachronistic defense of fixed boundaries and spatial divisions separating the Northern and Southern hemispheres. Calling for subversion, insurrection, and biopolitical self-organization, Hardt and Negri end their study by introducing the heroic figure of the "militant" as the one who best expresses the life of the multitude. A cross between St. Francis of Assisi and a Wobbly organizer, the counter-imperial militant engages with new vigor in a "revolution that no power will control," thus posing against the misery of Empire "the irrepressible lightness and joy of being communist" (p. 413).

The greatest virtue of the book lies in its authors' ability to construct a new grand narrative of globalization that draws on relevant insights from diverse left traditions such as Marxism, anarchism, existentialism, poststructuralism, critical theory, critical race theory, subaltern studies, and feminist theory. Yet, contrary to its professed philosophical eclecticism, *Empire* remains stubbornly wedded to dogmatic Marxist categories that reify production (albeit in its postmodern,

"biopolitical" version), reaffirm a communist teleology, and reduce the political arena to a "field of pure immanence" (p. 354). Unable to resolve the tension between the posture of revolutionary subjectivism and that of detached objectivism, the authors frequently fall prey to a deterministic view of globalization as a "structurally inevitable" and "irreversible" process (p. 336)—politically disempowering constructions that are the hallmarks of both Marxist and neoliberal ideology.

While rightfully criticizing "ideological master narratives" that are partly responsible for producing the very conditions they purport to explain, Hardt and Negri nonetheless accept all too easily Marxist and neoliberal representations of globalization as an all-encompassing process driven by a single capitalist logic. It is one thing to argue that modernity has both touched and transformed even the remotest corners of this earth, but it is quite another to suggest that the alleged "systemic totality" of Empire is no longer opposed by a qualitatively different "outside." To this reviewer, Hardt and Negri's account of globalization is hard pressed to explain the current renaissance of patriotic discourses that reify the nation-state as the chief provider of security against terrorist attacks from the antimodernist "outside"—or, conversely, exclusivist narratives that glorify the nation-state as the chief supporter of antimodernist struggles against the menacing secular "outside."

Still, *Empire* ought to be required reading for those interested in what is shaping up to be the most significant issue of our time.

Globalization of Japan: Japanese Sakoku Mentality and U.S.

Efforts to Open Japan. By Mayumi Itoh. New York: St. Martin's, 1998. 256p. \$45.00.

Kent E. Calder, *Princeton University*

The tide of globalization that has swept the world political economy over the past two decades is clearly a development of epic importance. Given Japan's economic scale—one-seventh of global GNP and more than one-quarter of world savings—its ability and inclination to respond is likewise a subject of broad international concern. Yet, the debate on Japan's evolving political-economic course has so far yielded far more heat than light.

Despite the bitter, sterile, and increasingly irrelevant struggle of the past decade between "revisionists" and their detractors, there are remarkably few well-researched and theoretically coherent works on the domestic political origins of Japanese foreign policy. There are virtually none in English on the Japanese worldview. This solidly researched and theoretically conscious work, which employs the clear and original concept of *sakoku* to explain Japan's international behavior in a wide range of policy spheres, helps fill an important void.

By *sakoku* (literally: "secluded nation") mentality, Itoh means the parochial worldview with which both the Japanese public and, to a substantial degree Japan's political leadership, approach international affairs. This mindset, she argues, "is not only ubiquitous in the business sector, but is also prevalent in Japan's cultural, educational, and societal systems" (p. 13). "It constitutes the core of Japanese foreign policy decision-makers' attitudinal prism" (p. 14).

The first third of *Globalization of Japan* is a concrete explication of how *sakoku* mentality originated and how it is expressed consistently in Japanese perceptions of the United States and Asia as well as in Japanese prescriptions for economic diplomacy. The other two-thirds of the volume present five richly detailed empirical studies of how *sakoku* is expressed in concrete policy terms, followed by a conclusion that

assesses prospects for Japan's internationalization in the face of such parochial tendencies. The cases cover immigration, Okinawa policy, rice liberalization, collective security, and UN peacekeeping. The conclusion includes suggestions on how foreign nations should deal with Japan, given its deeply rooted but not incorrigible parochial tendencies.

Itoh's provocative overall argument involves a number of contentions. (1) The internationalization of the Japanese mind has not occurred. (2) Cultural barriers are among the foremost barriers to internationalization. (3) Other Asian nations, such as Korea and Taiwan, are responding more actively and sensitively to globalization than is Japan. (4) Japan has a long way to go, but (5) significant future movement toward true internationalization is not impossible. To encourage internationalist tendencies, Itoh argues that the United States, in particular, should continue to pressure Japan to liberalize but should tailor its appeals more sensitively to exploit potential for collaboration with allies inside Japan (pp. 183-5).

The strength of the book is its combination of a clear original analytic concept (*sakoku* mentality) with meticulous application in a series of admirably well-researched cases. Itoh makes no pretensions to grand theory, but she draws effectively on Robert Jervis, Jack Snyder, Harold and Margaret Sprout, and other international relations theorists to situate her argument. The *sakoku* concept seems to have explanatory value across a remarkably broad range of cases, including some, such as the textbook controversy with Japan's Asian neighbors, that have intensified since the book was written.

This creative and carefully researched volume does, to be sure, raise some important yet unanswered questions. Most important, Itoh does not clearly come to terms with arguments that blame institutions, rather than parochial mentality, for Japan's disappointing lack of prointernationalist behavior. She does observe, intriguingly, that on immigration issues the Japanese government has often been more rigid than the general populace (pp. 109-18). Itoh does not pursue the possibility of institutionalist alternatives to her worldview-based explanation very thoroughly.

Itoh is stronger in her description of current Japanese realities than in her policy prescriptions. Few, for example, would quarrel with her sensitive and original observations that Okinawans have suffered as a result of mainland *sakoku* mentality. Whether it follows that making Okinawa (or any individual region) self-supporting should be a national policy priority is not so clear.

Overall, this is one of the four or five most creative and solidly researched studies on the politics of Japanese foreign policy to appear in the past decade. It deserves wide readership among students not only of Japanese politics and policymaking but also of international relations more generally. Written from the unique standpoint of an expatriate Japanese scholar, it casts critical yet sensitive light on problems of growing importance to our increasingly integrated world.

The Cultural Construction of International Relations: The Invention of the State of Nature. By Beate Jahn. New York: Palgrave, 2000. 182p. \$65.00.

The State and Identity Construction in International Relations. Edited by Sarah Owen Vandersluis. New York: St. Martin's, 2000. 207p. \$69.95.

Cynthia Weber, *University of Leeds*

Conceptualizing the sovereign nation-state remains a core concern in the discipline of international relations (IR). Yet, as the volumes by Sarah Owen Vandersluis and Beate Jahn demonstrate, the theoretical location of this conceptual debate is shifting. Questions of identity, like those re-

garding sovereign nation-states, were answered in the 1990s with reference to terms like social construction. In the new millennium, "the social" is increasingly joined by "the cultural" as an intellectual marker of how serious IR scholars must pose questions of identity. Why this shift? And what difference does it make to our understandings of sovereign nation-states, not to mention IR theory more generally?

The glib answer to the first question is that the discipline of IR is following trends found in the humanities and social sciences generally, albeit typically years behind other disciplines. So the shift in IR is simply a reflection of what occurred earlier elsewhere. Even if this is the case, what accounts for this shift in the first place? Some might say it is the (postmodern?) ungrounding of the terms social and cultural, making the first a less necessary place from which to theorize question of identity and the second a less objectionable place to begin such theorizations. Others might argue that the shift from the social to the cultural is less theoretical and more material, reflecting a recognition that contemporary global phenomena must be equally understood through notions of consumption (more often associated with cultural consumption) as well as through notions of production (more often associated with social construction).

Whatever the reasons for the shift in emphasis from the social to the cultural, the volumes by Vandersluis and Jahn offer an opportunity to address the more important second question: What difference does this shift make for our understanding of sovereign nation-states and for IR theory in general?

The Vandersluis volume consists of a series of engagements with "the question of how to understand the state and its relationship to identity" (p. 4), without caving into the tired debates about whether the state is whithering away in an era of globalization. Organized into two sections, "Constructions of the State" and "Beyond the National State," the volume adopts a self-consciously social constructivist perspective. Its project is to undertake "an interrogation of the ways in which the state and sovereignty as intellectual categories and as historical practices are very slowly changing in response to the different demands, expectations, and constraints being placed upon them" (p. 1).

After a brief "Prologue" by James Mayall and Vandersluis, which does a nice job of offering a coherent overview of what is to follow, the first part of the book contains four essays that address primarily post-World War II understandings of the state. In "The State of International Relations," Martin Shaw provides a synopsis of his historical sociological "globality" argument, which presents the contemporary international system not only as socially constructed in general terms but also as characterized by a Western unified state conglomerate and other centers of state power. Christopher Clapham's chapter, "Degrees of Statehood," argues against an absolutist understanding based on his discussions of statehood and rival centers of power, such as guerrilla movements in sub-Saharan Africa.

The next two chapters in Part I examine the nation/state relationship. Dominique Jacquin-Berdal discusses Eritrean national identity and examines how the principle of "one-nation, one-state"—usually used to legitimize existing states—was in this case used to justify national separatism. Peter Shearman's chapter, "Nationalism, the State, and the Collapse of Communism," analyzes the case of the former Soviet Union to suggest how nationalism, globalization, and democratization are connected in the contemporary world order.

In Part II, three essays take up alternatives to the nation as the only legitimate site for securing state identity. Tim Dunne,

in "Colonial Encounters in International Relations: Reading Wight, Writing Australia," reads Australia's encounter with colonialist diplomacy, law, and theories of identity to argue for a "radicalized post-colonial Rationalism [that] can contribute to the process of reconciliation" (p. 124). By focusing on Southeast Asia, Hari Singh, in "Hegemons and the Construction of Regions," looks at how hegemons construct identifiable regions and how these regions influence domestic and international relations. Naomi Mobed's chapter, "Culturalizing Security: Narratives of Identity and the Politics of Exclusion in the Arabian Gulf," examines how the practices and interests of the Gulf Cooperation Council are constructed at the intersections of two discourses of identity, one based on Western state forms and the other based on traditional religion.

Overall, the Vandersluis volume is accessible, clear, and varied. It may not offer many novel theoretical insights and does not risk straying as far away from the state and the nation as it might in the discussion of sovereign identity, but its contribution is to be found in its organization around key questions about state identity and some alternatives and in an array of strong empirical pieces, all of which enact a social constructivist project in one way or another. As Daniel Warner States in his illuminating "Epilogue," "what makes these chapters interesting is that they indicate the variety of responses available to [what Warner calls the state/sovereignty/institutional] dialogue" (p. 197).

In many ways, Beate Jahn's volume is a nice companion to the Vandersluis collection. It also might be classified as social constructivist. Jahn traces the social construction of the concept of the state of nature, which she explains undergirds both realist and liberal conceptualizations of the state, back to its specific historical introduction by the Spanish in their encounter with the Amerindians. In so doing, she produces a straightforward deconstruction of the nature/culture dichotomy and some of its uses in IR. Put simply, Jahn argues that there is nothing "natural" about what IR calls a state of nature, which is a cultural construct.

Jahn does not stop there. What distinguishes her book from the Vandersluis volume is her preoccupation with how the construction of the state of nature functions culturally. Drawing upon the Clifford Geertz notion of thick description, Jahn argues that state of nature stories function as a cultural context to IR theory generally. She positions herself as a cultural analyst (in addition to a social constructivist) and demonstrates how IR theory conceives of culture as a problem and nature as its solution, how culture and nature became opposed in the Spanish/Amerindian encounter, and therefore how the Western concept of the state of nature is rooted in this cross-cultural experience, thereby making it cultural rather than natural. When IR theorists base their theories on state of nature stories, which construct a hierarchy of cultures between so-called civilized and so-called primitive peoples, they are "justifying in the process the very policies which have produced injustice and inequality in the international sphere in the first place" (p. 155).

Jahn concludes: "Culture, then, exerts quite a considerable power in mainstream International Relations. For the concept of the state of nature contains a culturally peculiar understanding of human nature, history, and destiny" (p. 168). Jahn argues that IR theory would do well to abandon such a concept and is particularly well placed to do so. It should enquire "into the conditions of cooperation and conflict between cultures which are mutually constitutive and subject to change, rather than hope for the day when nature will eventually overcome culture" (p. 169).

Jahn's story is a powerful one. The author convincingly deconstructs one axis of the nature/culture dischotomy in IR

theory and analyzes its effects. Even though Jahn often slips from theorizing culture as a context into theorizing it as an ontology, her analysis enables us to think of IR theory not only as socially constructed (which has become a euphemism for "change" in IR theory) but also as cultural practice, as what Geertz has called "an ensemble of stories we tell about ourselves" (Clifford Geertz, *The Interpretation of Cultures*, 1975, p. 448).

Overall, these two volumes lead us to engage not only with specific IR concepts, such as the sovereign nation-state or the state of nature, but also with IR theory itself conceived of as cultural practice. "Culture" in IR theory should not reducibly be read as merely "change" but as complexity analyzed in much the same way Michel Foucault read power (Gayatri Chakravorty Spivak, *Critique of Postcolonial Reason*, 1999). Such a cultural analysis of IR theory is indeed distinct from a social constructivist analysis, having implication for how we understand not only sovereign nation-states and states of nature but also IR theory more generally.

The International Dimension of Genocide in Rwanda. By Arthur J. Klinghoffer. New York: New York University Press, 1998. 208p. \$40.00.

Peter Uvin, *Tufts University*

This well-documented book analyzes the manifold inconsistent, inefficient, piecemeal, and sometimes counterproductive ways the international community intervened in the dynamics of violence that beset Rwanda before, during, and after the 1994 genocide. Klinghoffer takes a position that runs counter to the popular description of the international community as being totally inactive and uncaring before and during the genocide. Rather, he maintains, interventions did take place, but they failed to achieve their aims. At a descriptive level, Klinghoffer, following Bruce Jones ("Intervention without Borders: Humanitarian Intervention in Rwanda 1990–1994," *Millennium* 24 [Summer 1995]: 225–49), makes this point rather successfully. The book contains many interesting pieces of information that, taken together, force us to acknowledge some new elements in the simple picture that has become so dominant about the international community's conduct in Rwanda.

Klinghoffer also seeks to make a few broader, more theoretical, arguments based on his empirical analysis. The foremost is that sovereignty is withering: The events demonstrate that sovereignty "had fallen by the wayside" (pp. 13, 61, 154) or is in "death throes" (p. 154). But more is required to demonstrate that sovereignty is close to death than merely documenting the presence of ethnic hatred, regional war, and cross-border interventions. A second theoretical point is a plea for "humanitarian realism," that is, a serious look at the combination of altruism and self-interest that underlies many countries' behavior in such crises as Rwanda. This point has been made better both empirically and conceptually by Stephen Garret (*Doing Good and Doing Well. An Examination of Humanitarian Intervention*, 1999). In short, judged on its theoretical contributions, this is a rather disappointing book.

In its attempt to make an original argument that runs counter to established consensus, the book suffers from some defects. The background section is weak; it simplifies too much, draws solely on far-removed tertiary sources (see, e.g., note 1 p. 6), and occasionally contradicts itself (compare data on 1959–62 refugee flows on pp. 8 and 13). In addition, in what I take to be an effort to demonstrate the complexity of the situation, Klinghoffer seems to adopt a policy of equal criticism against the Habyarimana government and the Rwandan

Patriotic Front (FPR). This creates some odd outcomes: To mention but one extreme example, the discussion of the number of persons allegedly killed by the FPR after the genocide (pp. 64-5) is significantly longer than any discussion of the number of people slaughtered during the genocide.

More generally, the author ends up following an overly simplistic "they are all bad guys" storyline, which oversimplifies the complexities and ethical quandaries involved. In order to make his points, Klinghoffer tends to overstate the degree of external peace efforts in the Rwandan crisis before the genocide. There is much very good information pointing to the diplomatic trail left by Western countries as they sought to promote peace. Simultaneously, however, a great deal was done by the international community that seemed to be totally indifferent to the dynamics of violence, exclusion, militarization, and polarization in Rwanda. (I, among others, document this at length in *Aiding Violence. The Development Enterprise in Rwanda*, 1998). The latter information does not fit into Klinghoffer's points, so he simply does not mention it.

This book adds some important questions to the standard story about the Rwandan genocide. It provides a nice collection of factual information to make that point. Its weakness is that it is rather obsessed with making this point, as well as a few theoretical ones that supposedly follow from it. This seriously reduces the analytical and descriptive quality of the overall work. I recommend it for those who are familiar with the Rwandan case and want a well-documented argument that asks some tough questions. I do not recommend it to people seeking a nuanced and well-grounded understanding of the Rwandan genocide's causes or, indeed, even the overall performance of the international community.

Same Bed, Different Dreams: Managing U.S.-China Relations, 1989-2000. By David M. Lampton. Berkeley: University of California Press, 2001. 497p. \$35.00.

David Bachman, *University of Washington*

David M. Lampton's book on the U.S.-China relationship is the best in a series of strong works on the topic in recent years. In contrast to James Mann's *About Face* (1999) and Patrick Tyler's *A Great Wall* (1999), books by scholarly journalists that cover the history of the relationship from the late 1960s to the late 1990s and that use declassified documents from the U.S. side extensively, Lampton concentrates solely on the former Bush and Clinton administrations. Moreover, he takes great advantage of his experience as president of the National Committee on United States-China Relations from 1988 to 1996 (he was professor of political science at Ohio State University before serving on the committee, and he is now a professor at Johns Hopkins School of Advanced International Studies) to illustrate the ins and outs of U.S.-China relations. In contrast to Mann and Tyler, Lampton had great access to top Chinese leaders (and top U.S. officials as well) on a recurring basis, and the Chinese side is presented here with more insight than in any other source.

The book is part memoir, part history, part description, and part issue analysis of the dynamics of U.S.-China relations from early 1989 until the last days of the Clinton administration. Political scientists looking for great theoretical insights will be disappointed, but for those who want to understand the nature and evolution of U.S.-China ties, this is the best book to read on the subject.

Seven major themes are interwoven throughout the text. (1) The U.S.-China relationship can be seen as operating on three levels—global, domestic, and individual leader. (2)

Leaders confront two very different constituencies—global and domestic. (3) Interdependence and external demands create opportunities for nationalistic appeals. (4) China is the largest late modernizer and a growing force in international affairs. At the same time, Chinese leaders and citizens see China as having been victimized by the international system for a long time. (5) Secure and effective political leadership in both countries is a key to managing the bilateral relationship well. (6) The challenge of managing this relationship will only grow in coming years. (7) Despite differences, the United States and China share many important interests, and this gives the relationship more durability than is commonly thought.

Lampton sees four main or potential turning points in U.S.-China relations during the period 1989 to 2000: maintaining the relationship after the Tiananmen massacre of 1989; Clinton's linking and delinking of human rights and China's most-favored-nation status in 1993-94; the Taiwan Strait crisis of 1995-96; and the series of events between April and November 1999, when the United States first turned its back on major Chinese concessions for a bilateral agreement on China's accession to the World Trade Organization, then bombed the Chinese embassy in Belgrade, and finally concluded the bilateral accord on WTO accession. These historical events provide the structure for much of the work, but there are chapters on security issues (with a tour d' horizon of China's international relations), economics and human rights, global institutions, the role of third parties in the bilateral relationship, the images by which each side sees the other, and how their media portray each other.

This is the first book to read on Sino-American relations for the 1989-2000 period, but it is not flawless. The major problem is that the book tries to be a bit of everything. Is it a history of the relationship? Is it an overview of issues? Is it an analysis of conditions in which the relationship is likely to be good (or bad)? Is it an analysis of conditions in which the relationship is likely to be good (or bad)? Is it justifying a series of policy recommendations? Is it designed to show the importance of the National Committee on United States-China Relations (this reviewer is a member of that committee)? Because Lampton tries to do some of all these things, he provides less depth on a number of issues than might be the case if there were a clearer sense of what the book is supposed to be.

A second problem is that of a bias (which I share). Lampton is deeply committed to U.S.-China relations and more generally to the policy of engaging China. As a result, more critical views of U.S.-China ties do not get the type of play that one might hope. This can take several forms or directions. Some expect the Chinese Communist Party will fall from power (or, indeed, believe that the United States should work to hasten its fall), but Lampton does not say much about the implications of these beliefs for the relationship. China's growing military capabilities and human rights abuses are discussed, but they do not receive a balanced treatment or are not constructed into an effective dialogue with the "comprehensive engagement" school of thought.

Implicitly, Lampton presents a view that is sympathetic to the primacy of the executive branch of government and to business in the setting of U.S.-China ties on the U.S. side, a view that is critical of the role of Congress or the role of public opinion in the framing of American policy toward China. Thus, when Lampton examines how individuals matter in the policy process, he highlights congressional critics of engagement (no member of Congress is singled out for advancing U.S.-China relations) and, for an example of an individual who advances the relationship, highlights Maurice Greenberg of the American International [Insurance] Group,

who was a major supporter of the National Committee on United States–China Relations. The section on Greenberg almost reads like a thank-you note for the work he did for the committee when Lampton was its president.

These criticisms do not detract from the vast majority of insights provided in this book, which is, as stated earlier, the best work on the subject. But will those who do not share Lampton's views on engagement be persuaded by his analysis? Unfortunately, for all too many people, U.S.–China relations are no longer an issue on which rational argument can contribute to the setting of good policy.

Peace, Power and Resistance in Cambodia: Global Governance and the Failure of International Conflict Resolution.

By Pierre P. Lizee. New York: St. Martin's, 2000. 206p. \$65.00.

David W. Roberts, *University of Ulster*

This work discusses conflict resolution in the Western paradigm and Cambodia's recent experience of that. Lizee makes a notable contribution to our understanding of the management of transitions from conflict to peace in this sophisticated piece of analysis. The work revolves around the general hypothesis that the failure of the Cambodian peace process is attributable almost entirely to the inappropriate character of the Western-determined peace process. The book starts by comparing the evolution of conflict management processes in the West and in Cambodia; Lizee makes the differences quite clear, and this part of his work is very strong as an indicator of the evolution of socioinstitutional mores in Cambodia (especially pp. 39–43). He argues that a critical tension accounts for the failure of the Paris Peace Agreement (PPA). This is to be found in the Khmer approaches to social harmony through balance-of-power equations and Buddhist values.

Political violence and conflict resolution in Cambodia can be explained, claims Lizee, because harmony is only maintained when there is a weighted balance of power in favor of a hegemonic group, such as a political clan or monarchy. When a weaker body tries to redress this, it destroys the fabric of harmony, legitimizing violent behavior by the challenged party or grouping. This was probably the case in the fighting that erupted in July 1997, inaccurately characterized in much of the less critical literature as a "coup" by Prime Minister Hun Sen. Moreover, Lizee continues, the academic literature does not acknowledge such concepts, so it can provide no alternatives for the unsuccessful peacekeeping operation in Cambodia between 1991 and 1993. This process, it is claimed, is self-reinforcing because of the repetition of Western peace-creating assumptions, which are transmitted through globalization and which ignore difference in cultural traditions of conflict resolution and the use of violence in settling disputes.

The book is divided into four parts. The second part is directed toward proving the impropriety of the PPA by applying an analysis of cultural institutions and processes in Cambodia to the PPA. According to Lizee, the PPA was conceptually and culturally flawed from the outset, letting down Cambodians and Westerners alike. A great deal of this analysis is thoughtful and probably gleaned through personal insights into Cambodian culture, but it neglects the limitations specific to the evolution of the peace process. Lizee discusses the stages of the war and its movement to peace in a clear manner, and he concludes that the outcome of the political settlement is inappropriate for Cambodia. But this was not because people were ignorant of, or well versed in, Khmer culture. Rather, the limitations were those of geopolit-

ical Cold War realism. The PPA did not take the shape and form that it did because not enough policymakers had an inside track on Cambodia. It took its rigid form because that was what the United States and China sought as conditions for normalization between them and Viet Nam and the USSR.

Throughout the peace process, and certainly up until mid-1990, Washington demanded inclusion of the Khmer Rouge, not because this made sense in terms of conflict resolution but because they were a tool with which to continue the confrontation with Hanoi. The peace process itself only broke through the impasse of the Cold War when the USSR and Viet Nam acquiesced to the terms and conditions for normalization. Lizee provides evidence of these processes, but he does not relate them to the PPA and the failure he identifies. Thus, to blame the whole failure on the cultural limitations of the peace plan is to neglect the overarching shaping process conducted as an adjunct to normalizing relations with Viet Nam and, to a lesser degree, Cambodia. It seems hard to blame either the PPA or the UN Transitional Authority in Cambodia (UNTAC) if their contours were shaped largely by the various constraints that framed and determined the peace process.

Part three of Lizee's work seeks lessons from the peace-making experience in Cambodia that do not focus on orthodox arguments regarding technicalities and logistics. It teases with the suggestion that the author is going to provide some evidence as to why the main parties did not cooperate, but this is not forthcoming. A minor criticism is that Lizee describes the Cambodian people as being "fortunate enough to benefit" from the UNTAC operation (p. 139), which seems a little patronizing.

Part four argues that many important lessons from the 1993 experience were not incorporated into the 1998 elections. Lizee misses one crucial new experience, when the opposition refused to accept the victory of the Cambodian People's Party (CPP). The solution was found in the creation of the Senate, an unelected upper house into which the defeated parties' members could be dispersed in the traditional patronage and clientelist system of loyalty and reward. Also, Lizee takes a rather limited perspective on what he terms the "coup" of July 1997. Evidence available since then challenges strongly Lizee's view that Hun Sen was to blame. Furthermore, there is no reference to the fact that Hun Sen was publicly seeking to integrate the remaining Khmer Rouge while Ranariddh was engaged in covert processions with the intention of recruiting forces to influence the all-important balance of power against Hun Sen and the CPP. And there is no reference to Ranariddh being caught red-handed while smuggling weapons into Cambodia disguised as machine parts.

Despite the sometimes dense writing style (e.g., p. 47, para. 3; p. 76, para. 3), this work goes farther than most on this topic. It is especially welcome for offering a sophisticated conceptual alternative to the usual explanations for the failure of UNTAC as being rooted in administrative, technical, and logistical problems. It is marred, however, by certain evaluations, including the persistent reference to "failure" of UNTAC, which did surprisingly well in many areas. Any failure in conflict resolution was a fault neither of UNTAC as an institution nor its members (generally). The fault, as Lizee correctly argues, lies in the international nature of the PPA and the fact that the Cambodian conflict was not settled due to any concern for the Cambodian people. Rather, the PPA ended a superpower conflict in a manner that suited China and the United States. Those needs were incongruent with conflict resolution in Cambodia, and Lizee does well to illustrate this at certain points. With some exceptions, this is

a valuable contribution to peace and conflict studies and to work on Khmer.

Democratic Commitments: Legislatures and International Cooperation. By Lisa L. Martin. Princeton, NJ: Princeton University Press, 2000. 225p. \$49.50 cloth, \$18.95 paper.

Kathleen R. McNamara, *Princeton University*

One of the sharpest and most consequential divisions in international relations lies between those who believe that international politics is a realm unto its own and those who see the lines between domestic and international politics as both permeable and pertinent. For the former group, the consequences of anarchy swamp any potential for politics to be ordered at the international level as it is within states. In this view, the dynamics of domestic political life, such as the rule of law, norms of trust, or the independent effects of institutions, are foreign to the international system.

Lisa L. Martin's groundbreaking new book, *Democratic Commitments*, challenges this (still dominant) canonical viewpoint by forging strong theoretical and empirical linkages between the realm of domestic legislative institutions and international cooperation. By demonstrating that legislatures in stable democracies have the potential to increase the credibility of states' commitments and thus improve the chance of international cooperation, this book contributes significantly to emerging work on the interaction between the domestic political sphere and international outcomes.

The nature of domestic political arrangements has been shown before to be important for international politics, of course, but a persistent obstacle plaguing the literature is a lack of systematic theorizing as well as overly broad hypotheses. So many things may matter in the domestic realm; the trick is to focus on one element without trivializing it. Martin offers a good example of how to solve this problem by positing the role of the legislature as a crucial explanatory variable in the progress of international cooperation. Legislatures are a key element in the broader debate about the nature of democracy's effects on interstate relations, but they have not received ample study. Martin points to how legislative-executive arrangements reverberate back and forth to the systemic level, and the challenge of credible commitment acts as the fulcrum of her investigation.

The general analytical framework explains how differences in international cooperation may vary with different patterns of domestic legislative structure because of their effect on the credibility of national commitment. Martin evaluates alternative perspectives on executive-legislature relationships, and then she teases out more specific hypotheses from different logics of institutional delegation and how they might play out in the area of international cooperation. Legislative influence, not executive dominance, is her emphasis, against most of the literature and much conventional wisdom. Provocatively, she argues that instead of being pesky interlopers who ruin the chances for serious diplomacy, legislatures contribute to positive and cooperative outcomes by allowing agreements to be reached that are more likely to be implemented.

Martin parses out specific predictions and applications of her general framework in several empirical chapters that range over a variety of areas, but all examine the level of executive discretion in foreign policymaking and the effects of such discretion on the policy outcomes. In the U.S. context, she looks at a series of executive agreements and treaties over the postwar era, using statistical methods to explore the interaction among the domestic influence of legislatures, decisions regarding ratification processes, and the credibil-

ity of U.S. commitments to specific agreements. Other chapters examine economic sanctions and variation in the role of Congress, and foreign aid, particularly food assistance.

The second part of the book applies Martin's line of questioning to Europe and parliamentary governments. Arguing quite rightly that the general outlines of her approach should be applicable in a variety of comparative settings, the author examines how variation in the organization of legislatures across the European Union (EU) had a predictable effect on the particular role member states have played in European integration, particularly decisions over the single market. This research is particularly helpful, given the paucity of systematic empirical work on the role of electoral politics in the path to EU integration.

The book also contributes usefully to practical debates in the EU over democratic accountability, in which democracy and efficiency seem locked in a zero-sum battle. Martin suggests that under some conditions (when legislative involvement makes implementation of agreements more likely) these two ideals can be complementary. Her reading of the EU case suggests that legislative involvement increases when there is a high level of conflict between the executive and the legislature on an issue, but she argues that this may be good in the long term, by making the commitments of states internationally more durable and implementable. This focus on the dynamics of implementation is a real strength of Martin's research, as most scholarship in international relations stresses only the negative effects of legislative involvement in bargaining or ratification processes.

Readers may not be satisfied by some of the solutions to empirical challenges. Much of the analytical story is about indirect, subtle mechanisms of influence. Anticipated reactions, such as the ways in which executives may *a priori* take into account legislators' preferences, and the dynamics of credibility are two areas critical to political life, but there is unlikely to be much in the way of direct evidence. Martin employs proxy tests and looks for observable, logical consequences in lieu of process-tracing methods. For example, credibility, an inherently intersubjective social phenomenon, is measured by outcomes, that is, commitments are credible when a state does what it says it will do. Despite the thoughtfulness and sophistication with which Martin rises to these challenges, some readers may decide the empirical work demonstrates correlation but not causation. Also, Martin's cases were chosen because they represent interesting substantive areas not probed by other authors, not because they offer the maximum leverage on her critical variables. The mechanics of executive-legislative relations at times become quite complex and contingent in her account, as the specifics of the cases vary dramatically. But these points are dealt with up front by Martin and do not detract from the overall contribution of the book.

As we prepare for the next round of GATT negotiations, we will need to know how domestic politics may play into the future of cooperation. The crucial issue of the timing of the EU's enlargement to the East will undoubtably continue to be entangled with domestic politics as well. The implication of Martin's work for these and a host of other issues demands attention from scholars as well as from policymakers concerned with the future of these international agreements and institutions. Joined (although not necessarily in agreement) with other important works, such as Helen Milner's *Interests, Institutions, and Information* (1997), the ideas advanced by Martin crystallize one of the promising new directions in the field at the turn of the millennium. Her forceful view that "part of the secret of democratic success lies precisely in its supposed handicap, constant interference in policymaking by legislative actors" (p. 47), is bound to cheer some and provoke

many more, both in the scholarly world and in the real one Martin writes about so astutely.

Justice and Reconciliation: After the Violence. By Andrew Rigby. Boulder, CO: Lynne Rienner, 2001. 207p. \$49.95 cloth, \$19.95 paper.

Gary J. Bass, *Princeton University*

Much of the growing literature on human rights issues focuses on the post-1989 democratizations and the ethnic conflicts of the 1990s. It reaches across many disciplines, including work not just by political scientists but also by legal scholars, historians, sociologists, anthropologists, journalists, and practitioners. Much of the literature confronts the tension between the pursuit of justice and the pursuit of stability. Andrew Rigby, who teaches on forgiveness and reconciliation, contributes an eclectic and good-hearted meditation on the dilemma of political reconciliation after mass atrocities.

Rigby considers a number of different ways of coming to terms with the past: forgetting about it; holding trials or simple purges; setting up a truth commission; paying reparations. He briefly considers the effect of these various options as they have played out in different cases: Germany and Western Europe after World War II, Spain after the Franco dictatorship, Chile after Pinochet, Argentina after the “dirty war,” Eastern Europe after communism, South Africa after apartheid, and others. His goal is the creation of “a new culture of respect for human difference and human rights—what some might term a culture of peace as opposed to a culture of violence” (p. 183).

The book reveals an intelligent skepticism about all these policy options; for instance, some Latin American truth commissions were not allowed to single out perpetrators by name (p. 9). “There is an ongoing tension between the need for truth, the quest for justice, and the desire for peace” (p. 12). Justice is not an absolute value for Rigby; it does not trump all other political concerns. More specifically, following Samuel P. Huntington’s *The Third Wave* (1991), Rigby argues that “wherever the transition has been negotiated rather than imposed, some kind of amnesty is almost inevitable, particularly if the parties to the settlement continue to possess the capacity to shatter the peace” (p. 184).

Rigby does not just think this tradeoff is inevitable; he suggests that it may, perversely, be helpful. His most intriguing argument is that dwelling on terrible events in the past may not be a wise way to deal with them: “I am not convinced of the appropriateness of opening up the past and talking about it as a means of dealing with the hurt” (p. 1). He even notes that he has lost loved ones in terrible circumstances: “I will not forget them; to do so would dishonor them in some way. But I do not want the pain of those times to come back” (p. ix). This runs contrary to the therapeutic argument popular among some human rights advocates, who see truth commissions and trials as cathartic opportunities for the surviving victims to face and thereby overcome past traumas.

But Rigby does not always stick to these guns. Toward the end of the book he also argues that victims need to be “heard and validated” in order for members of a shattered society to build “a new definition and relationship that acknowledges difference but on the basis of a shared identity as survivors and as human beings” (p. 186). He also suggests that France has been too quick to close the book on its Vichy past, and Spain was wrong to engage in “a collective exercise in public amnesia” (p. 54) about Franco’s regime in order to proceed with the establishment of parliamentary democracy. This sounds more like the therapeutic argument he earlier seemed to be against. In a splendid book, *Unspeakable*

Truths: Confronting State Terror and Atrocity (2001), Priscilla B. Hayner gives a more sustained and richer account of the difficulties of pursuing truth, and she points out it is not clear whether truth commissions provide the kind of therapeutic effects that Rigby sometimes attributes to them.

Unlike Hayner, Rigby relies almost exclusively on secondary sources. There is always a potential tradeoff between depth and breadth. The liveliest chapter is the only one for which he has done primary work: on Palestinians accused of collaborating with Israel. He condemns the internecine violence among Palestinians, many of whom are targeted for personal rather than political reasons, as “the worst kind of lynch law” (p. 147; see also Mark Tessler, *A History of the Israeli-Palestinian Conflict*, 1994, pp. 747–8, and Baruch Kimmerling and Joel S. Migdal, *Palestinians: The Making of a People*, 1993, p. 268). For the rest of the cases, Rigby covers a lot of ground in a relatively slender book, which shows a laudable ability to synthesize broadly; but this comes at the expense of digging more deeply into the cases.

This book adds to a growing discussion. The question of the therapeutic effects of trials is one that might usefully be supplemented by the work of psychologists, as well as by a reconciliation specialist such as Rigby, all of them joining in what is increasingly a multidisciplinary area for study.

Appeasement in International Politics. By Stephen R. Rock. Lexington: University of Kentucky Press, 2000. 237p. \$29.95.

David Cortright, *University of Notre Dame*

Since Neville Chamberlain’s concessions to Adolph Hitler in Munich in 1938, appeasement has become a term of disrepute. The word is almost an epithet, denoting weakness in the face of aggression. Generations of scholars and policymakers have learned the lesson that appeasement emboldens the aggressor and makes war more likely. Academic attention has focused instead on deterrence theory and the role of coercion and compellence as key elements of international politics.

In recent years scholarly interest in inducement policies has been rekindled. Stephen R. Rock’s new book is an important contribution to this emerging literature. Rock challenges the assumption that concessions do not work, and that hostile leaders cannot be appeased. He reminds us that appeasement was once an accepted practice of European diplomacy and was considered an effective means of reducing tensions and removing the causes of conflict. Through analyses of five cases—British concessions to the United States in the late 1890s, the appeasement of Germany before World War II, the Anglo-American acceptance of Soviet demands at Yalta, the American “tilt” toward Iraq in the late 1980s, and the use of incentives that led to the 1994 Agreed Framework containing North Korea’s nuclear program—Rock offers lessons on the benefits and limitations of conciliatory strategies. He develops theoretical propositions and policy guidelines to aid scholars and policymakers in assessing the merits of inducement policies.

As Rock notes, appeasement is a subcategory of the broader concept of engagement. He acknowledges that engagement is a more widely accepted term among scholars and policymakers. Engagement is the more encompassing concept, referring to the building of longer-term cooperation. Appeasement tends to be narrower in scope, referring to concessions that remove the causes of conflict and reduce tensions. Because Munich still casts such a long and dark shadow, it would be best to subsume the analysis of appeasement under the general heading of engagement. A book with the word appeasement in the title is inherently handicapped.

This is unfortunate, for there is much insight and wisdom in Rock's compact volume.

Rock catalogs the various purposes that appeasement policies may serve, the mechanisms by which inducements exert influence, and the factors that account for success or failure. Inducements may seek to preserve the status quo—through crisis resolution and prevention—or to alter conditions—by offering political tradeoffs and developing alliances. The four mechanisms by which such policies reduce tensions are satiation, reassurance, socialization, and altering political dynamics within the adversary regime. Incentive policies, like sanctions, are an attempt to influence the political debate within a target or recipient regime by strengthening the hand of advocates of cooperation, while weakening the position of hostile factions. These internal influences are most likely to occur where some degree of pluralism exists.

While it is true that war-seeking states cannot be appeased, most countries that engage in hostile behavior are pursuing objectives that can in theory be appeased. Rock draws a major distinction between hostile policies that are driven by greed—a state's pursuit of territory, resources, or commercial advantage—and those that are security-driven—when a nation is motivated by concerns for its safety. Rock's characterization of these motives and how they function in different cases is not always convincing—nor is his attempt to define the circumstances in which incentives should be conditioned on reciprocity and accompanied by threats. Rock argues that when the adversary is motivated by greed, inducement policies should be conditional and combined with coercive measures. When the adversary is motivated by insecurity, inducements should be offered without the requirement for reciprocity and without accompanying threats. These are innovative propositions, but they have little grounding in Rock's case studies. They are based more on what he terms their "logic" than on hard empirical evidence. These propositions may be valid in some circumstances, but they need more thorough testing than Rock offers in his study.

The nature of inducements and how they are perceived by the adversary are crucial to the chances of success. Inducements must be properly directed to address the goals and motivations of the adversary. They should be delivered as much as possible to the supporters of reform rather than hostile factions. They must be of sufficient scale to satisfy the adversary. Finally, the state or coalition offering incentives must send clear signals and have a reputation for fulfilling its commitments. Mixed or confused messages and a failure to deliver on promised incentives can undermine effectiveness. As Rock notes, the hesitation and slowness of the United States in fulfilling its promises under the 1994 Agreed Framework have impeded progress in freezing Pyongyang's nuclear weapons program and slowed efforts to tackle ballistic missile testing and other contentious issues.

Inducement policies carry significant risks. They may be perceived by the adversary as a sign of weakness and may lead to further attempts to extract concessions. The adversary may become more daring and aggressive in pursuit of its hostile objectives. Rock argues that this is more likely in a regime that is motivated by greed than by insecurity, although his evidence in support of this claim is limited. Rock is on firmer ground when he identifies means for avoiding this moral hazard. A state can diminish the chances of being perceived as weak by making concessions conditional on reciprocal cooperation, by establishing a reputation for firmness and strength, and by maintaining the materiel capability to employ deterrent or coercive strategies if conciliatory gestures fail. In the case of North Korea, the United States and its South Korean and Japanese partners offered incentives on a step-by-step basis and made the delivery of

benefits conditional on reciprocal cooperation from the other side.

Rock's case studies and theoretical propositions significantly advance our understanding of the strengths and weaknesses of inducement strategies in the conduct of international affairs. The book has limitations, including an overreliance on the distinction between greed and insecurity as motivations for hostile behavior, but its strengths far outweigh its weaknesses. Rock has confirmed, as other scholars have demonstrated, that engagement strategies can be and have been successful in advancing international cooperation.

Vehicle of Influence: Building a European Car Market. By Roland Stephen. Ann Arbor: University of Michigan Press, 2000. 240p. \$49.50.

James A. Dunn, Jr., *Rutgers University, Camden*

The significance of European integration as a process and an intellectual focus is undeniable. The economic importance of the European auto sector and its worldwide dimensions are also self-evident. Roland Stephen's effort to deepen our analytical understanding of the former through a detailed examination of the latter thus promises to be of interest to readers from a variety of fields, ranging from economics and politics to the analysis of corporate strategy.

Stephen acknowledges that no single, parsimonious theory is adequate to explain the complex trajectory of the development of the European Union (EU). Indeed, he gives short shrift to many of the traditional theoretical approaches to European integration, particularly the neofunctionalist or spill-over theory long identified with Ernst Haas. Stephen offers instead an analytical framework based on an eclectic set of insights and propositions taken from well-known theories of regulation, policy formation, and institutional analysis. Its key elements are the perceived distribution of the costs and benefits of integration across auto firms and member states as well as the institutional arena in which policy initiatives are debated and decided. The meat of the book is the application of the framework to four key issue areas: setting common standards for permissible levels of auto emissions, reducing national government subsidies to auto firms, antitrust policy toward the auto sector, and developing an EU-wide voluntary export restraint (VER) system with Japan to replace the hodge-podge of national restraints.

Stephen's framework can produce very plausible explanations of policy outcomes. For example, in the case of auto emissions, different firms specialized in different propulsion technologies and were divided because the costs of potential regulations would fall more heavily on some firms than others. National governments were also divided, because some felt more environmental political pressure than others. Policy-making shifted to the European Parliament and eventually resulted in higher common emissions standards than most auto companies would have preferred. In the case of Japanese imports and transplants, the auto companies were united in their reluctance to accept complete openness to what they perceived as unbeatable (and perhaps unfair) competition. Some national governments were more liberal in theory than others, but none was willing or able to put together an open auto trade coalition in the face of determined industry opposition. Policymaking remained very informal, and the result was a variant of the European tradition of managed trade in the form of a complex Europe-wide VER with Japan, which was "an antiliberal outcome . . . imposed on consumers and importers" (p. 19).

In his cases the author makes good use of interviews with European auto industry officials, lobbyists, and Brussels

Eurocrats. He also draws on appropriate official documents from various EU institutions, some national documents, and a wide range of secondary literature about auto industry economics, politics, and corporate strategies. He displays an impressive knowledge of the kind of policymaking by informal understandings and insider deals among auto firms, national governments, and European officials on issues such as Japanese imports, which he labels as “typical of Brussels at its most opaque” (p. 134).

Occasionally, familiarity with the interests and positions of the various stakeholders and the desire to cast light on murky behind-closed-doors politics leads Stephen to the very edge of what his evidence will support. He resorts to assertions that might well be true but cannot be documented. His language then takes on a slippery, conditional air. For example: “Spain no doubt expected to be a beneficiary from an open investment regime. . . . The Japanese may even have quietly indicated as much, and EU structural funds were perhaps promised by the president of the Commission, Delors” (p. 133). Or: “Also, VW’s preferences for high emissions standards must have been somewhat moderated after the acquisition of small-car producer SEAT (in Spain) in 1986” (p. 89). This can be annoying, in that it requires the reader to parse his sentences very carefully. But it is hardly a fatal flaw, given the less than full disclosure of much of the politics surrounding the European auto industry in these years.

Stephen acknowledges that he has traded breadth of scope for depth of analysis. He notes that he makes little mention of potentially significant actors, such as labor unions, local and regional governments, consumer groups, and components manufacturers (p. 83). Except for a few brief references, the detailed coverage ends in the early 1990s. And there is no mention of the efforts, which were being discussed at both the national and European levels even in the 1980s, to limit the automobile’s effect on cities, to promote alternative modes of mobility such as urban transit and high-speed trains, and to increase fuel taxes and levy other kinds of fees and eco-charges on automobiles.

Within these self-imposed limits, most readers will not be disappointed by what Stephen has to tell us about how the integration pulse provided by the Single European Act affected European auto markets, auto producers, and both national and EU policymakers. Readers interested primarily in the European auto industry will find a wealth of detail and some fascinating interpretations of events. Those interested primarily in the process of cooperation, integration, and institution building will also be rewarded by some very interesting and important insights into how to think about the interaction of business and politics at the national and European levels in the late 1980s and early 1990s.

What Do We Know About War?. Edited by John A. Vasquez. Lanham, MD: Rowman & Littlefield, 2000. 420p. \$75.00 cloth, \$29.95 paper.

H. E. Goemans, *Duke University*

The new collection of essays edited by John A. Vasquez in *What Do We Know About War?* provides a useful overview of the quantitative literature on war. This book makes no claims to move the field forward significantly but, instead, offers seniors and first-year graduate students a good basic understanding of how statistical analyses have been used to explain the variation between war and peace. The book has significant strengths and weaknesses. Its strengths are the wide array of questions addressed and the attempt to provide a systematic discussion of the current state of the quantitative

knowledge on war; its weaknesses are the paucity of attention paid to new insights from the rational choice literature and their implications for the quantitative study of conflict.

The book is divided into four parts. The first offers an overview of the history, progress, and underlying issues of the Correlates of War (COW) and International Crisis Behavior (ICB) projects. The second examines factors that bring about war; the third, factors that promote peace; and the fourth and final part offers up some lessons and conclusions. The first part is particularly useful for undergraduates and first-year graduate students. In three chapters J. David Singer, Stuart A. Bremer, and Micahel Brecher, Patrick James, and Jonathan Wilkenfeld—prominent pioneers in the quantitative study of war—provide a good overview of the background, progress made, and questions that still need to be addressed by the COW and ICB projects. It is somewhat surprising to find that the contributors to this volume are strikingly modest in their claims about the success of the program. When Bremer assesses progress in attempts to answer the questions “Who fights whom, when, where, and why?” he feels compelled to “admit that this has not been a very successful endeavor” (p. 35). (See also Zeev Maoz’ caution in his chapter on alliances [pp. 112, 137].) Singer’s discussion of assumptions and some metatheoretical considerations offers good advice and food for thought for graduate students. In the third chapter, Brecher, James, and Wilkenfeld assess the progress of the ICB project and make some short comparisons of their findings with the COW findings. This is one of many instances where new insights from rational choice theory should have been discussed. James D. Fearon (“Signaling Versus the Balance of Power and Interests—An Empirical Test of a Crisis Bargaining Model,” *Journal of Conflict Resolution* 38 [June 1994]: 236–69) has argued that ex ante observable characteristics such as military power should play a fundamentally different role in situations before and during crises. Except in the excellent chapter by Jack Levy, fundamental problems such as selection effects and endogeneity are barely discussed. (To be fair, Paul K. Huth, Gary Goertz and Paul F. Diehl, and James Lee Ray briefly address some of these issues in their chapters.) This is regrettable since these issues are coming to the front of the field.

The second group of essays discusses factors that bring about war. These nine chapters offer a nice mix of new and emerging research agendas (or variables), such as the role of territory and enduring rivalries in international conflict, and more established research agendas, such as the role of alliances, capabilities, military buildups, and escalation. The chapters by Paul R. Hensel and Paul K. Huth are among the best in the book and form a particularly nice match. Hensel carefully and instructively unpeels several layers of the onion and offers several original empirical analyses on the relationship between territory and war. He finds that “[t]erritorial claims appear to be a leading source of militarized conflict and war, increasing both the likelihood [and] . . . severity levels of [militarized conflict]” (p. 81) While Hensel analyzes new data and proposes new hypotheses, Huth offers a rich and subtle discussion of theoretical explanations for the role of territory in conflict. The chapters on alliances by Zeev Maoz and Douglas M. Gibler consider a broad range of factors affecting how alliances can promote peace and war. The chapter by Maoz presents an especially thorough overview of the role alliances have played, with extensive tests and replications of the main propositions. Gibler proposes a reconceptualization and new typology of alliances and some initial tests. The two chapters, by Goertz and Diehl and by Frank Whelon Wayman, on rivalries offer only limited empirical evidence on the role of (enduring) rivalries but provide a good discussion of this emerging but relatively neglected research agenda.

Russell J. Leng's chapter on crisis escalation is a clear instance where the book achieves its goals. Since it is impossible to keep up with all the literature on conflict, I had missed Leng's earlier work on crisis escalation. He presents a very interesting typology of crises based on both realist (rationalist) and psychological factors. He finds that "[v]ariations in the crisis structure are strongly associated with whether the crisis escalates . . ." and "that any examination of dispute escalation that does not consider its dynamic character is incomplete" (pp. 247, 256). Daniel S. Geller's chapter on material capabilities unfortunately highlights this book's failure to examine the links between the existing quantitative research and more recent formal work. Thus, while Geller broadly discusses the role of status quo orientations, no attempt is made to evaluate the empirical work in light of recent extensive work on status quo orientations and war, as, for example, by Robert Powell (*In the Shadow of Power*, 1999).

Part III of the book examines factors that promote peace and includes a chapter on international norms by Raymond and a chapter on democracy and war by Ray. The chapter on international norms discusses mainly theoretical concerns about norms and their effects; little attention is paid to the rich empirical literature linking norms and the democratic peace. Ray examines the empirical literature that links democracy and war with a careful eye on potentially confounding variables and spurious correlations. His discussion does incorporate the contributions made by the formal literature.

Three chapters, by Levy, Manus I. Midlarsky, and Vasquez, summarize the findings presented in the preceding chapters and offer conclusions on the progress of what is referred to as the "scientific study of war." Although brief, Levy's chapter stands out in this book as an excellent primer for future empirical research. Levy discusses emerging issues and concerns in the study of war, paying attention to new insights from the formal literature and constructivist approaches. He discusses how these insights affect and impact the quantitative study of conflict. To name but two of these, Levy offers brief but insightful discussions of the poor efforts in much of the quantitative study of war to understand and incorporate interaction and selection effects fully.

Most chapters clearly point to a lack of good theories to explain the relationship of the author's preferred variable to the variation between war and peace, and so it is all the more surprising that so little attention is paid to the emerging formal literature. Overall, however, I would recommend this book to new graduate students. Some of the chapters are excellent starting points for students surveying the existing quantitative literature on international conflict and for students starting their own research. Regrettably, there is a distinct trend toward Balkanization in international relations. A not-to-be-underestimated quality of this book is that it makes it easier for those interested mainly in other research traditions to follow and understand current debates in the quantitative study of war.