POLITICAL THEORY

Sex Rights: The Oxford Amnesty Lectures 2002.
Edited by Nicholas Bamforth. New York: Oxford University Press, 2005. 320p. $27.50.

— Eileen Bresnahan, *Colorado College*

A collection of revised versions of lectures given in 2002 on the general topics of gender, sexuality, and human rights (with two additional commissioned pieces), this volume is in some ways what one would expect: a loosely connected group of essays most (but not all) of which discuss the relationship of gender and (other-than-normative) sexuality to human rights. As the cover photograph—of two men in dress clothes embracing, perhaps in the aftermath of a gay commitment ceremony—suggests, one unifying theme of the work is whether lesbian, gay, bisexual, and transgendered (LGBT) people's claim to be treated by society, the law, and the state with the same respect and protection as are heterosexual couples is, in fact, a human rights claim. However, readers who might be led by the packaging to expect the work as a whole to center on that consideration will be disappointed.

Only two authors, Judith Butler and Robert Wintemute, deal extensively and directly with LGBT rights. Butler, in an essay that is the book's most interesting and lucid contribution, considers human rights in terms of what it means to construct a livable world. Her meditation on the lives that society sees as grievable, and on the importance of sexuality, ecstasy, and fantasy to human life, reminds us that the commitments to human dignity that lie at the heart of what remains radical about liberalism can still inspire in an inhumane world. Wintemute, on the other hand, offers us a rather tedious essay that was already in some ways obsolete before the volume was published. His typology of LGBT rights worldwide sees such rights as mainly depending on claims to formal equality and against (invidious) discrimination, claims which should be intelligible to rational and secular societies everywhere. In making this point, Wintemute gestures toward the clear but often overlooked or misunderstood connection of LGBT rights to women's rights, themselves still a subject of contestation across the globe. Indeed, women's rights is, in fact, the most prominent topic in much of the book, especially as this relates to the conflict of the demands of local cultures for group autonomy and self-determination versus the universal claims of human rights to protect individuals from discrimination and oppression.

While evaluations of what is often called "cultural relativism" are certainly germane to LGBT rights, it is not in those terms that the issue is here mainly discussed. Susan Moller Okin, in another of the book's best essays, analyzes the progress made toward achieving women's rights in the twentieth century, seeing the rising of cultural claims against women's equality as one of the "steps back" that women have faced (the other being a deteriorating economic situation in many countries due to Western lenders' demands for structural adjustment policies). The first of the commissioned essays, by Rajeswari Sunder Rajan, also addresses this problem, though from a different perspective, arguing that it is better to "sidestep" an analysis of women's traditional subordination in terms of a "conflict of rights," focusing instead on "the *values and interests* rights are meant to protect." In her view, it is better to "privileg(e) Shari'a" than to try to achieve "liberal divorce laws," "respecting the 'cultural, religious and historical contexts' in which the interests and values take shape" (pp. 135–36). Of course, she does not tell us what to do with the fact that women worldwide are still facing regimes that do not value them or their interests in ways that can support meaningful equality. One must wonder, as well, what process allowed hers to be the only essay in the book to escape appearing with an introduction, which might have forced her to be more clear in what she means by this.

Yet another of the volume's main concerns is the reality of the often horrific violence that is routinely inflicted on sexual transgressors of all sorts, be they heterosexual women who flout patriarchal norms, men who engage in anal sex, or any others who refuse to reliably conform to the rigid prescriptions of traditional gender. Rose George's essay, "Share a Spliff, Share a Girl—Same Difference: The Unpleasant Reality of Gang Rape," confronts this issue in the French suburbs, while Alan Sinfield discusses another face of the rapist in an analysis of Shakespeare's *Measure for Measure* (and does make some points about gay male sexuality, but not of the sort that are particularly revealing to a consideration of human rights, and also seems a bit naive about the relationship of desire to reality). Finally, the volume concludes with Maria Warner's essay on public apologies by authority figures, a rather puzzling piece that pronounces such performances "feminine," indeed "'a girly thing'" (p. 234), but does not ever tell us how its analysis is relevant to the human rights of women or anybody else.

In sum, this book is a mixed bag, and rather irritatingly so. While many of the essays are worthy in themselves, their gathering in a book entitled *Sex Rights,* especially given the cover attached, amounts to something of a "bait and switch." Readers interested specifically in LGBT and human rights issues can certainly find better resources, or at least ones more to the point. One must also wonder why, when soliciting new work for such a volume, the editor would seek an article about gang rape that fails both on its own terms—in that it tells us little about who these people are who are raping women or who are the women being raped, beyond their place of residence—and that also fails to deal with all the issues of sexual violence that affect LGBT people. If the point of soliciting additional articles for such a volume is to fill in some

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of the gaps, what gaps exactly was this journalistic and perhaps subtly racist essay meant to bridge? And then, what happened to the lecture delivered by the usually fascinating Jeanette Winterson? The editor explains that the volume contains "revised versions of five of the six lectures given" because Winterson "did not submit a written version" (p. x). As I would write to a student, "And . . . ?" These lapses, combined with the missing Rajan introduction, and the high price of $27.50 for a book that looks and feels as if it will physically fall apart in a day or two, leave me wondering what is going on here.

Democracy's Literature: Politics and Fiction in America. Edited by Patrick J. Deneen and Joseph Romance. Lanham, MD: Rowman & Littlefield, 2005. 248p. $80 cloth, $29.95 paper.

Political Philosophy Comes to Rick's: *Casablanca* and American Civic Culture. Edited by James F. Pontuso. Lanham, MD: Lexington Books, 2005. 208p. $65.00 cloth, $26.95 paper.

— Henry T. Edmondson III, *Georgia College*

Democracy's Literature is the most recent foray by political philosophers to save literature from itself, or at least to save good books from Departments of English. At the same time, as coeditor Joseph Romance suggests in the conclusion to his own essay, such an undertaking throws a lifeline to narrow-minded political scientists, urging them—if I may paraphrase Flannery O'Connor—to meet political perplexities with an examination of conscience rather than an examination of statistics. And it is this political examination of conscience that literature will often facilitate.

These essays, as a group, ably demonstrate the way in which good books improve our understanding of politics. For example, the most frequent theme in this collection is the tangled political dilemmas that America's tragic racial history poses to citizens and leaders alike, and the need for political efforts to be grounded in an appreciation of the transcendent dimension of political and personal life.

Not only does Romance's fine insightful essay on William Faulkner's *Intruder in the Dust* help us to understand Faulkner's contribution to an understanding of our race relations, but the essay is also balanced between an exegesis of the novel and an analysis of the political problem so as to provide a model of just what this genre of political writing should be. Wilson Carey McWilliams illuminates James Baldwin's contribution to the race problem by explaining that Baldwin would inspire us through his literature to approach such conundrums with "faith," and by insisting that in spite of his angry politics, he nonetheless wrote out of a love for his country.

Pamela Jensen's discussion of Ralph Ellison's prose and fiction is equally perspicacious in demonstrating the complexities (and promises) of our national quest for equality and unity in America. Both Romance and Jensen remind us that if citizens are to change their country for the better, they must love it—not hate it. Hate multiplies sins; love covers a multitude of the same. Less known to readers is the focus of Lawrie Balfour's essay, namely, Octavia Butler's 1979 novel *Kindred.* Balfour's chapter, though, convinces us that Butler deserves her place, that her fiction enables us to accommodate ourselves appropriately to the "memory" of slavery, which if forgotten, weakens our resolve to deal today with its consequences.

These essays, however, are not confined to the theme of slavery and racism. In his insightful essay on Flannery O'Connor's biting short story "Good Country People," Peter Lawler reminds us that the only sure antidote to the prevalent nihilism of our day is intelligent faith. Smug, arrogant secularism is simply too vulnerable to philosophical currents that cannot be understood—and withstood—without reference to something beyond ourselves.

Paul Cantor cleverly turns the tables on anti-Western, postcolonial literary criticism by arguing that Mark Twain's *A Connecticut Yankee in King Arthur's Court,* perhaps the very first "postcolonial" novel, is a constructive criticism of the Western tradition, written from *within* the Western tradition itself. Perhaps most insightful is his suggestion that the child becomes father to the parent insofar as eighteenth- and nineteenth-century America inspired political reform in England.

Catherine Zuckert's chapter on Tom Sawyer takes its cue from Twain's own suggestion that his creation, once grown up, might occupy the same desk as Teddy Roosevelt. Her explanation of Sawyer's political education, however, concludes that we must resign ourselves not to expect disinterested political leaders. On the contrary, as Lincoln warns in his speech to the Young Men's Lyceum, the country will never be relieved of the worry of self-interested political ambition, a lesson Zuckert draws from Twain's ironic novel.

Moving even farther away from recognized "canons" of literature, the last two chapters deal, respectively, with the graduation speeches of acclaimed novelist Kurt Vonnegut and the contemporary dystopian novel *White Noise* (1985). Since this collection is firmly anchored by its essays on canonical literature, there is room left for these final two essays, even if the literature they examine is not "classic." They are worth reading in their own right; what is more, their addition puts the lie to the charge that the Western canon is static and reactionary.

Readers (and publishers) have become less patient than they once were with edited collections, having tired of the inevitable variability of style and quality that even good editors cannot overcome. *Democracy's Literature,* though, is worth the reader's effort, given its thematic unity and talented contributors. The editors make a small misstep in their introduction. They quote Laura Bush's response to the White House boycott by several literary scholars who,

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troubled by the war in Iraq, spurned the president's invitation to a gathering in honor of literature. The first lady responded, "There's nothing political about literature." Patrick Deneen and Romance protest that "nothing could be further from the truth." Although they attempt to qualify their disagreement with the first lady, Laura Bush is exactly right: Good literature is never "political" in the sense of its being pretexts for partisan politics. On the other hand, time-tested literature is profoundly political, in that it speaks to our perennial democratic challenges with insight, elegance, and truth—as this collection so expertly demonstrates.

Two of the authors in the Deneen and Romance volume (Cantor and Lawler) join nine others in *Political Philosophy Comes To Rick's:* Casablanca *and American Civic Culture,* an imaginative text that treats cinema in a manner befitting its status, namely, as the new literary genre. Cantor's essay makes the text worth buying, Lawler's confirms the wisdom of the purchase, and several of the other essays make as strong a case as can be made that this classic film is a worthy study by historians, philosophers, and political philosophers. The immensely popular classic, second only to *Citizen Kane,* seems an unlikely inspiration for scholarly activity. Indeed, Paul Cantor warns that popular culture should be taken seriously—but not too seriously. Though at times uneven in style from one chapter to another, the essays as a whole are seriously undertaken by capable scholars, and the work thus deserves a place on the shelf of political scientists, movie buffs, and anyone else seeking serious entertainment. Even if *Casablanca,* at its inception, was only meant to entertain and return a handsome profit, James Pontuso convincingly argues that those who produced it have provided entertainment that tells us something about the "permanent passions, interests, duties, and questions inherent to the human condition" (p. 3).

The Headless Republic: Sacrificial Violence in Modern French Thought. By Jesse Goldhammer. Ithaca, NY: Cornell University Press, 2005. 218p. $45.00.

— Graeme Garrard, *Cardiff University, UK*

The problem of how to found a political regime is one of the oldest and most vexed in the history of Western thought. The founding act has often been drenched in blood because certain forms of violence actually help to foster political foundation. This is the illiberal starting point of Jesse Goldhammer's study of the French discourse on sacrificial violence, a theme that he rightly describes as "anathema to mainstream political theory" (p. x), which tends to divert its glance from what it sees as unseemly anachronisms like this. Undaunted, Goldhammer bravely plunges in, turning to 1790s France and the theorizing about sacrifice that it spawned for insight into the violence that he claims is "necessary for political beginnings" (p. 1). While the French revolutionaries engaged in "sacrificial practices and interpretations" such as the beheading of Louis XVI, they never actually developed a *theory* of sacrifice. That was the contribution of notorious postrevolutionary writers, such as Joseph de Maistre, Georges Sorel, and Georges Bataille, who well understood that violent sacrifice "facilitates the process of conferring moral legitimacy to political power and setting boundaries for political identity" (p. 192).

Sacrifice is a form of violence with a useful capacity for setting things apart from the ordinary and profane, thereby endowing them with a sense of the sacred. Goldhammer argues that because humans "are more inclined to act morally or to obey when the origin of the law or religious command is hallowed" (p. 4), sacrificial violence has been employed throughout history by the politically astute to render secular laws legitimate. Realizing this, the revolutionaries appropriated two traditions of sacrifice—from ancient Rome and early Christianity—when violently founding and legitimizing a new republican regime in France. By gathering French people together "as witnesses to violent, sublime spectacles" (p. 47), such as the ritual beheading of the king, the nation was to be cleansed and purified and sovereignty symbolically transferred from him to the people.

Goldhammer argues that the reactionary Catholic writer Joseph de Maistre was the first French intellectual to offer a comprehensive theory of sacrifice, a fact that we are told no fewer than five times, which is typical of the author's occasional tendency towards repetition. Maistre believed that the revolutionaries unleashed a fury of divinely inspired destruction that both punished France for its sins and morally purified a decadent nation. The Terror was therefore salutary because it restored the social and political equilibrium upset by the regicide. For Maistre, such violence, while morally restorative, is powerless to create anything new, contrary to the beliefs of the revolutionaries. The anarchist Georges Sorel shared Maistre's belief in the moral utility of violent sacrifice. He also agreed with Maistre that violence is incapable of founding states. However, some forms of proletarian bloodshed can facilitate moral reformation and inspire revolutionary action by stirring the emotions. Sorel believed that this was urgently needed to save the working classes of France from bourgeois decadence. Unlike most of his socialist contemporaries, he was revolted by the state violence initiated by the Jacobins, which the authoritarian Maistre grudgingly admired. Instead, he believed that the inspirational example of a small number of martyred workers modeling themselves on early "primitive Christianity" would greatly amplify the impact of proletarian violence, thereby "preventing the need for widespread incidences of bloodshed" (p. 116).

According to the "excremental philosopher" Georges Bataille, violent sacrifice is not only incapable of founding new political regimes but cannot even promote moral

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regeneration, as Maistre and Sorel believed that it could. The French revolutionary sacrifice of the king led to a "permanent destabilisation of political power" (p. 156) that rendered all subsequent politics "impossible." The very "uselessness" of such violence is its greatest virtue for Bataille, since he believed that bloodshed can foster the formation of an "acephalic" (headless), self-subverting "community" bound together by "the shared experience of unrecoverable violent loss" (p. 163). So Bataille embraced a form of "unproductive violence" that would give birth to "a metapolitical community paradoxically defined by its permanent lack of foundation" (p. 11).

Goldhammer's book is refreshingly unorthodox in its subject matter and admirably clear in the exposition of its themes and ideas, with mercifully little academic jargon. It introduces the reader to a disturbing and fascinating subject that few political theorists pay any heed to these days. It is essentially a work of intellectual history whose aim is expository rather than argumentative. This is made clear at the outset, where the author states that he will address three questions, all of which are historical: "Why did the French revolutionaries recuperate ancient concepts of sacrificial violence? How did this recuperation inaugurate a modern theoretical debate about the role of sacrifice in founding politics? Finally, how do Maistre, Sorel and Bataille, writing in reaction to the violent, sacrificial founding of the French Republic, reconceptualize the relationship between sacrificial bloodshed and political instauration?" (p. 2).

The weakest parts of the book (of which there are few) come when Goldhammer switches from exposition to argument. For example, he criticizes the French revolutionaries, Maistre, Sorel, and the postwar Bataille for believing that certain forms of violence are morally regenerative and politically useful. Yet he nowhere explains in any depth why he thinks this belief is wrong. It amounts to a mere counterassertion. Also, in his conclusion, he claims that violent sacrifice "remains part of the modern political condition" because political authority "requires an element of sacredness" and, in the absence of gods, "modern political actors must find alternative sources of the sacred," which they often find in bloody sacrifice. Yet not until two pages from the end of his book does Goldhammer actually take up the question of how this discourse on violence might apply in contemporary circumstances, and his answer is very sketchy and superficial. As a work of intellectual history, *The Headless Republic* is both highly engaging and an important contribution to a neglected subject in the history of ideas. The author's occasional surrender to the temptation to go beyond this only detracts from his accomplishment.

This book suffers from three relatively minor defects. First, we are told in passing that Sorel read Maistre. But we are not told what he read or what impact (if any) it had on him. Nor are we told if Bataille read either Maistre or Sorel and, if he did, how they influenced his views on violence. While these are not crucial questions, they are important and interesting, and their absence is disappointing. Secondly, Goldhammer describes the works of Maistre, Sorel, and Bataille as forming "a distinctive, modern tradition of thought" (p. 2) on the theme of sacrificial violence. It is not clear whether three thinkers can constitute a tradition and why the Marquis de Sade and Michel Foucault are not included in this tradition (very little is said about either), given their interest in violence. Finally, Goldhammer states that "Maistre came to reject every conceivable facet of the Revolution" (p. 71). Yet elsewhere he rightly asserts that Maistre interpreted the violence of the revolution in providential terms, as a divinely inspired corrective to the decadence and hubris of eighteenth-century France. Maistre was therefore far from rejecting "every conceivable facet" of an event that he saw as ultimately guided by the hand of God. Defects aside, this work is an insightful exploration of the significance and power of sacrificial violence in modern politics.

Democracy and America's War on Terror. By Robert L. Ivie. Tuscaloosa: University of Alabama Press, 2005. 276p. $38.75 cloth, $24.95 paper.

In the Name of Terrorism: Presidents on Political Violence in the Post–World War II Era. By Carol K. Winkler. Albany: State University of New York Press, 2005. 260p. $65.00.

— Michael J. Thompson, *William Paterson University*

Two recent books on the relation between the rhetoric of political discourse and American democratic politics explore the ways that language can shape political outcomes. Political culture has been a woolly subject for political scientists, specifically because explanatory models of political culture tend to oppose the more predominant institutional models of explanation. But both books convincingly make the claim that political culture can and is shaped by the rhetoric of political elites and that this has a deep impact on the ways in which political phenomena are interpreted and understood in a collective sense. Both analyze political discourse and seek to show how it impacts, shapes, and can even predict certain outcomes in political life.

Carol Winkler frames her analysis of presidential discourse on terrorism in the post–World War II era by making a distinction between what she terms a "crime narrative," which frames terrorist acts as the acts of isolated individuals, and a "war narrative," which casts terrorism as an ideological struggle between two opposing value-oriented political extremes. Both narratives become central in understanding the ways that presidential discourse has affected the formulation of policy and the ways that Americans themselves understand their relation to the outside world. Presidential discourse on

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terrorism can be broken down into three different, but interdependent, levels of analysis: "labels," terms through which the public can associate an act or event "with a particular ideological orientation" (p. 8); "narratives," which frame the interpretations of events in a particular way; and "ideographs" which are "foundational values that serve as a basis of a culture's identity" (p. 12).

Throughout the course of *In the Name of Terrorism,* Winkler shows—by close readings of the public papers of the various presidents, their speeches and addresses, and other internal memos and documents—how the discourse of terrorism changes during different presidential administrations. During the Vietnam War, John Kennedy, Lyndon Johnson, and Richard Nixon all utilized what Winkler refers to as the "Cold War narrative" to understand and communicate about terrorist acts. This narrative viewed terrorist acts as not simply acts of isolated individuals but, rather, acts associated with the ideological conflict between communism and American democracy. This discourse changed with the Carter administration and its response to the Iranian hostage crisis. In order to contain a broader conflict with the Muslim world, Winkler argues, the Carter administration labeled the seizing of American hostages in Iran as a terrorist act, but chose to frame it within a "crime narrative" that focused attention on the specific acts of the terrorists, thereby limiting the scope of the implications of the act.

It is under Reagan's presidency, however, that Winkler sees terrorism turn into an ideograph, one that will have profound implications for the remainder of her analysis. Reagan's rhetorical response to the increase in terrorism during his administration—the linkage of drug traffickers with terrorists—was to reframe the idea of terrorists as neither state actors nor misunderstood freedom fighters, but as "dangerous enemies who plagued American society" (p. 75). This shift in the framing of the terrorist label effected a shift from the "crime narrative" of the Carter administration to the broader ideological terrain of American principles and values and all those who would seek to destroy it. Here, terrorism becomes an ideograph: an entrenched signifier with deep ideological resonance and "a linguistic resource available for all who would follow" (p. 95).

This has grave consequences for the present, as Winkler points out. In the end, she finds that Democratic presidents employ "crime narratives" and thereby seek to confine the scope of conflict, whereas Republican ones tend to utilize "war narratives" that enlarge the scope of conflict beyond the actors themselves into broader political and sometimes—as in the current administration—civilizational terms. Democratic presidents have tended, as a result, to leave themselves open to attacks of being too weak on terrorism. Winkler's excellent study shows how presidential rhetoric changes in response to objective events, and the ways that different presidential ideologies shape those events through the kinds of narrative they employ.

This relation between rhetoric and American political self-consciousness is more deeply probed by Robert Ivie in *Democracy and America's War on Terror.* Ivie's task is to examine what he sees as the degeneration of political rhetoric in American political culture and the erosion of democratic life, and also to construct a new normative argument for deepening democratic politics through a shift in the style of political rhetoric. Unlike Winkler, who uses the analysis of presidential language and discourse as an analytical tool, Ivie has a much broader and more provocative claim: "Our choices are always between one kind of rhetoric and another" (p. 4). This signifies his methodological as well as theoretical perspective. Ivie pursues two separate but interrelated themes: first, the identification of the way that American democratic culture has eroded into a "republic of fear," or what he also refers to as America's "democratic deficit"; and second, a vision of how to enrich "democratic culture and practice."

Ivie's central claim is that the rhetoric on terrorism in America reveals our deeper deficit of democratic culture. Only by conceptualizing foreign and domestic "Others" are we able to construct a conception of national identity. Because of the particular forms of rhetoric available to us—grounded deep in the recesses of American politics—we have been forced into a veritable rhetorical cul de sac wherein we are caught in the simplistic, rigid dualism between selves and others, Americans and terrorists, civilized and barbarians. It has given rise to the excesses of nationalism and stubborn unilateralism, and it has not only provided ideological weaponry to elites but also distorted the very ways that Americans understand themselves and, more importantly for Ivie, done so with a restricted rhetorical strategy that constricts newer forms of democratic politics. The "rhetoric of evil," he claims, becomes predominant over a more "flexible and robust conception of democratic practice" (p. 5).

Ivie traces four interconnected points of argument throughout the text. The first is that America's idea of deliberative democracy—one "reserved for experts and privileged political leaders" (p. 5)—results in what he calls "demophobia," a fear of the public and of public reason. A second, and related, point is that this results in the rhetorical symbol of a "distempered demos" in American political culture, where Americans see the public as "prone to popular rage and fits of passion, convulsions of factionalism that poison public deliberations" (p. 6). As a result, America's republican institutions were founded on the basis of the "fiction of representation," rather than on a more expansive form of participatory democracy. This rigid form of political culture leads to a third problem, that of a fear of any kind of political ideology outside liberal democracy

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and, therefore, to a discourse of war and peace. Universal peace can only be achieved, Ivie claims, "by a desire to expand the domain of liberal democracy in order to contain the forces of disorder" (p. 6). This leads to the fourth and final problem that plagues contemporary American democracy: the creation of a "republic of fear" produced by the previous three problems in American democratic culture, which have now led us to a situation of "open-ended war on international terrorism" (p. 6).

Few would deny that American political rhetoric has often veered toward a populist discourse of fear of terrorism and even, in some respects, an outright racism with respect to certain ethnic and religious groups, both domestically and internationally. But unlike Winkler, whose analysis of presidential rhetoric is clearly linked to her theoretical claims, Ivie lacks sufficient empirical evidence for the broad claims he puts forth. It could be that reading presidential speeches and addresses can give us a glimpse into the messages of the current administration, but to expand this into a broader claim about American political culture is a questionable strategy. History is a good place to look for counterfactuals, especially when we consider the relative *lack* of xenophobia after the 9/11 attacks in contradistinction to previous eras in American history. (I am thinking here of the Chinese Exclusion Act, the Restriction Acts of 1921 and 1924, the Anti-Japanese Alien Land Laws in California during the same period, etc.) Viewed long range, Ivie's claims to the damaging prevalence of the "Other" seems to have weakened as liberal democracy deepened in American history, not the reverse. In addition, the extent to which Americans actually favor aggressive foreign policy is also not clear from most political polls and surveys. The case he makes is hardly cut and dried if one scrutinizes the empirical data; and this should not be a surprise, since his emphasis is wholly on an analysis of discourse (or "rhetoric") at the expense of institutions, interests, and the ways that—in terms reminiscent of E. E. Schattschneider—elites can pressure certain policy choices that do not harmonize with the broad strokes of public opinion.

Ultimately, Ivie's is not an empirical case study. His intention is to be polemical, to call for a reinvention of democratic thought and practice. To his credit, this is dearly needed, but the central thrust is a critique of the institution and ethos of deliberative, liberal democracy, one formed around republican institutions of representative government. It is an argument that eschews institutional concerns and reduces its theoretical claims to "rhetoric." This is an all-too-familiar argument from the not-yet-dusty pages of academic postmodernism. The argument stems from Nietzsche's argument that there exists a tension between "rhetoric" and "reason"; and that rationality, in the final analysis—reversing Plato's argument in his *Gorgias* that philosophy, or reason, was in fact superior to rhetoric—is nothing short of an ideological mask for elite power.

America's democratic culture can be renewed, Ivie claims, by pursuing new rhetorical strategies, opened up to more diversity and expanded to include new forms of difference by embracing an explicitly postmodern notion that emphasizes the decenteredness of liberal democracy as a totalizing narrative in American political culture. In this postmodernist reading, liberal, representative democracy is a hegemonic ideology that, through its dominance in American political culture, ultimately leads to the exclusion of opposing argument, a facile degeneration of political discourse, and a stultifying conformity of thought and action that erodes democratic principles and practice. Of course, one need not look toward postmodernism for such an argument; one can simply recall Louis Hartz's thesis in his 1955 *Liberal Tradition in America* that as a result of America's "liberal consensus," times of crisis would lead American liberalism into its political opposite. What institutional form this will take, Ivie does not tell us. Political culture is important, but institutions are as well, and it is questionable whether one can be fruitfully discussed without the other. We are left with the option of an open-ended invention and reinvention of political rhetoric.

Both Winkler and Ivie analyze the rhetorical framing of politics. Winkler's analysis is an insightful case study of the "evolution" of the discourse of terrorism in the post–World War II era, and convincingly shows how discourse can shape the justifications for certain policies taken by different administrations. Ivie's broad claims are provocative and lead us to areas of concern that are insufficiently debated. Each book shows how political scientists can learn a great deal from an examination of political discourse and the way that political culture can constrain, or perhaps even enhance, different aspects of political life and the creation of policy.

Scientific Values and Civic Virtues. Edited by Noretta Koertge. New York: Oxford University Press, 2005. 256p. $70.00 cloth, $24.95 paper.

— Ed Portis, *Texas A&M University*

This interesting collection of essays is dedicated to the proposition that the values essential for the functioning, indeed even the existence, of a scientific community are by and large the very values necessary for the sustenance and optimal functioning of a democratic society. To this end, the book is divided into three sections: The first is devoted to explicating the values entailed in any real scientific community and to exploring the historical nexus between the development of civil society and science; the second to illustrating the role of these values in science through a number of case studies; and the third to the examination and critique of

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contemporary anti- or pseudoscientific cultural movements as threats not only to science but to civil society itself.

To characterize this book as dedicated to a proposition suggests that the proposition itself is largely unchallenged. Although a number of the contributors in the first two sections raise some interesting questions concerning its adequacy, at least by implication, the authors of the third accept it as a given. Unfortunately, the result is a distinct polemical tenor to a number of these essays. This is especially true of those by Keith Parsons, whose targets are feminist critics of objectivity and fundamentalist critics of evolution, and Philip A. Sullivan, who takes aim at postmodernist scholarship. These are certainly well-informed essays, but they are no more than critical reports, offering few if any genuine arguments. Much the same could be said of the contribution by Barbara Forest and Paul R. Gross on the advocates for intelligent design as a scientific alternative to natural selection, but their focus on a specific, organized effort and its pretensions to scientific legitimacy render their essay an interesting exposé, rather than a mere jeremiad.

Many readers may find the essays of the third section more interesting than those of the first two because they are focused on contemporary events, but I personally found the preceding essays, without exception, both intelligent and intellectually significant. However, I question the editor's claim (p. 155) that they collectively present "a positive appraisal of the values that animate scientific research and an optimistic picture of how they reinforce the civic virtues necessary for a liberal democratic society." This is certainly the case for the chapter by Gerald Holton, comparing physicists Percy Bridgman and Niels Bohr, and also for that by Allan Franklin illustrating the weight of evidence in scientific disputes. Yet Michael Ruse's essay on the necessity of trust in science and its role in scientific disputes, amply illustrated by examples from evolutionary biology, would seem to have disturbing implications for those who would find a democratic ethos in the scientific community. In brief, Ruse describes a tendency for persistent scientific disputes to become personalized as scientists call into question the integrity or qualifications of opponents who resist evidence taken to be conclusive. Although scientific discourse is governed by norms of openness and rationality, it is a discourse confined to the qualified, and as such it would seem to be aristocratic rather than democratic. Not everyone can be a citizen of the scientific community, and once attained, citizenship is never assured.

The three historical chapters of the first section provide scant support for the idea that scientific values were important in the development of either liberal democratic regimes or liberal democratic ideas. Edward Grant traces the growth in the prestige of reason, as opposed to mere faith, in the Middle Ages, as well as the beginning of a tradition of dispute and challenge to authority. Both of these developments, however, involve the influence of Aristotle, usually considered the bête noire of modern science, rather than one of its pioneers. As Grant points out (p. 40), "it is unclear if there were any scientists to uphold scientific values" during this period. Rose-Mary Sargent finds that a concern for the public good did indeed animate a number of the real pioneers in the development of the scientific community, but also makes clear that such factors had largely disappeared by the time liberal democratic societies became consolidated. Finally, John C. Moore's assessment effectively captures the conclusion I would draw from these essays: "Although modern science has helped to promote civic virtue in modest, indirect ways, it has been the beneficiary of civic virtue more than its source" (p. 60).

With a single exception, these authors have a much more sophisticated understanding of science than of democracy. This is not surprising since, with a single exception, the contributors are scientists, historians, or philosophers rather than political scientists. It is also not surprising that the infrequent and brief discussions of democracy in this book clearly indicate that the authors see the aim of politics as the discovery of a true, rational public interest (pp. 3, 159, 174). I suspect that if a democratic ethos is to be found in scientific endeavor, this is a necessary assumption, since the values necessary to the community of science are not based on justice or communal commitment but are instrumental to the pursuit of truth. Actual politics, however, is not animated by a search for truth, but rather by a commitment to realize goals that are assumed to be valid, valuable, or true. Actual politics is essentially a form of struggle rather than deliberation. Consequently, there is a normative deficit, so to speak, in this book. It is not obvious that the public good is simply something to be discovered. Nor is it obvious that deliberation is capable of generating sufficient consensus to sustain the public authority necessary for collective decisions.

As the sole political scientist represented in this volume, and as a sophisticated theorist of deliberative democracy, Steven M. DeLue knows what is obvious and what is not. He argues that, despite Rawls, public commitment to constitutional democracy can only be assured if it is grounded on the Kantian principle of personal autonomy and on the respect for other persons that follows from this principle. DeLue hopes that a general belief in the practical value of science will encourage those incapable or unlikely to work their way through Kant to support the values necessary for scientific success, which significantly overlap with those of constitutional democracy. Perhaps so, but this assumes that science needs constitutional democracy, or if not, that the populace can be deluded that it does. So perhaps DeLue is also positing rather than arguing the proposition to which this book is dedicated.

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Politics of Nature: How to Bring the Sciences into Democracy. By Bruno Latour. Translated by Catherine Porter. Cambridge, MA: Harvard University Press, 2004. 320p. $55.00 cloth, $24.95 paper.

— Nancy S. Love, *Pennsylvania State University*

"What is to be done with political ecology? Nothing. What is to be done? Political ecology!" Bruno Latour begins *The Politics Of Nature* with this provocative statement (p. 1). What follows is a creative reconceptualization of the relationship among nature, science, and politics with profound implications for the future of ecological change, traditional right/left ideologies, and modern political processes, including globalization.

Since political ecology does not yet exist conceptually, Latour's project is best understood as the act of its production. This production process partly involves the rediscovery of ancient meanings. The author defines "political ecology" as "the right way to compose a common world, the kind of world the Greeks called a *cosmos"* (p. 8). Multiculturalism and, more recently, multinaturalism make it possible for politics and the sciences to work together today to articulate the common world in radically new ways. His argument is motivated by a concern that humanity might miss the current moment, might refuse to slow down enough to reflect on its possible futures, and might instead rush from twentieth-century totalitarianism to twenty-first-century globalization. According to Latour, both phenomena involve similar processes of exclusion; they create collectives that prematurely juxtapose a universal humanity to an external nature.

Latour develops his argument through a discussion of successive dichotomies—nature/culture, subject/object, and fact/value—each informed by a more fundamental distinction between human beings and nonhumans. In Chapter 1, he employs Plato's Cave allegory to describe how contemporary constitutions create a public life with two houses: science (as distinct from various sciences) and politics. As the object of science, nature remains outside politics and simultaneously becomes its essential reality and toxic dump. Only when humans abandon the concept of nature does Latour think we can begin to consider what matters most, that is, the multitude of nonhumans and our associations with them. Political ecology directs humans toward the urgent task of re-presenting interactions between humans and nonhumans. It supports the creation of new political institutions that replace the old "two-house collective" with a "collective without outside recourse" (p. 37). In Chapter 2, the author argues that bicameral constitutionalism has reinforced a combination of multiculturalism with mononaturalism. He proposes replacing this "politics of subjects" and "science of objects" with "an assembly of beings *capable of speaking*" (pp. 61–62). The various natural and social scientific disciplines are charged with finding ways to make formerly mute objects speak. He asks: *"How can we go about getting those in whose name we speak to speak for themselves?"* (p. 70).

In Chapter 3, Latour begins to outline the terms and institutions of his new separation of powers. In place of the fact/value distinction, itself an artifact of prior distinctions between nature/culture and object/subject, he proposes a new categorical imperative: *"Thou shalt not simplify the number of propositions to be taken into account in the discussion"* (p. 104). ("Proposition" or "well-articulated actor" is Latour's term for "citizen," which transcends the latter's human connotations.) In his new Constitution, the first house has the power to take into account, to ask—and answer—the question: "How many are we?" The second house has the power to order society in response to the question: "Can we live together?" (p. 109). Only after nature is denaturalized and people are resocialized can we begin consciously to form a common society with an explicit process of representation and sense of reality.

Latour names this process of self-reflective association "composition," and in Chapter 4, he describes the various skills different disciplines contribute to it. Most important, he urges politicians, scientists, and moralists to work together to produce the voices of a multitude and to articulate a "we." Once the upper house produces this "we," then the lower house can create the world—the cosmos—we hold in common. According to the author, the supposed rationality as well as the continuing violence of modern politics short-circuits the learning processes required for the composition of society. By doing so, modernity temporarily (he hopes) "paralyzes" the body politic. Political ecology offers an antidote, a multinaturalism that refuses the metaphysical outside and the shadowy inside of the Cave, bringing nonhumans and humans together in a fully democratic politics.

As even this brief overview suggests, Latour's argument is as complex as it is creative. In addition to Plato and Aristotle, he alludes to Hobbes, Rousseau, Kant, Lenin, Jürgen Habermas, and, among the most intriguing, Charles Fourier. Latour supports his challenging text with a glossary of new terminology and a brief summary "for Readers in a Hurry," for those who cannot afford (or think they cannot) the slowness reflection requires. Although I am reluctant to jump to critique, I will offer some reflections of my own, specifically, on the production of voices.

As Latour quite rightly claims, "the modernist Constitution never had the courage to 'motivate its decisions to reject'" (p. 169). As a corollary, it remains largely unaware of what Iris Young (2000) in *Inclusion and Democracy* labels processes of internal and external exclusion. To his credit, Latour takes seriously Habermas's stricture that everyone affected by a decision must participate in making it. What I question is his claim that "without the work of production of voices, there would no longer be voices at all" (p. 145). He seems to assume that voices are only produced—and productive—as speakers and

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writers. But what of active, reflective listening? What of the sounds—cries and screams of suffering (as well as joy)—already produced by the all-too-human voices of oppressed groups? Latour too easily relegates such calls for justice to the identity politics of cultural recognition. However, when nonhuman nature is exteriorized, so are the human beings most closely associated with it. He admits: "Once the question of the number of collectives is reopened, the Other is going to change form. . . . [A]lterity is going to change: it too has become *altered"* (p. 209). Yet what if the cosmos is already en-chanted? Sonorous? (Jane Bennett, *The Enchantment of Modern Life: Attachments, Crossings, and Ethics,* 2001). Although Latour acknowledges his logocentrism, he presents it as an artifact of his disciplinary location. What matters, he claims, is what follows from political ecology. I would also ask, what follows whom? *Man*-kind? What I sought was greater attention to how political ecology alters the propositions of dehumanized humans, as well as denaturalized nonhumans.

Perfection and Disharmony in the Thought of Jean-Jacques Rousseau. By Jonathan Marks. New York: Cambridge University Press, 2005. 200p. $65.00.

— Emily Hauptmann, *Western Michigan University*

Many of us feel we know Rousseau, perhaps better than we do any other modern political thinker. For this we have Rousseau himself to thank; his *Confessions,* though often opaque and perplexing, still give us a vivid sense of the person who wrote them—ardent, dramatic, petulant, grandiose. Anyone who has read Rousseau's own account of his life will surely be puzzled by the cool, muted tones Jonathan Marks uses to portray him: Marks's Rousseau is a sober, measured thinker who tempers his most radical ideas with a sharp awareness of human limitations. This portrait, so deliberately drawn, looks the way it does for several reasons.

First, Marks sets his reading of Rousseau against only a few other interpretations, although he is clearly aware of the much wider extent of interpretive work on him. In contrasting his reading of Rousseau primarily to Kantian and communitarian ones, Marks purposefully fashions his sober subject to address what he believes each of these groups of interpreters misses. Second, for as much as Marks works to set his reading of Rousseau apart from others, he is clearly working within an interpretive tradition himself. He acknowledges an especially deep debt to Allan Bloom (pp. vii, 156–57) and, like Bloom (*Introduction to Emile* or *On Education,* 1979), draws heavily on *Emile* to discern Rousseau's most considered views (pp. 38–51, 65–70, 113–17).

The core of Marks's contribution lies in his rejection of the Kantian reading of Rousseau, most closely associated in the twentieth century with Ernst Cassirer (*Rousseau, Kant, and Goethe*, 1963, *The Philosophy of the Enlightenment*, 1979, *The Question of Jean-Jacques Rousseau*, 1989), but also compatible with some more radical interpretations (e.g., Strong, *Jean-Jacques Rousseau: The Politics of the Ordinary*, 1994). Marks follows an interpretive path that begins with Rousseau's conception of nature, including his conceptions of the natural world and of our nature as humans; he maintains that Rousseau sees our moral and political development as an expression of our nature, rather than an overcoming of it, as his Kantian readers do. To make this point, Marks shows that what Rousseau calls our "nature," though sometimes an original state untouched by time and circumstance, is also manifest in how he imagines people developing through their prepolitical interactions with their physical environment and with other people (pp. 23–33). What Rousseau discerns about people in these conditions informs his ideas about human happiness, as well as his sense of our inherent limitations. Most notably, Marks argues that Rousseau believes we are happiest when we are free to pursue "disharmonious" ends—both "solitary and social" ones (pp. 85–87). Rather than look for Rousseau's defining ideas in his grandest, most radical visions, Marks sees them instead in imperfect "middling states" (p. 54).

Could this Rousseau, so sober and cautious, be the same thinker who imagines political institutions changing human nature to make moral freedom possible or who enjoins his readers to think beyond our sense of our limits (*Social Contract* II vii; III xii)? More fundamentally, why should we accept that Marks's reading of Rousseau is better or truer than others? Marks poses a version of this question himself: "Why does my interpretation elevate some things that Rousseau says over others that I also concede he says?" (13). To answer these questions, Marks offers an analysis of what he calls Rousseau's rhetorical strategy. How one evaluates this book—that is, whether one sees it as offering a few specific insights or a viable, comprehensive interpretation—depends, I think, on how convincing one finds this analysis.

Several assumptions govern Marks's interpretive approach: Because Rousseau is "a careful writer," Marks assumes that he is in control of everything he says and has consistent, considered views (p. 91). On the strength of these assumptions, Marks believes that he can clear up why Rousseau says things he does not mean, as well as why he sometimes fails to say what he does mean explicitly (p. 91). Additionally, Marks focuses on a "single feature that characterizes most of Rousseau's audiences—that they are either modern sophisticates or tempted by modern sophistication" (p. 92). It is within these parameters that he then argues that whenever Rousseau writes about nature as if it were purely beneficent and allowed us to live solitary lives, he does so only to disrupt his audience's prejudices against independence—not because he believes it (pp. 96–106). Conversely, Marks argues that Rousseau

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deliberately mutes what he really thinks about nature "because it would be of no use to most human beings and, if promulgated and vulgarized, of use to those pretenders to philosophy who would like to be kings or, at least, advisors to kings" (p. 112). That is, Rousseau anticipates that if his view that human beings have natural ends and limitations were "promulgated and vulgarized," it would most likely be used to buttress illegitimate claims to political right—therefore, he chooses not to make it explicit.

Although Marks sounds a wary note at the beginning of this chapter about Straussian approaches that distinguish "between [what is addressed to] the philosophic few and the vulgar many," he ends up employing just such an approach himself (pp. 90–91, 112). Leaving aside the question of why Rousseau would have been moved to divide the world this way in the first place, this approach summarily closes off some interpretive paths by stipulating what he thinks about his audience—and by assuming that he is simply too smart to be inconsistent. The rigid pattern into which Marks sometimes forces Rousseau's thought is all the more jarring because he also can be sensitive to the provisional and unresolved (pp. 116–17).

So why does Marks close what he closes, and yet leave some questions about Rousseau's thought open? His final chapter, "Rousseau and Charles Taylor," suggests one possible answer. Having closed off the paths that lead toward a more politically radical reading of Rousseau, Marks uses his sober subject to argue against what he sees as the excesses of Taylor's defense of community. Marks begins by citing Taylor's concern that Rousseau's attachment to community may be excessive, only to turn the tables to show that this is really Taylor's problem instead (pp. 118–19, 138–40). This is more than a clever debating trick; Marks means it to serve what I believe is the ultimate aim of the book—to wrest Rousseau from radicals and to welcome him as a sober critic of dangerous modern fantasies, liberal and communitarian alike.

Athenian Legacies: Essays on the Politics of Going On Together. By Josiah Ober. Princeton: Princeton University Press, 2005. 296p. $29.95.

— George Klosko, *University of Virginia*

In the *Philosophy of History,* Hegel says that the only thing anyone ever learns from history is that no one ever learns anything from history. Josiah Ober dissents. He believes that the study of ancient Greek history provides valuable lessons for maintaining democratic polities in today's world. *Athenian Legacies* is comprised of 10 essays—eight published previously, lightly revised. Ober is primarily a cultural historian. He describes this field as "concerned with functional explanation: how members of a society, or subgroups within a community, negotiated a set of meanings that allowed them to continue to live in an existing community" (p. 177). This volume can be read as a follow-up (one of several) to his important 1989 work, *Mass and Elite in Democratic Athens.* That study explores the role of discourse and ideology in alleviating conflict between richer and poorer citizens in ancient Athens and so contributing to democratic stability. In this new book as well, Ober's central concern is how products of public culture contributed to the reconciling of differences in Athenian identity, thus helping the Athenians "go on together." The essays were written for different audiences: political theorists, classicists, moral philosophers. But the collection is more unified than many such works, as most essays bear to some degree on the central idea of "going on together."

After a general introduction, Ober discusses lessons contemporary democracies can learn from radically participatory Athenian democracy, with Athenian institutions conceived as "inter-connected nodes of knowledge exchange, of civil learning, and of public teaching" (p. 31). This is followed by an inquiry into how a community can borrow moral authority from an interpretation of its past. He uses Thucydides as an example, arguing that the latter's attention to the recent past was intended to serve as a counterweight to the amnesty passed by the Athenian democracy when it returned to power, in the year 403. As opposed to the historical forgetting dictated by the amnesty, Thucydides attempted to provide Athens with a positive, usable past.

In the fourth essay, Ober argues that Athenian identities were characterized by thin coherence, due to overlapping allegiances to many different communities, in contrast to the extraordinarily thick coherence associated with Sparta, or the ideal cities of Plato's *Republic* and *Laws.* This is followed by a discussion of how participatory Athenian politics gave rise to "quasi-rights" (more on which later). Essay 6 describes how the "process of sociocultural reproduction" in Athens was carried on through participation in public institutions, not by means of the state-directed system of intensive education practiced in Sparta and, again, recommended by Plato for his ideal cities. This is followed by an attempt to overcome the notorious conflict between Socrates' rigid adherence to the law in Plato's *Crito* and his proclaimed willingness in the *Apology* to disobey a law that would require him to stop philosophizing. Ober's main claim is that Socrates viewed himself as following the law in the *Apology,* because impiety was against Athenian law, and Socrates believed that to cease his mission would be impious. The remaining essays concern mainly questions of interpretive methods: competition between cultural history and rational choice explanations, the meaning of Greek boundary markers, and the nature and significance of the public iconography of tyrant killing.

This is a rich collection; almost all readers will learn from most essays. Ober provides detailed accounts of Athenian culture while keeping his eye on larger themes. But given the breadth of his claims, there is much to argue

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with or to criticize. For instance, in keeping with his interest in cultural history, in the second essay, he examines how the distinctive institutions of Athens contributed to democracy as clearinghouse networks for social information, rather than through the direct political effects of open participation in the Assembly and rapid rotation of offices, especially in the Council of 500, which controlled the Assembly's agenda. In regard to the institutions' contribution, it is instructive to contrast Ober's claims with the more conventional argument of A. W. Gomme in a well-known essay, "The Working of the Athenian Democracy" (*History* 36 [February and June, 1951]). Gomme compares the structure of the Athenian Council and that of the Roman Senate, describing how differences in institutional form allowed Athens to avoid the concentration of power that effectively eliminated democracy in the Roman republic.

Ober provides a fascinating account of Athenian boundary markers, *horoi,* and their significance. For instance, on each marker surrounding the Athenian Agora was carved "I am the *horos* of the Agora." The author argues persuasively that the meaning of these stone "texts" was not self-evident. However simple their ostensible message, understanding them properly required detailed knowledge of their place in the Athenian legal system, and allowed varying interpretations by people who approached them from different perspectives. Ober's conclusion: "Artifacts and texts alike gain meaning through their situation in a broader cultural context" (pp. 210–11). But this conclusion is unremarkable, differing little from Quentin Skinner's celebrated argument for reading historical texts in accordance with the linguistic conventions of their authors' societies. Skinner's classic works on this subject are missing from Ober's bibliography.

For political theorists, the most interesting essay is probably the fifth, on quasi-rights. By this expression, Ober means rights that are granted by a given state's legal system, without implications that they are ontologically or metaphysically grounded (p. 96). He seeks to lessen the divide between democratic participation and protection of liberal rights—between Benjamin Constant's liberties of the ancients and moderns. While many theorists question natural affinities between these liberties, Ober discusses a case in which participatory democracy gave rise to formal legal protections—for women and slaves as well as for democratic citizens. The rights in question concerned gradual extension of prohibitions against hubristic behavior to members of all these classes of people. Ober describes the relevant mechanism as follows: "Lacking any clear distinction between citizenship and government, or any metaphysical basis for the assertion of rights claims, the Athenians saw that establishing and maintaining individual dignity and democratic public authority must be predicated on the actions of society's members" (p. 127). But Ober's wider claim that this case "may point towards the development of a model for deriving liberalism from democratic participation" (p. 93) is questionable, given that the rights in question protected their bearers from little more than physical assault and were not extended further.

In the final analysis, readers' responses to *Athenian Legacies* will likely depend heavily on their attitudes toward historical explanations that concentrate primarily on cultural factors. But for even those with little sympathy, engagement with Ober's essays will enrich their understanding of Athenian history and its relevance for contemporary concerns.

Love and Politics: Re-Interpreting Hegel. By Alice Ormiston. Albany: State University of New York Press, 2004. 164p. $40.00

— Oona B. Ceder, *Harvard University*

With this book, Alice Ormiston presents a compelling interpretation of Hegel's philosophy that speaks to the needs of our time. Her central argument is that love is "the continuous foundation at play in Hegel's understanding of the modern self" and the "experiential basis" of his philosophy (pp. 5–6). Hegelian love is an expansive notion that has historical grounding in early Christian mysticism. No longer readily felt in widely shared religious or civic practices, love has been all but eclipsed in the modern world. But love's knowledge is not lost. Reappearing as a moment of grace in situations of moral and political conflict, love helps us heed our conscience. Hegel's political philosophy is, we learn, a call to conscience. If we do not know what conscience requires, we become vulnerable to "the problems of our time": alienation and social atomism—seen in poverty, weakened commitments to family and local politics, and selfish disregard for the welfare of others—and a collective inability to distinguish right and wrong that contributes to evil (pp. 115–24).

Some of Hegel's readers argue that these very problems indicate a failure of his ontology (p. 115). Ormiston parts with this reading. Locating the problem within reason itself, Ormiston's Hegel warns that reflective rationality must be guided by love, or it will "repudiat[e] its own subjectively experienced, moral understandings" (p. 107). Hegel offers three strategies for how we may temper the destructive tendencies inherent in reflective thought: a courageous return to action from conscience, the cultivation of lasting commitments to family and local communities, and a renewed engagement with philosophy as a practice of justification (pp. 107–14, 125–32).

In Chapter 1, Ormiston investigates the role of love in Hegel's early theological thought. Influenced by German romanticism and idealism, Hegel's earliest essays envisioned "a new society of freedom" grounded in Kantian moral reason. However, in "The Spirit of Christianity and Its Fate," Hegel discovers the importance of love for moral action. Love rather than reason is now posited as "the true

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substance of subjectivity" (pp. 3–5). The religious mysticism of the early followers of Jesus shows that love of God and one's fellow Christians serves as a force of reconciliation: Not reason but love reconciles man and God, self and other, and the individual and society. But Hegel realized that the task of creating a rational society out of love eludes the modern self from a practical standpoint. Grappling with the nature of modern subjectivity, he recognized that love's reconciliatory power was eclipsed by reflective rationality when the legal conferral of abstract personhood in the Roman world granted individuals the freedom to think and act with a view only to their own particular needs, desires, and wills.

In Chapter 2, the author completes her interpretation of the ontological shifts in Hegel's thought, tracing the experience of reflective reason in the dialectic between conscience and forgiveness in the *Phenomenology of Spirit.* Reason has been cut off from the experience of love, and therefore cannot settle on its content as the dialectic intensifies. The moment of grace in which reason and its content are finally reconciled represents the return of love on the level of experience. Love is not conscious here, and is therefore not explicitly discussed in the text of the *Phenomenology,* which deals with what is phenomenologically present to consciousness (p. 36). But for us, who observe consciousness, "the inner knowledge of love" is detected as the source of the grace that allowed the acting and judging self to actualize "the truth of love" (pp. 44, 48). Hegel's notion of a consciously felt, mystical experience of love has not been abandoned; love has simply become an *unconscious* source of the content of the will but brings felt reconciliation on the level of our lives (pp. 11, 23). Because it is in love's nature to reconcile conflicting motivations or desires, it protects us from the divisive and destructive effects of our abstract rationality and instrumental will (p. 16).

The analysis of the *Philosophy of Right* in Chapter 3 completes Ormiston's interpretation of love's role in Hegel's philosophy. "[T]he movement of the will" that actualizes reason in the world "presupposes the experience of love"; therefore, "the logic of [this] text is meant to affirm the knowledge of love" (p. 71). To demonstrate the "final reconciliation of love and reason," Hegel formulated the will's objectification of love's knowledge conceptually. We learn that "human willing in history from the time of Christianity forward is . . . to be comprehended as the objectification of the knowledge of love *in* life, *for* reflective consciousness" (pp. 65, 68). Reason gives objective form to moral and political right, but cannot by itself generate the content of morality, justice, and freedom. Hegel's philosophy teaches reason to recognize the fundamental importance of love: The conceptual edifice of his philosophical system is motivated by the aim to vindicate love's knowledge by presenting it to reason in the only form in which reason can recognize it—that of the Concept.

For Ormiston, Hegel views reconciliation rather than recognition as the source of rational freedom. She thus rejects the reading that intersubjectivity alone generates the rational content of Hegelian modernity. She also parts with communitarian readings of Hegel, arguing that individual conscience rather than a collective subject operates at the source of the will. A middle Hegelian, Ormiston contributes new insight into the central thesis of this minority tradition in Hegel scholarship, that finite human rationality draws its content from an infinite, divine substance (p. 5). However, she shifts the focus from a religious understanding of substance, proposing instead that love designates the core meaning of substance for Hegel. While other middle Hegelians fail to explain what happens to love or assume that it has been lost as Hegel's philosophy takes conceptual form, Ormiston concludes that he only abandoned love as the basis of his *philosophy* (p. 10). *In life,* love continues to play its crucial mediating role and remains the ontological source of objective reason in the world.

Ormiston's account of the implicit, presupposed role of love in the progression of reflective consciousness in the *Phenomenology,* and in the objective unfolding of the modern will in the *Philosophy of Right,* is highly persuasive. Yet, since Hegel neither explicitly defends nor refutes the thesis that love is the unconscious source of objective thought and action, Ormiston may leave some readers wishing that she had attempted a more comprehensive interpretation of these two works, as well as a direct engagement with the *Science of Logic* and *Encyclopaedia Logic.* However, she convincingly demonstrates that we must take seriously Hegel's metaphysics if we want to understand his central contribution to modern political thought. In welcome contrast to interpretations of Hegel's metaphysics that proceed with reference to the two works on logic alone, Ormiston's lively and readable examination of the early theological writings, the *Phenomenology,* and the *Philosophy of Right* offers an unusually accessible exposition of some the most contested metaphysical aspects of Hegel's thought—the viability of his ontology, the meaning of his philosophical system, and the fate of reason in history.

A Turn to Empire: The Rise of Imperial Liberalism in Britain and France. By Jennifer Pitts. Princeton: Princeton University Press, 2005. 392p. $39.50.

— Anthony Pagden, *University of California, Los Angeles*

This book is a brilliantly successful attempt to account for the apparent transition from the fierce, bitter assault on the idea of empire by the writers of the second half of the eighteenth century—from Montesquieu and Adam Smith to Benjamin Constant—to the often self-congratulatory, high-minded endorsement of a new kind of imperial mission less than half a century later, here associated most clearly with the writings of John Stuart Mill and Alexis de

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Tocqueville. To have identified this shift in the first place is not the least of Jennifer Pitts's merits. Much of the modern, postcolonial historiography of this period has assumed, generally without much argument, that it was the "Enlightenment" that provided the ideological inspiration for the evolution of the nineteenth-century European empires. True, Pitts has been greatly assisted by two previous works, Sankar Muthu's *Enlightenment against Empire* (2003) and Uday Mehta's *Liberalism and Empire* (1999), but although these have done much to establish the existence of an antiimperial Enlightenment, neither has anything much to say about the transformation in European imperial policy, and in European political theory, that followed the end of the French Revolution.

Not, as Pitts points out, that the transition was either abrupt or complete. Condorcet's *Esquisse d'un tableau historique des progrès de l'esprit humain* (1794), as Pitts says, "suggests a civilizing enterprise that might have looked something like the imperial liberalism of the decades to come" (p. 173). John Stuart Mill's father James, who exercised considerable influence over his son's writings on India, was deeply indebted to utilitarian and Scottish Enlightenment sources, yet developed a form of liberal imperialism that, in Pitts's view, "violated the subtleties and insights of both traditions" (p. 123). Nevertheless, the roughly 60-year period with which she is concerned saw a dramatic change in the perception of just what an empire was, how it should be run, and in whose interests. On Pitts's account it witnessed a radical shift, first, in how by the end of the eighteenth century that which had come to be called "civilization" was perceived, and secondly and perhaps more significantly, it ushered in a concern about how to maintain liberal, in some cases even democratic, governments within Europe, while keeping indigenous and settler populations overseas properly in their places. As always, it was the impact of the very existence of an empire on the metropolis that proved to be decisive. The great critics of empire in the eighteenth century, in particular Edmund Burke and Adam Smith in England (on both of whom Pitts has some highly significant and original things to say), had been far less concerned with the fate of the indigenes than they had with the continuing stability of what Burke called the "British Constitution." Burke, like Montesquieu and Diderot before him, was overwhelmingly preoccupied with what happened to Europeans when they went overseas and the corrupting vices they might bring home with them.

For the liberals of the nineteenth century, for Mill and Tocqueville in particular, the problem was not so much one of corruption, as it had been for Burke, nor, as it was for Constant, the fear that the experience of nonconstraint of the colonies might give birth to new Napoleons in France. It was, rather, how to maintain levels of responsible participation in newly democratic societies while preventing the newly empowered "people" from becoming dangerously subversive. For Tocqueville, Algeria offered—as colonies had always done since Roman times—a conveniently remote home for what the sixteenth-century apologist of empire Richard Hakluyt had called "superfluous peoples," but it also offered a means of persuading the French that their governments were worthy of their continuing loyalty. Hence, Tocqueville's continuing support, in 1841, of the French presence in Algeria, even after 11 years of unrelenting warfare. For to have given up would have been "in the eyes of the world . . . a certain declaration of the decadence [of France]." A decadent France could not hope to resist for long the revolutionary undertow that threatened constantly to tear the fragile new constitution apart as, indeed, it would do just seven years later. The same applied to Britain, despite Mill's greater confidence in the stability of its representative institutions. Hence, the fanfare surrounding Disraeli's investiture of Queen Victoria as Empress of India in 1876, at a moment when the popular fortunes of the monarchy had fallen to an all-time low in popular opinion. What Disraeli had called "those wretched colonies" might indeed have been, in many respects, "a millstone around the neck of the mother country." But they were believed to be highly successful in deflecting the ever-unpredictable mob from disruption at home.

The advent of liberal society, and the massive technological progress of the late eighteenth and early nineteenth centuries, had also had the effect of reinforcing the European sense—already deeply entrenched—of a social, intellectual, and ultimately religious superiority over the rest of the world. While the older European monarchies were still clinging to power, it was hard for many of the reformers, particularly in France, to be entirely certain of their own ascendancy. The great Enlightenment critics of empire had, therefore, placed their new cosmopolitan "empires of liberty" safely in a future in which Europe itself would have become transformed into a new enlightened world freed, in Condorcet's imagination, from the dead hands of priests and kings. For those like John Stuart Mill, writing only a few decades later, this world seemed already to have arrived. Empire could now be seen not as an always dubious act of usurpation but as a mission to provide Kipling's "new-caught sullen peoples, half devil and half child," with the proper instruments of emancipation.

Pitts's finest pages, however, are on Tocqueville and the Algerian question. Those who are familiar only with Tocqueville's writings on democracy in America and ancien régime France are often puzzled, and shocked, to discover that the moral champion of representative government, and the critic of (American) slavery, should also have been a staunch defender of empire. The inconsistency is real, and Pitts's subtle reading makes no attempt to hide it. Although Tocqueville deplored the violence that conquest required, and cautioned the French against making too many enemies

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among the Arabs by attempting to transform them too rapidly into Frenchmen, his varied writings on Algeria glaringly lack the moral urgency that informs so much of his other sociological and political reflections. As Pitts says, Tocqueville himself "never confronted his own ambivalence" (p. 206). Her own explanation for this seeming dichotomy is that Tocqueville's apparent indifference to the fate of the peoples of North Africa derived precisely from his overriding concern with the "difficulty of maintaining political engagement in France in an age of democratization." The future of France took priority over the present of the "barbarous" races of North Africa. This may be so, but it is, as Pitts is fully aware, only part of the answer. Tocqueville, like Mill, looked upon liberal government as suitable only for the truly civilized peoples of the world. For the rest, as Mill said, they would have to be content with "an Akbar or a Charlemagne, if they are so fortunate as to find one." The task of the British, said Tocqueville, was not to "dominate India, but to civilize it." Yet domination was always a necessary precursor to civilization, the two being "closely connected."

In the end democracy, old and new, is a creed intended for export. It had been for the Greeks of Periclean Athens. It was for Tocqueville and Mill. It still is today. As more and more is said, much of it delusional, about globalization as "Empire" and of the United States as the heir presumptive to the British Empire, an understanding of just what empire actually was, and how it was perceived when the Euroepan powers truly did control over nine-tenths of the world's populations, is of immense and pressing importance. *A Turn to Empire* has made that task a great deal easier.

Perestroika! The Raucous Rebellion in Political Science. Edited by Kristen Renwick Monroe. New Haven: Yale University Press, 2005. 608p. $35.00.

— John Medearis, *University of California, Riverside*

Critics have berated the Perestroika movement since it erupted in 2000 for engaging in academic politics before improving the tools available for apprehending the political world. But the guiding thread of any group's thinking generally arises out of its common activities, as Karl Mannheim pointed out. So it is no surprise that Perestroika, as a movement of disaffected political scientists, would coalesce first as an attempt to storm the discipline's citadels in the name of "methodological pluralism." Some years later, however, we are in a better position to assess the principles implicated in the movement and its slogan, a judgment now enabled by the publication of *Perestroika! The Raucous Rebellion in Political Science,* edited by Kristen Renwick Monroe.

The 39 essays in this volume do not represent the full debate incited by "Mr. Perestroika's" original e-mail (pp. 9–11), which decried the dominance of statistical methodology, on the one hand, and rational choice theory, on the other. And since that communique's author remains anonymous, the movement may never have a named and authoritative voice. A number of the contributors emphasize that they write "with," rather than "for," Perestroika. Yet Monroe's volume is an enormously valuable—and frequently insightful—roadbook to the troubled terrain of political science today.

It may be best to begin with the book's assessments of the disciplinary "hegemony" of statistical methodology and rational choice theory. Lee Sigelman concedes in his contribution that by the time of Perestroika's emergence, "the rich theoretical, methodological, and substantive variety of our discipline was not being reflected nearly as well as it should have been" in the *American Political Science Review* (p. 324). Only five "qualitative" articles—ones uncategorized as "statistical," "math modeling," or "theory"—made it into the *APSR* from 1991 to 2000, as shown by David Pion-Berlin and Dan Cleary (Table 24.1). Elsewhere, Peregrine Schwartz-Shea finds that more than half of 57 leading graduate political science programs she surveyed effectively define "empirical" research as "quantitative" research—requiring statistics courses, while dropping language requirements and not offering "qualitative" methods instruction (Tables 30.1, 30.2). That said, however, the documented response of the *APSR* to the Perestroikans suggests that methodological hegemony was never institutionally centered. Sven Steinmo describes his seemingly unopposed campaign to "take back" the *APSR* in an article entitled "The Emperor Had No Clothes." But on his account, the emperor's lack of a defensible Winter Palace or a Rasputin seems to have been more important than any sartorial deficits.

The response by the *APSR* has its parallel in the selection of Perestroikans for key American Political Science Association offices. In the section of Monroe's volume devoted to associational governance, the focus broadens from methodological representation to embrace race, gender, sexual orientation, and institutional type. From the beginning, many Perestroikans promoted mandatory competitive elections for APSA offices. The report of an APSA committee chaired by Gary C. Jacobson (pp. 241–49) rejected the idea, primarily because there already exists a mechanism for forcing competitive elections and because such elections could harm attempts at including underrepresented groups in APSA's leadership. Contributors such as Marsha Pripstein Posusney and Martha Ackelsberg disagree on the plausibility of seeing democratic and inclusionary interests as being at odds with each other. But there seems to be agreement that both interests are important ones, and that they represent polls of the present debate. The way out may be some combination of set-asides for selected groups—de facto the current approach—with a voting system that can sustain representation even

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for minorities, such as the single transferable vote (although a 2005 APSA special committee report advised against adopting STV).

The centerpiece of the volume, for many readers, will be its methodological contributions. Throughout, Perestroikans and their opponents attempt to define each other with stylized dichotomies. True, most contributors avoid the otherwise-prevalent qualitative versus quantitative. But at times they replace it with "scientific" versus "interpretive" (p. 15); "project of a scientific discipline" versus "fear of the modern" (p. 115); "positivist" versus "constructivist-interpretivist" (p. 206); "hard science" versus "qualitative empirical" (p. 343); and "description" versus "work at the [scientific] frontier" (p. 349). These oppositions do not illuminate because only a dupe would accept the disfavored labels, as described; because, when it comes to the philosophy of science, most political scientists are irregular churchgoers, not theologians; and because there are more problems in heaven and earth than are dreamt of in these antitheses.

Yet despite the failure of these broadest attempts to characterize what is at stake in the volume it is clear that many Perestroikans, in contrast to their opponents, do proceed from two important and familiar premises. The first is that human beings—individually, but especially jointly—are self-interpreting and reflective, capable of assigning meanings to their actions and revising these meanings recursively. This insight is behind the frequent references to "interpretation" and "meaning" by a number of contributors, including Susanne Hoeber Rudolph (p. 15), Sanford F. Schram (p. 113), Brian Caterino (p. 136), and Dvora Yanow (p. 206). The second, related, premise is that social scientific study is directed, most broadly, toward interrelated and irreversible processes of historical change, rather than undifferentiated amalgams of events and measurements—a point well expressed by Samuel H. Beer (p. 59), and echoed by Brian Caterino (p. 136) and Greg Kasza (p. 428). These premises, jointly, disturb the search for strictly universal social, economic, and political laws because what seems to be a stable relation at one time or in one society may not be so in another—and this because of actual changes in the interconnections of things, not merely because the full set of relevant variables was not previously identified. For example, because social agents can act on and interpret terms such as "democracy" in their own ways, and can both learn from and be affected by the history of democratization, there may be, as Ian Shapiro argues, "no single path to democracy" and no "serviceable generalization" to be made about "which conditions give rise to democratic transitions" (p. 76). If so, as he points out, "looking for a general theory of what gives rise to democracy may be like looking for a general theory of holes" (p. 76).

One could wish that these shared tenets about meaning and history were supplemented by equally prominent reflections on other problems often poorly conceptualized by political scientists—such as power, as well as structure, viewed most challengingly for Perestroikans as a limit to self-interpretive agency. In any case, even meaning and history are more often alluded to than lengthily explored by many contributors to the volume. And this may be because there is not much new to be said about these topics. Some of the most interesting contributions, instead, focus more concretely on the interplay among investigators, their tools of inquiry, and particular social objects and processes. These essays are thoughtful about the difficulty and importance of properly conceptualizing the social and political world, and of asking what aspects of it are in the most urgent need of explanation before deciding what investigatory approach to use. (These are, or ought to be, key features of what Shapiro and many Perestroikans call problem-driven investigation.)

Stuart J. Kaufman's article on ethnic war argues that while rational calculation may help guide the conduct of some parties who precipitate genocide, the more momentous issue is generally "why genocide [becomes] a thinkable option at all," along with the role of "myths on both sides" and the "tendency to exaggerate both the hostility and the cohesion of the opposing group" (pp. 95, 98–99). If Kaufman is right, rationalist approaches are not ruled out as "positivist" or "hard." Rather, they simply do not generate "the best or most promising theories" of ethnic violence (p. 98).

Kamal Sadiq highlights one of the difficulties that arises from the fact that data are not socially indifferent objects "collected" by scientists, but rather are the outcome of social activities. In particular, Sadiq points out that states themselves are major producers of the very data in light of which many scholars propose to study state activities. After reviewing the manipulation of demographic categories by the Malaysian government (pp. 183–89), Sadiq argues that even those who do multicountry ("large-n") studies can only properly interpret such data in light of the kind of local, historical knowledge usually cultivated by their "small-n" colleagues. Serious reflection on the "politics of data collection" (p. 195), Sadiq argues, has to be part of any data-driven study of politics.

Dorian T. Warren points to the way in which adopting a particular methodology and the sources of evidence associated with it can implicitly narrow the questions an investigator asks and cause him or her to conceptualize the objects of study differently (pp. 223–26). Warren writes that previously, she might have more conventionally focused on individual union members' attitudes toward race, ethnicity, and gender. Instead she decided to ask the historical question why there has been change in "unions' politics around race" (p. 223), a project that required her to conceptualize "unions as political organizations and sites of collective action" (p. 224)—and one in which union members' "attitudes" might play only a part.

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Finally, Rogers M. Smith argues that in his work on American citizenship, the important thing was to establish the existence of an illiberal tradition in legislation and judicial practice concerning immigration. Knowing that critics would focus on this interpretative construction of opposed traditions, Smith writes, "I devoted much of my text to defending my categorizations, not to statistical inference testing of the data set of citizenship laws and rulings I had generated" (p. 530).

Perestroika emerged as a movement within political science, not—like the Caucus for a New Political Science—as the scholarly wing of a broader political movement. Indeed, most participants in Perestroika's online discussions "have successfully argued in favor of restricting the challenge posed by Perestroika to one of 'methods,'" according to Cecilia Lynch (p. 163). But it is not clear that political questions can be avoided. To mention just one example, a number of contributors, including Schram (p. 108), Caterino (p. 147), Yanow (p. 211), and Lloyd I. Rudolph (p. 230), see an imperative to recognize the political knowledge of lay actors, especially less powerful and privileged ones, as growing out of their interpretive methodology.

Maurice J. Meilleur, in bringing up John Dewey, places this profoundly important topic—political scientists and their political obligations—within a helpful framework for thinking about it. Dewey insisted on situating all philosophy and scientific inquiry within the context of what he called "experience," a repeated, continual social process, beginning with problematic situations, continuing with practical attempts to resolve them and then intellectual reflection on the results of those attempts. Neither philosophy nor science could ever escape its status as just a step in this process—nor could the results of either one be adequately assessed except as contributions to practical attempts to manage common affairs. In this light, Meilleur suggests a Deweyan model for political science that would regard it as a "profession," rather than a "discipline" (p. 500). The professional political scientist would focus, among other things, on clearing up errors in public perceptions, countering ideological manipulation, highlighting alternatives to existing arrangements, and promoting the ability of democratic citizens to solve their own problems (pp. 499–500).

Yet Dewey has been rightly criticized for naïveté in thinking that because inquiry was integrally bound up with practice, society could solve its problems when citizens learned to be better investigators. Too often missing from Dewey's analysis was a proper recognition of conflict and power. A related problem for scholars who might like to be Deweyan professionals is the fact that complex societies are characterized by a division between intellectual and other laborers. In such a situation, scholars respond not just to the problems confronted by society at large, however urgent, but also (perhaps even more so) to the "problems" of getting along with colleagues, advancing in the field, getting published, and obtaining tenure. As the Perestroikan controversy itself demonstrates, the sociology of political science has its own unique problems, many of which may have to be overcome before Dewey's vision can be realized.

We Who Are Dark: The Philosophical Foundations of Black Solidarity. By Tommie Shelby. Cambridge, MA: Harvard University Press, 2005. 336p. $27.95.

— Hawley Fogg-Davis, *Temple University*

The intellectual and strategic moorings of contemporary black political solidarity are increasingly unstable. As the political memory of the race-specific Civil Rights movement fades further into history, intraracial differences that have always existed, such as gender, religion, sexuality, multiracial identification, immigration, region, cultural affiliation, political ideology, and generation, are being highlighted by race scholars from a variety of fields as reasons for rethinking race-based political cohesion. Tommie Shelby acknowledges all of these internal pressures, but sees the widening gap between poor and more affluent blacks as the intraracial fissure that most threatens political cohesion among today's African Americans. From this sociological premise, Shelby sets out to articulate a "progressive," philosophically sound basis for black political organization that appeals both to the class interests of poor and working-class blacks and to those of middle- and upper-class blacks. By "progressive," Shelby means a recognition that basic social injustices linger, and can and should be corrected through state intervention and/or collective political action. Remedies for current racial inequalities cannot be found in a "conservative" return to a former era of ostensibly better social organization.

The unifying progressive principle that Shelby develops is a *political* black identity grounded in a sense of common oppression that is expansive enough to capture class as well as other differences among blacks. Somewhat counterintuitively, he finds the raw material for this political accord in a particular strand of black nationalism he names *pragmatic* black nationalism. Pragmatic black nationalists invoke a "thin," strategic notion of racial identity, as a practical way of overcoming the collective action problem of black diversity, in order to identify and pursue political goals that will benefit all blacks. By contrast, he describes *classical* black nationalism as demanding a "thick" sense of pan-ethnoracial blackness that conceptualizes black racial identity as a worthy and necessary end in itself. Shelby performs this philosophical surgery "from the standpoint of persons who identify with the tradition [of black nationalism], from a hermeneutic point of view" (p. 32), with the goal of drawing out the nascent progressive potential of a philosophical tradition that has long been respected, if not wholly endorsed, by a great many African Americans. He rightly chides liberals and progressives for using

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the personalities and institutions associated with black nationalisms to prejudge all black nationalist thought as fanatical and unprincipled.

Through a close reading of the nineteenth-century black nationalist Martin Delaney, Shelby extracts the features of black nationalism he finds most conducive to his project of conceptualizing a strategic sense of black identity. Although Delaney "wavered" between a thin and thick sense of racial identity, Shelby maintains that his deepest and most abiding commitment was to a progressive, pragmatic black nationalism. Africa, for Delaney, was not a predestined, terminal homeland for blacks, but represented instead a reprieve from American racism. In contrast to cultural nationalists who invoke racially simplified conceptions of Africa to ground a politics of racial separatism, Shelby argues that Delaney's pan-African cultural appeals were meant to "rejuvenate" democratic independence in black minds. Delaney's political principles are illuminated through Shelby, but whether Delaney's "pragmatic" nationalist principles really do trump his "classical" nationalist principles is another matter. Delaney's calls for black sovereignty may have been inconsistent, but they were calls for black sovereignty nonetheless.

Shelby's methodology, which he states forcefully at the outset, provides some conceptual clarity. He is a philosopher committed to reforming, rather than overturning, the methods of his discipline. Whereas many race theorists deem abstraction and generalization to be irrelevant, and even counterproductive, to the study of racial politics, he deftly demonstrates how these tools can serve the practical purpose of forcing us to articulate the antecedent principles that support and justify political practice. Empirically tracking when thin blackness morphs into thick blackness may be practically impossible. But for Shelby, this practical problem does not release us from the duty of guarding against what Tocqueville derisively called "spontaneous" or unreflective solidarity, the kind of group think found in Shelby's depiction of classical nationalism. Black individuals are free to embrace black cultural practices, but no conception of black culture can provide a sound philosophical anchor for black solidarity. He sees personal racial self-identification as neither imprisoning (Naomi Zack) nor fascist (Paul Gilroy). His concerns with identity are strictly political. One can be unashamed of being black without staking one's pride upon racial identification. The least and most culturally identified black individuals can and should unite under pragmatic black political solidarity.

They should do so because antiblack racism still affects all African Americans, albeit in different ways. Poor blacks seek basic material goods, such as well-paid employment and affordable housing. Middle-class blacks, Shelby opines, are preoccupied with the "public image" of blackness because they see racial stereotypes as impeding their social mobility (e.g., career advancement). And more generally, they suffer the psychological costs of these stereotypes. More affluent blacks thus have an economic and psychological interest in improving the material conditions of the black poor because the condition of the black poor fuels a more broadly disseminated racial stigma from which middle-class blacks cannot escape. The objective of black political solidarity should be to "release" all blacks from racial stereotypes.

In the end, Shelby leaves us without any sense of what sort of policies might flow from this interest convergence. Pragmatic black nationalism ends up being so expansive, so "thin," that it is hard to imagine that black women, black LGBT (lesbian, gay, bisexual, transgendered) persons, black immigrants, and other black subgroups will find this loose economic and psychological tie sufficient for the expression of their particular political needs within and outside of black communities. Yet it is precisely such internal divisions that make it philosophically necessary to predicate black politics on a thin, rather than thick, sense of racial identity. Whether the language of nationalism furthers this foundational project is questionable, given that Shelby's definition of black political solidarity bears so little resemblance to what most perceive, rightly or wrongly, to be black nationalism.

Laws of Fear: Beyond the Precautionary Principle.
By Cass R. Sunstein. New York: Cambridge University Press, 2005. 246p. $65.00 cloth, $23.99 paper.

— Judith Lynn Failer, *Indiana University, Bloomington*

This is the era in which many prefer to be safe than to be sorry. In the past five years, we have seen the United States justify a preemptive war against Iraq lest Saddam Hussein use weapons of mass destruction against us or others. Ordinary Asian citizens donned surgical masks to avoid contracting severe acute respiratory syndrome, or SARS. And in the wake of the terrorist attacks on September 11, 2001, Americans stocked up on duct tape so that they could be "sheltered-in-place." Is not an ounce of prevention worth a pound of cure?

In this tour de force, Cass R. Sunstein persuasively puts the lie to the increasingly popular concept known as the "precautionary principle" (PP). According to the PP, "regulators should take steps to protect against potential harms, even if causal chains are unclear and even if we do not know that those harms will come to fruition" (p. 4). As President George W. Bush put it when justifying the preemptive war in Iraq, " 'If we wait for threats to fully materialize, we will have waited too long,' " and " 'it is essential that when we see a threat, we deal with those threats before they become imminent. It's too late if they become imminent' " (p. 4). The problem with this mode of thinking, argues Sunstein, is that it is literally incoherent. Regulation may well protect against potential harms, but failing to regulate may protect against potential harms, too. After all, it was risky *both* to allow Iraq to threaten use of its

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weapons *and* for the United States to start a war we did not know how to conclude. If we were true to the PP and aimed to avoid *all* risks, we could neither go to war nor refrain from war.

In Chapters 1 and 2, Sunstein vitiates the PP as a matter of both theory and practice. He then goes on in Chapters 3 and 4 to explain why—and how—it nevertheless appeals to so many people. Part of the explanation stems from our tendency to focus on the worst-case scenario, even if it is unlikely. He describes this as the phenomenon of "probability neglect" (pp. 39ff), which becomes more salient when people feel strongly about the possible bad outcome. Part of the explanation comes from the social context in which fear operates. As Sunstein puts it, fear is contagious. It can spread through a series of "social cascades" (pp. 94ff), which can extend the fear geometrically; simply put, each person shares his or her fear with several others, who in turn infect several others, who frighten still others, and so on. Intense fears of bad outcomes can also intensify when groups of people confer about their worries. When people get together to discuss their fears, rumors spread, fears grow more extreme, and people end up more afraid than they were at the outset. The author calls this phenomenon "group polarization" (pp. 98ff) and uses it to explain how moral panics happen. In both chapters, he deftly substantiates his claims with a careful analysis of evidence from psychology, cognitive science, policy studies, current events, and history.

In the remainder of the book, Sunstein provides "solutions" to the problem of irrational fears. In Chapter 5, he introduces the "Anti-Catastrophe Principle" for managing risks to which we cannot assign a realistic probability. This principle is much narrower than the PP, calling on us to minimize the worst-case scenario. We should only do this, he argues, after first evaluating fully the associated social risks, striving to be cost-effective, ensuring that the risks are distributed fairly, and paying close attention to the costs (and people's willingness to pay). People may panic when contemplating "irreversible harms" (pp. 115ff), but we need not act on those fears unless the predicted harm is sufficiently probable and of appropriate magnitude.

Sunstein recognizes that government now manages risk through the use of cost–benefit analysis (CBA). After providing an outstanding summary of how CBA actually works in Chapter 6 (including a fascinating description of how the federal government calculates the "uniform value for a statistical life" [pp. 132ff]), he explains why this flawed method is nonetheless necessary. In short, CBA can help "discipline" the analysis of risks and promote "coherence," even as it translates into dollar figures the otherwise nonmonetary values associated with (not) regulating risks (p. 174). He wisely modifies CBA so that it allows for consideration of people's willingness to pay to alleviate the feared risks. He also advocates choosing regulatory options that may not maximize net benefits if those policies provide the best way to avoid harming the disadvantaged or violating rights.

On my reading, these are welcome alterations to a necessary but deeply flawed policy tool. As Elizabeth Anderson has so eloquently argued in *Value in Ethics and Economics* (1993), CBA evaluates all costs and benefits monetarily, but we often value things using alternative modes of valuation that do not translate easily into dollar figures. This means that CBA either excludes important values from consideration or includes them in a way that fails to capture their importance to us. For example, how can we put a "price" on the value of life, rights, or equitable distribution of costs and benefits? Moreover, even Sunstein's modified CBA would be done by bureaucrats who are not electorally accountable. This may well allow for rational management of the public's irrational fears, but removing decision making from the democratic process is, prima facie, problematic. As a leading deliberative democrat, Sunstein acknowledges this problem and advocates public education on real and exaggerated risks. This certainly helps to create conditions where deliberation could be more rational. But he is also right that when fear escalates into a contagious irrational fear, it undermines deliberative democracy. He makes a strong case for tolerating a potentially undemocratic solution to facilitate more genuinely deliberative democracy, but it is a bitter pill to swallow, especially for those who would rather be safe than sorry.

Trust and Rule. By Charles Tilly. New York: Cambridge University Press, 2005. 214p. $55.00 cloth, $19.99 paper.

— Ian Bell, *Commonwealth Ombudsman's Office, Australia*

From his earliest publications, Charles Tilly has focused on the relationships between varying sets of agents in civil society, forms of rule, and state formation. Democracy, and the circumstances under which it arises or disintegrates, has been an important aspect of these inquiries. Tilly's fullest, recent examination of these issues is his (2003) *Contention and Democracy in Europe, 1650–2000*. There, he argued that what was crucial for democratization over the long period was the insulation of public politics from "categorical inequalities" of class, race, gender and the like; the transformation of public politics in ways that broadened and equalized participation, enhanced collective control of government, and inhibited arbitrary conduct by political actors; and the integration of "trust networks" into public politics. *Trust and Rule* elaborates the conceptual and historical arguments for this last "necessary" condition for democratization. In doing so the author jousts briefly (but persuasively) with the recent debate over "trust," social capital, and democracy. He then recasts that debate in the light of older literatures in social history, sociology, and institutional economics on networks as a means of

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collective action in the presence of various forms of rule, not only democracy.

Tilly objects to "dispositional" accounts of trust that, in his view, dominate political analyses. These stress the importance of an attitude or individual orientation that somehow extends to popular trust of governments and political leaders. Against this view, he sees trust as a property of interpersonal relations and especially of "trust networks." Within such networks, members' relations to one another put major collective and long-term enterprises at risk from the malfeasance, mistakes, or failures of other network members. Migration chains across states, trade diasporas, kin networks, religious sects, and the like may all illustrate such relations over time—but they also may not. The precise sense in which Tilly defines trust networks excludes most ordinary voluntary associations and routine social networks.

Apart from this work's exploration of the internal dynamics of, and external threats to, trust networks, a key concern is how nonpolitical trust networks politicize themselves, connect with political networks, or give way to politically connected networks. Tilly's response is framed in several ways, and here I set out just one taxonomy. First, totalitarianism, theocracy, and democracy are conceived as exemplars of a "zone of direct rule" in which a central government extends its writ effectively to local communities, including trust networks. Systems of patronage or brokered autonomy typify a "zone of indirect rule" wherein either powerful intermediaries protect trust networks or the latter are granted substantial autonomy. Where highly segregated trust networks persist through evasive conformity, or ad hoc particularistic ties with authorities, then there is merely a "zone of thin political control."

Second, Tilly asks how trust networks move between these zones. To move into the zone of direct rule, he suggests, segregated trust networks may dissolve, a previously segregated network may be integrated into public politics, or corporatist-like, the state itself may create politically connected trust networks. In the latter case, for instance, veterans benefits might be instituted, thereby tying that constituency directly to the state. Further, in the zone of direct rule, whereas totalitarianism and theocracy involve very extensive integration of trust networks into public politics, democracy always involves only a partial integration of the same. Contingent consent on the part of members of trust networks is also central to democracy, and thus political agents are more likely to rely on shared commitment in order to secure their integration into public politics than on coercion or material resources. For the author, then, it is not associations as such that hold the key to democratic participation but the connections between trust networks and public politics. Where integration of trust networks occurs in a democracy, their members develop a real stake in political processes, and the prospects for both participation and consultation improve. Conversely, he worries that where a withdrawal of trust networks occurs from public politics, whether voluntary or involuntary in character, democracies—present and future—are vulnerable.

If key networks and interests withdraw or are excluded from public politics, democracy may weaken. But equally interesting is a fine-grained assessment of which specific networks and interests never form in the first place, which get to be integrated and/or enjoy state sponsorship to do so, and which gain what degree of power. Tilly's analytical focus is to set out abstract meso-level causal mechanisms affecting the development of *all* such networks, and the relations between them and other actors. It is not clear to me that a single concept, trust networks, can bear the weight of the vast array of historical material he brings under its rubric.

Further, the conception of trust networks employed here has shades of the predicament of interest-group analysis in a pluralist worldview. The analyses of specific networks are inflected with all kinds of macroinstitutional expressions of power, but these go unexplored. For example, Tilly laments situations where rich, powerful people buy public offices or capture parts of government most affecting them because this represents an effective withdrawal of their trust networks from genuine bargaining and consultative processes (p. 136). Elsewhere, he refers to the way proletarians were integrated into public politics in democracies over the last century in circumstances where it seemed vital that new politically connected trust systems be created through social security policies and the like (pp. 120–24).

This analysis of key economic agents seems to focus on a limited notion of direct power and to lack the type of macroinstitutional understanding of business's power over investment in liberal societies, which is standard in those literatures where the postliberal tradition typified by Charles Lindblom meets the post-Marxist analyses of a Fred Block. Having regard for the constraining and enabling affects of such macroinstitutions, we might grasp better why, on many occasions, business has neither needed to organize directly nor seen fit to connect to public politics. Further, we would gain insight into why workers were especially reliant on forms of political organization and integration sponsored by governments. In short, Tilly's style of meso-level relational analysis risks missing certain broad, albeit historically bounded, macroinstitutional insights (whether economic, symbolic-cultural, or political in character) that would explain better why certain networks sought and/or needed direct integration into public politics in liberal democracies while others did not. While his theses about categorical inequalities were designed to address this kind of criticism, this work makes little effort to connect those concepts to his discussion of trust networks.

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This work offers a stimulating, demanding, and provocative argument on trust and forms of rule. It also reveals Charles Tilly again as a fine storyteller. It is, however, best read with a working knowledge of his other recent publications on contention and democracy.

Race and Racism in Modern Philosophy. Edited by Andrew Valls. Ithaca, NY: Cornell University Press, 2005. 294p. $45.00 cloth, $21.95 paper.

— Kevin Bruyneel, *Babson College*

This is an important work, which I highly recommend. Andrew Valls has gathered an impressive collection of essays, primarily from philosophers, each of which examines the place of race in the work of a writer of the modern Western canon. The writers under examination here are Descartes, Hobbes, Spinoza, Leibniz, Locke, Berkeley, Hume, Rousseau, Kant, Hegel, John Stuart Mill, Marx, and Nietzsche. They are all strong, thoughtful essays, and the text as a whole will prove valuable for upper-level undergraduate and graduate philosophy and political theory classes.

Still, with regard to a political science audience, not all of these essays speak easily to the concerns and approaches of political theorists. For example, the idea that a philosopher such as Berkeley is "not a philosophical racist" but is an "ethnocentric" who would welcome "the destruction of Native American cultures insofar as this might aid in the conversion of these people to Christianity," as William Uzgalis concludes in his essay, might be read by political theorists to be a philosophical distinction without a political difference (p. 123). The same might be said about the distinction between "racialism and racism," which Valls, drawing from Kwame Anthony Appiah's work, recommends in the Introduction and effectively employs in his piece on Hume: "Racialism" refers to the empirical claim that "there are races that differ in certain characteristics," and "racism" denotes the moral position that "members of different races are justly treated differently" (p. 7). In reading this collection, one gains a strong sense of the differences between a philosophical notion of race based in ontological and epistemological claims and a political theoretical notion of race concerned with the public construction and articulation of racial identity and inequalities in the formation and maintenance of political communities. These essays lean toward the philosophical take on race, but not entirely, demonstrating the complexities of the issue.

Timothy Reiss's chapter on Descartes does a nice job of dealing with a tough assignment: how to discern the racial views of a philosopher who "named race never and slavery twice" (p. 17). Reiss provides a rich sense of Descartes's intellectual forebears—notably Aristotle, Aquinas, Vitoria, Las Casas, and Bodin—on the question of slavery and the prescient issue of the status of indigenous people in the midst of European colonization. Reiss does more than ask the Nixonian question "what did Descartes know and when did he know it?" He connects context to Cartesianism to show, for example, how Descartes ultimately echoes Aristotle by seeing slavery as still "a *civil* condition, however appalling" (p. 37). This chapter nicely leads off the collection by offering a preracialization-of-slavery view of modern philosophy and a model for how to bring together historical context and textual analysis. A number of chapters utilize this model to good effect, in particular those by Anthony Bogues on Mill, Robert Bernasconi and Anika Maaza Mann on Locke, and Richard Peterson on Marx.

Bogues's piece does the best job of balancing text with context, set in particular around the fascinating story of Mill's involvement in the fallout and assessment of the Morant Bay Rebellion of 1865. In so doing, Bogues offers an example of the racial coding in modern liberal thought located most notably in the savagery/civilization binary that served as a popular trope for theorists seeking to reconcile their liberal views with their nation's colonial practices. In a similar way, Bernasconi and Mann deftly bring Locke's work on *The Fundamental Constitutions of Carolina* into conversation with the *Two Treatises* to consider as "evidence of his racism" the "contradiction between the Locke who profited from slavery and the Locke who, as a theorist of natural rights, would be appealed to by opponents of slavery" (p. 90). In the end, among other things, they substantiate Locke's role in and advocacy of racializing slavery. My only significant quibble is that it would have been nice to see the authors consider if Locke's involvement with slavery and his liberalism may not have been contradictory at all, but rather indicative of the mutually constitutive forces shaping an embryonic *herrenvolk* liberal democracy. Peterson's take on Marx is also subtle in situating him historically, leading to an acute assessment of how Marx's racial "blind spot" limited the possibilities of his work. To Peterson, "Marx's shortsightedness about race is connected to a general failure regarding modern identity and the politics concerned with it" (p. 248), and this failure undermines his theory of class consciousness because racial identities are viewed as epiphenomenal rather than as constructed identities with their own independent power.

Other essays eschew context in favor of close textual readings. A nice example of this is the chapter by Debra Nails on Spinoza, where like Reiss, she is dealing with a thinker who said very little about race. Nails provides an edifying reading of Spinoza's philosophical system, especially his ontology, in which she connects an illustrative take on the right to exist and power of the coffee cup in her hand to an explanation of the right and power of the concept of race(s) to exist. This essay should provoke a fruitful debate between philosophical and political theory understandings of right, power, and race. James Winchester's chapter on Nietzsche is also

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compelling reading, with a specific focus on the latter's attentiveness to the intricate relationship between physiology and philosophy, and thereby his fascination with the concept of race breeding. On balance, Winchester sees Nietzsche as more of a racialist than a racist. But here we also see an example of how this distinction can ring a little hollow. When interpreting the meaning of Nietzsche's claim that "Negroes are representatives of a prehistoric men who are capable of enduring pain" and his claim that Jews "are the strongest, toughest, purest race living in Europe 'today'" (pp. 259, 265), Winchester sees this as not fundamentally racist—as not justifying unequal treatment—because to Nietzsche, these were compliments paid to two noble, even racially *superior* groups. But it is too easy to deny the existence of a racist claim by saying that a writer has referred to a particular group as in some way superior. One only needs to look at the way this construction has worked against indigenous people—that is, "noble savages"—to see that the claim to superiority in one sense, here a philosophical one, can easily undergird and mask a justification for inferior status in another sense, usually a political one.

Due to chronological organization, the essay on Nietzsche closes the collection, but Charles Mills's chapter on Kant would have been a better choice, because Mills utilizes his specific analysis of Kant's racist formulations in order to address his wider concern with the construction of personhood in seemingly universalist modern philosophy more generally. Mills wants us to resist the urge to explain away a claim or exclusion a writer makes about race and instead take them up "as a philosophical challenge" in which we ask, in this case, "how Kant's theory needs to be rethought in the light not merely of his own racism but of a modern world with a normative architecture based on racist Kant-like principles" (p. 190). Cultivating this connection between philosophical approaches and political theory concerns is a goal worth striving for, one which this collection works to foster by advancing our investigation of race in modern thought and provoking interdisciplinary debates on this complicated issue.

Benjamin Franklin Unmasked: On the Unity of His Moral, Religious, and Political Thought. By Jerry Weinberger. Lawrence: University Press of Kansas, 2005. 352p. $34.95.

— James F. Pontuso, *Hampden-Sydney College*

There are two important issues raised by Jerry Weinberger's intricate, probing, and insightful book: whether his portrayal of Franklin is accurate and whether Franklin's understanding of existence is correct. Weinberger's thesis is controversial: From age 15, when Franklin read his father's books on theology until his death at 84, he never wavered in his conviction that God did not exist, religion was superstition, and moral principles were merely longings of the human psyche, unfulfilled in this life or the next. But if he took such a skeptical view of righteousness, why did he pen his *Autobiography* and promote such homespun virtues as temperance, frugality, justice, chastity, and humility?

Weinberger argues that Franklin's pieties were an elaborate and lifelong hoax concocted to ensure his personal safety, while disguising his real opinions from the wrath of morally serious and religiously devoted people. The young Franklin learned that his jabs at decency and faith, while effective at vexing his interlocutors, were risky. After reading Xenophon's *Memorabilia* at 17, he adopted the Socratic technique of irony and never again openly attacked prevailing orthodoxies. Behind the veneer of probity and devotion, he employed humor—often salacious—to deconstruct moral pretense and irrational, passionate faith. Weinberger maintains that Franklin made fun of everything and held nothing sacred. John Adams, the sort of earnest and solemn personage Franklin ridiculed, would not allow Franklin to work on a draft of the Declaration of Independence for fear that the Philadelphian would somehow slip in a joke.

Weinberger's most contentious claim is that not only did Franklin fabricate his facade of homespun philosopher and down-to-earth moralist, but he also left behind an elaborate set of clues in his writing that allow careful readers to uncover his ruse. He never deviated from his authorial intention to disguise and disclose his meaning.

Ben Franklin used esoteric writing? The theory may not be as far-fetched as it seems. He was, after all, a scientist, successful businessman, hugely popular author, renowned statesman, sexual rake, and, perhaps, an atheist. What does a person of superior ability and unpopular views do in a highly conventional hidebound society? He might pretend to be those things that his pious countrymen value. He might also be shrewd enough to take advantage of the simple-minded folk among whom he lives to secure his own comfort and pleasure. And, of course, he might have just enough pride in his own superiority to leave behind a road map for those willing to take the time to search out the "hidden" truth of his life.

But was Franklin not a humanitarian, the postmaster, patron of a hospital and an antislavery society, inventor of the lightning rod, and—most impressive of all—founder of a democratic nation? No doubt, but Weinberger argues that he undertook these activities more for his own pleasure and amusement than from duty. Franklin enjoyed the good things in life, including fame and the perks—all of them—attendant on celebrity.

It is striking how similar Weinberger's Franklin resembles the Richard Rorty of *Achieving Our Country* (1998). Like Rorty, Franklin denies the existence of metaphysical standards of right and wrong. Both men are pragmatists,

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more interested in making things work than in adherence to some abstract, unalterable principle of proper behavior. Both suppose, to paraphrase Rorty, that individual beliefs are constructed by historical circumstances and are little more than prevailing attitudes about what is considered true and just. Both are committed egalitarians, less from a devotion to democracy than from a belief that there are no standards and every person's views are as good as every other's. Both seem to engage in civic activities for the same reason that people play chess: to amuse themselves.

Franklin believed that there was nothing wrong with using his superior intelligence to his advantage, especially since he could get away with it. If this principle is true, perhaps it is also proper to take advantage of beauty for one's advantage. And to be consistent, is it not fitting to use guile and cruelty to get ahead, provided, of course, that they are employed effectively? Weinberger's Franklin looks eerily like the perfectly unjust man with a perfect reputation for justice in Plato's *Republic.* Is Franklin's view of virtue correct?

Franklin became truly famous because of his association with the first large-scale experiment in democracy. Neither the revolution that gave birth to the American republic nor the government formed to manage its affairs would have been successful without George Washington. Washington was not particularly brilliant, beautiful, or cruel. Rather, he was renowned for his integrity and virtue, qualities to which his countrymen responded with admirable devotion. Perhaps virtue is not as verifiable as are the laws of nature uncovered by Franklin's scientific experiments, but it does exist for human beings. Without Washington's virtue, the American Revolution would not have succeeded and Franklin might be remembered as a dilettantish oddity who suffered the unpleasant experience of hanging.

Weinberger has an extraordinary talent for illuminating the meaning of texts. He forces us to look for hints and ponder alternative points of view. He makes us consider what is said, what is implied, what is omitted, and what might have been said to make an argument persuasive—but was not. His analysis of Franklin's writings is so thorough and complex that it is not clear whether it is Franklin or Weinberger engaging in philosophic discourse. I must confess that I am not a Franklin specialist, and I spent about a month attempting to recreate Weinberger's research. The best I can say is that Weinberger *could* be faithfully presenting Franklin's veiled intentions if we ignore Franklin's specific exoteric statements, such as that he believes in God. Readers will be led to wonder, then, if Weinberger has not read too much into Franklin, and if so, why he presents such a profoundly amoral portrait of Franklin. Perhaps the biographer hopes to remind us that philosophy is not narrowly parochial or merely edifying.

Origin Stories in Political Thought: Discourses on Gender, Power, and Citizenship. By Joanne H. Wright. Toronto: University of Toronto Press, 2004. 260p. $50.00.

— Christine Di Stefano, *University of Washington*

The author's express purpose in this book is "to cast a critical eye on the origins motif as it surfaces in the tradition of political thought" (p. 23). "Political origin stories," Joanne Wright tells us, "are narratives about the beginnings of politics and power" (p. 3). Origin narratives may be used to consolidate existing or threatened political arrangements, or to inspire radical efforts to reconfigure the prevailing terms of political identity and civic consociation. The crucial thing to note about origin stories is that they function as foundational narratives, effectively setting uncontested and quasi-ontological terms for political identity, aspiration, and legitimacy. In short, though politically motivated, origin stories are perversely antipolitical, in the sense that they "confer upon politics the permanence of nature" (p. 10). Origin stories have a number of other troubling aspects and effects, including "their manipulation of history, their naturalization of conventional political relations, and their function as justificatory scripts of citizenship" (p. 22). The myth of the metals from Plato's *Republic* offers an exemplary case of the origin story: "the autochthonous myth of the metals justifies and elicits consent to a hierarchical ordering of classes in the republic" (p. 4).

The "persistence and longevity of the origins impulse" (p. 23) is well—if selectively—displayed in the author's choice of material, which includes and highlights the political theories of Plato, Hobbes, Carole Pateman, and radical feminism. According to Wright, origin stories call out for demystification, not merely debunking. Thus, historical and political contexts must be attended to in order to uncover the specific political motivations animating the production of origin stories. Feminist political theory is chastised for failing to apply "the historical sensitivity of contextualist interpretation" (p. 16), and in chapters on Plato and Hobbes, the author demonstrates how such sensitivity yields an enhanced, if also chastened, understanding of the role of gender in political theory. For example, it makes no sense to cast Plato's appeal to a patrogenic cosmogony in the *Timaeus* as a deliberate effort to suppress the female role in reproduction, given the state of embryological knowledge at the time. Historical inquiry figures prominently in this text, not only as a methodological tool but also as a potential, if modest, substitute for political origin stories. While the author grants that origin stories may not be completely eradicable, she suggests that "history can potentially provide a better, more solid foundation for politics" (p. 12).

Hobbes emerges as a fascinating and unconventional origins theorist here: "Hobbes's significant insight on the origins discourse is his recognition that political solutions

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cannot be found in the narratives of mythical, historical, or customary beginnings" (p. 76). Why then, did Hobbes create an origins narrative? It turned out to be a useful device for discrediting some constitutional theories with which he disagreed and to demonstrate that precedent is not prescriptive: "Hobbes's origin story in the state of nature does not prescribe any *particular* political solution. And neither the state of nature nor the social contract summons a golden age that the citizens of England ought to try to recover" (p. 60).

While Hobbes's state of nature is emphatically not an attractive scenario for recuperation, the "first" social contract is presented by Hobbes as the only "logical" solution to the dilemmatic structure of the state of nature, particularly with respect to promise making. Furthermore, this "first" social contract contains instructive lessons about the logic of political legitimacy. It seems to me that Hobbes's state of nature *is* prescriptive, albeit in a negative sense of avoidance-at-all-costs, and that it plays a vital, if spectral, role in his unique rendering of political legitimacy.

In a separate chapter, Wright explores Hobbes's case for original maternal dominion in the state of nature. She argues, convincingly, that "Hobbes effectively disrupted gender norms, opening a space in which gender relations could be dramatically—if briefly—reconceived" (p. 77). Yet feminists have been more suspicious than appreciative of Hobbes because he fails to follow through with the gender-bending logic he deploys in the state of nature when he turns his attention to civil society. One of the most significant and influential efforts to identify such an underlying logic is Carole Pateman's *The Sexual Contract* (1988).

Pateman is criticized for "a general lack of historical and textual specificity in her theory" (p. 110), for her free-wheeling use of conjecture, for her characterization of Hobbes as a patrogenic thinker, and for offering an "origin story of her own" (p. 121) in which original rape by men over women establishes the conjugal right of husbands over wives. According to Wright, "Hobbes does not explain thoroughly his transition from the state of nature to civil society, and too much is left unsaid for us to make any reasonable conjecture" (p. 116). "Reasonable" conjecture for this author means conjecture that is consistent with the motivations of the theorist. So, she argues, it is extremely unlikely that Hobbes repressed "an all-out battle between men and women that established women's defeat" (p. 117) in the state of nature because he was not really concerned with patriarchy as a conjugal relation. All he cared about was to defeat paternal patriarchalist arguments. Therefore, there is *no point* in trying to connect the dots between the state of nature (where gender inequality has no foundational backing) and civil society (where it is alive and well).

This argument seems to me to delimit the contemporary enterprise of political theory in unacceptable ways. By restricting ourselves to (our reconstructions of) the conscious and historically plausible intentions of authors, we recuse ourselves from exploring logics of the text that may exceed authorial intent, even as they resonate with and reinforce cultural logics that are complicit with historical and contemporary relations of domination and subordination.

According to Wright, the errors of Pateman's ways are linked to the influence of radical feminism on her thinking. Furthermore, she argues, the radical feminist quest for origins and an "authentic" women's culture contributed, at least in part, to the decline in radical feminist activism by the mid-1970s. Presumably, if such feminists had been less interested in myth making and more interested in contesting "real world" political relations, feminism would have achieved more than it has to date. This sounds pretty conjectural to me, although I am inclined to agree that the search for origins was misguided.

Notably missing from this panoply of origins theorists is Rousseau, whose *Discourse on the Origins of Inequality* surely qualifies as a masterpiece of the origins genre, even as it complicates and extends the critical analysis to which Wright has committed herself. We might even want to think of this discourse on origins as a performative parody of the origins genre, simultaneously enacting the narrative structure of the political origin story, revealing its fantastic fictionality through counterfactual exaggeration, and putting a powerful and persuasive mythical origins story to work for critical, as opposed to justificatory, purposes. Rousseau, along with Nietzsche, effectively appropriated the genre of origins theorizing in such a way as to undermine the foundational urge that animates such theorizing. And each did so by riding roughshod over "history" and engaging in deliberate and extravagant mythmaking.

This suggests an alternative strategy for countering and contesting the origins impulse. History is not the singular cure for this particular brand of human susceptibility. Nor will a sharp divide between "history" and "myth" save us. Does "history" insulate us from the urge to feed the retrospective foundations imperative, or does it make that urge respectable? How does the invocation of "history," as opposed to "myth," make for a better politics? In this important and timely book, Wright has rightly alerted us to the pitfalls of "origins thinking" in political thought. What we still need is a proper appreciation of its seductive power, which is perhaps sublimated, but not yet vanquished, when it goes by the name of "history."

Feminism and the Abyss of Freedom. By Linda M. G. Zerilli. Chicago: University of Chicago Press, 2005. 272p. $55.00 cloth, $22.00 paper.

— Marion Smiley, *Brandeis University*

In her book, Linda Zerilli sets out to accomplish three very important and difficult tasks. The first is to persuade

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contemporary feminist theorists to leave behind their methodological concern with the category *woman* and to pursue political freedom understood as the practice of "beginning anew." The second is to articulate and defend such an understanding of political freedom in light of other ways of thinking about the topic. The third is to show how political freedom so understood might be used to develop a feminist project of political freedom that empowers women as a group without imposing false universals on them.

Zerilli's arguments about the depoliticization of contemporary feminist theory are both timely and important. So, too, is her critique of those feminist theories that place the coherence of the category "woman" at the center of feminist politics. According to Zerilli, "[i]t is strange to think that the future of feminism could possibly hang on the status of an analytic category of feminist theory" (p. x). For while the category in Western feminist theory is indeed less universal than it is made out to be, "few feminist activists understand theory as a guide for practice," and even if they did, the endless parsing of differences required by the debunking of false universals would most likely lead to a "politically destructive cynicism" (ibid.).

Zerilli's concern to move beyond the methodological debates of third-wave feminism is largely practical. The author wants to revive a feminist practice of freedom. But her critique of these debates is itself theoretical rather than practical. Indeed, it is based on an important insight, that those feminist theorists who now treat the category as key to feminist politics make two problematic assumptions: (1) that in order to be politically significant, claims to speak in the name of women must function like a rule under which to subsume particulars; and (2) that in order to be legitimate, these claims must employ a general category, called woman, that can be used to describe all women and their activities.

As Zerilli notes, such assumptions are both conservative and epistemologically (and politically) unnecessary. Why turn to political freedom in this context? According to the author, political freedom avoids the constraining logic of universal/particular. And it allows for continuous re-creation and control over a collective identity that is possible only when individuals leave the prevailing rules of recognition behind and together—in the public realm—invent new ways of understanding themselves and their world.

Zerilli's view of political freedom replaces the "I will" associated with notions of individual sovereignty with the "I can" of Arendtian world building. Political freedom in Hannah Arendt's writings is "the freedom to call something into being, which did not exist before, which was not given, not even as an object of cognition or imagination" (p. 24). This kind of political freedom can, according to Zerilli, be extended to the formation of the "we" of feminism: "Thinking about women as a political collectivity, rather than as a sociological group or social subject, means thinking the 'we' of feminism anew as the fragile achievement of practices of freedom" (ibid.).

How might political freedom understood as such become part of feminist practice? Zerilli answers this question by turning not to contemporary feminist projects but to Monique Wittig's *Les Guerilleres* (1985) and the Milan Collective's *Sexual Difference* (1990) for what she calls expressions of feminist political freedom. Wittig famously begins her revolutionary utopia by asking "What if women were to rule themselves?" and then proceeds to spin the tale of a group of women warriors who develop a new form of social interaction that is itself based on the creation of a space in which women can move in word and deed among equals. The Milan Collective, in a similar vein, takes as its major purpose in *Sexual Difference* the creation of a feminist community in which women are able to achieve freedom through the exercise of personal and political accountability to other women.

According to Zerilli, we can see in all of Wittig's efforts, including her efforts to break with norms of heterosexuality and to imagine what women would be like if they governed themselves, a kind of political freedom that "takes leave of the false security of epistemology and ventures out into the world of action" (p. 69) and that allows women to create both themselves and their own reality. Likewise with the Milan Collective, we can see in its efforts to develop women's bookstores new kinds of social relationships, as well as the "replacement of traditional images of women's wretchedness with the radical figure of feminist freedom" (p. 125), a kind of freedom that is both constitutive of new identities and truly political by virtue of its association with a (reconstructed) public sphere.

Zerilli's analysis of these texts is fascinating, and it contributes to the development of a conception of freedom that is genuinely political. But her reading of this notion of political freedom as distinctly Arendtian faces three challenges. The first is that feminists do not have world-historical events such as the French and American Revolutions to point to as examples of world building. "The problem of 'founding,'" Zerilli admits, "continues to haunt feminists" (p. 168). She suggests that we might substitute for an actual world-historical event or revolution in this context the "revolutionary spirit that animates such events" (p. 25). But such a spirit is not itself the kind of founding associated with world building. Instead, it is one of its conditions.

Second, the relationships that feminists have "founded" over the years—and the ones that Zerilli herself points to in the case of both Wittig and the Milan Collective—have generally been social, not political. The social realm is of course very important to democratic polities in general and to feminists who view the personal as political. But it is not something generally favored by Arendt, who was very skeptical about the social as a basis of politics in

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general. Hence, while the social relationships that feminists have founded over the years may be very important to feminism, they do not appear to be compatible with Arendt's own political theory.

Zerilli does not ignore or distort Arendt's own words as a way of getting around this dilemma. (Indeed, she demonstrates great integrity by underscoring the dilemma herself.) Instead, she attempts to reconcile her focus on social relationships with Arendt's theorizing by arguing that Arendt's major concern is not with its social content but rather with the means–ends rationality associated with it. Further, she claims, "Thinking about women as a collectivity rather than as a sociological group or social subject, means thinking the 'we' of feminism anew: as the fragile achievement of practices of freedom" (p. 24). Zerilli's interpretation of Arendt may be correct. But even if one can assume that women constitute a collectivity, one cannot assume automatically that women are, as members of this collectivity, political actors or that their sphere of action is public. Instead, one has to make explicit in what respect their collectivity is political and what it is about their sphere of action that is public. In other words, one has to define—redefine?—the terms "political" and "the public" in this context.

Third, the kind of political freedom that Zerilli advocates may simply be impossible since it requires individuals to leave the realm of determinism behind, if only temporarily, to build new worlds together. She does not, to her credit, posit a transcendental notion of free will. Instead, like Arendt, she embraces contingency. "What would it mean," she asks, "to think about feminism as an 'infinite improbability'? What would come of rethinking the 'we' of feminism as something utterly contingent, that is, as something that could just as well be left undone, as something highly fragile that could be driven out of the world?" (p. 24).

This view of contingency undoubtedly has appeal. But if those who view human agency as partly or wholly socially determined are correct—and they have some strong arguments—then human beings cannot "start anew" in Zerilli's sense. She does not, of course, have to accept these arguments. But she does have to take them seriously. Instead of invoking Arendt as an authority, she has to convince us that human beings are in fact capable of leaving behind existing webs of meaning and creating their own reality.

Zerilli has done an admirable job of presenting, as distinct from fully defending, her notion of political freedom on the basis of a very compelling and beautifully written interpretation of Arendt's political theory. Likewise, through her creative reading of the works of both Monique Wittig and the Milan Collective, she has succeeded admirably in showing how this notion of freedom might be realized in feminist practice. Hence, even though questions remain, *Feminism and the Abyss of Freedom* stands as a very important contribution to both political theory and feminist politics.

AMERICAN POLITICS

Ten Thousand Democracies: Politics and Public Opinion in American School Districts. By Michael B. Berkman and Eric Plutzer. Washington, DC: Georgetown University Press, 2005. 226p. $26.95.

— Charles F. Cnudde, *University of Massachusetts Boston*

An important and ambitious empirical study of policymaking in the (K–12) school districts of America, this book presents statistical support for noteworthy and sometimes surprising conclusions about the functioning of democracy at this level. The study is important because it raises and answers theoretically valuable questions about democracy in the organization and functions of the public schools. It is ambitious because the data and analytical requirements upon which these questions and answers depend are substantial.

In an empirical contribution to democratic theory, the authors take an essentially outcomes view. They see democracy as "policy responsiveness," which, in their case, means the correspondence between public opinion on school spending levels and the actual level of spending in the district. Among the most interesting conclusions of the book are the roles of the different institutional structures across American school districts and their differential impact on policy responsiveness.

American school districts use a variety of institutional forms for making policy, including decisions on taxing and spending. They vary from direct democracy, such as New England town meetings and voter initiatives for tax and spending decisions; to representative democracy through elected school board decisions on these issues, in some jurisdictions combined with citizen referenda, in others, not; to school boards not directly elected but appointed by higher authorities, such as city, county, or state officers.

Most observers would view this array as one of a declining level of democratic forms, with town meetings as the most democratic and appointed boards as the least. Consequently, one would expect policy responsiveness to decline across the array. Yet the findings are the reverse. Policy responsiveness is greatest when decision makers are appointed and is lowest in the New England town meetings. The authors' explanation is, in effect,

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that indirect democracy works: Public officials enact general citizen preferences more closely than do low-turnout town meetings and elections.

Similarly, the function of what the authors call "substantive representation" is consistent with this explanation. They use that term to mean, in effect, policy responsiveness to African Americans. It differs from "descriptive representation," which means whether the school board membership approximates the African American proportion in the district. In the South, the move toward elections and away from at-large elections increased descriptive representation, but the fundamental finding is that appointed school boards were the most representative of, and had the greatest level of, policy responsiveness to African Americans.

The findings about institutions and policy responsiveness have implications for public choice theory. Scholars from that perspective tend to hold that representative structures, such as school boards, are one or more steps distant from the preferences of the "median voter" and that some form of direct democracy would come closer. In policy terms, these scholars usually hold that this means that voter-isolated school boards and appointed school superintendents (and other bureaucrats) spend more on the schools than the average voter prefers. Yet as the authors point out, there has been heretofore no empirical support for that conclusion, and the authors' conclusion holds otherwise.

Other interesting findings include the impact of teachers' unions and the aged. The former, like school bureaucrats, have been accused of pushing school spending beyond citizen preferences, and the latter, the reverse.

The authors show that teachers' unions that lobby for increased school spending are effective at state and local levels, depending upon their strength and memberships at those levels. Nevertheless, they conclude that their effectiveness is just as often consistent with public opinion as not. Often the unions and the citizenry are united in advocating increased school spending.

The "Gray Peril" hypothesis is that older citizens in the community will try to hold down school spending as they no longer have a direct interest in children's education. A competing hypothesis is that they do have an interest in maintaining property values, which depend in part on good neighborhood schools. The authors say that the aged have differential effects on school spending. Those who are long-term residents support spending on the schools as they identify with the community, whereas newcomers of the same age group tend to depress school spending. Pity the Florida children.

In both of these findings and others, the authors make use of interaction effects to estimate the differential impact of groups on policy. This analytical approach is refreshing in a book of this nature; it does not limit its analysis to additive effects.

The analytical ambitions of the study include the need for public opinion measures from the 14 thousand school districts in America. No one can afford such a study, and the authors rely on a technique initiated in political science by the Warren Miller and Donald Stokes 1963 study of constituency opinion and congressional voting. They used a national sample broken down to congressional districts to measure opinion. A small industry grew up around this method, continuing with the present book. As a matter of full disclosure, I (with Donald J. McCrone) participated in a small way in an early (1966) and much-criticized part of this industry. (Indeed, a subsidiary industry of such criticism shortly developed.)

An issue that has not been well aired in these critiques is whether these opinion measures come from representative samples. In the present book, the authors add cases by using the opinion question on respondent preference for higher, lower, or the same level of spending on the schools that employs the General Social Survey (GSS) data from the 1985 to 2002 surveys. Aggregating, they achieve 26,000 respondents, enough to satisfy sampling error requirements. Yet sampling error estimates assume random sampling. We assume that the GSS meets this requirement for the nation as a whole. Because of the stratifying and clustering factors in a national sample, the conclusion that a sample—no matter how large—is representative of a component unit smaller than the nation, such as a state, or a congressional or school, is problematic.

The authors call this a part of their "small policy inference" approach. The approach uses school districts, ranked by opinion regressed on measures of demographic and geographic characteristics. The results of this regression form the independent variable, opinion, which is the basis of another regression in which expenditures are the dependent variable. The discussion of this analysis would be improved by sensitivity to the econometric and political science literature on the "error in variables" problem.

Michael Berkman and Eric Plutzer have provided us with a careful analysis of important issues that inform democratic theory, empirical analysis, public policy, and education in the United States. It needs to be on reading lists in all of those fields in political science and the other social sciences.

Framing American Politics. Edited by Karen Callaghan and Frauke Schnell. Pittsburgh: University of Pittsburgh Press, 2005. 264p. $27.95.

The Mediated Presidency: Television News and Presidential Governance. By Stephen J. Farnsworth and S. Robert Lichter. Lanham, MD: Rowman & Littlefield, 2006. 236p. $60.00 cloth, $22.95 paper.

— David M. Ryfe, *Middle Tennessee State University*

Researchers generally agree that media frames are an outcome of what Tim Cook (in *Making Laws and Making*

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News, 1989, 169) calls a "negotiation of newsworthiness" between elites and reporters. The idea is that each side to this exchange brings a set of resources to bear on the framing of issues and events: Elites control access to information; reporters control access to the news. Since neither can exercise complete control over the process, the participants seem destined to engage in a never-ending struggle. These two recent books buttress and refine this proposition, and also challenge it in some important respects.

In their analysis of presidential news coverage, Stephen Farnsworth and S. Robert Lichter analyze an extensive database of news stories. Spanning the presidencies of Ronald Reagan to George W. Bush, and including coverage from the network evening newscasts, the *Washington Post* and *New York Times,* and four regional newspapers, their corpus represents more than 100,000 news articles about the federal government. The authors devote special attention to coverage of economics, foreign policy, scandal, and terrorism.

Much of what Farnsworth and Lichter find in these data supports the "negotiation of newsworthiness" concept. For instance, they observe that when reporting on the federal government, reporters pay very close attention to policy elites. There is some variance across medium and over time—television news appears to use ordinary people as sources slightly more than print news—but the numbers are still highly exaggerated in favor of officials and experts: Roughly 80% of sources on network television news are public officials and experts, and the numbers for both national and regional print news are about the same (pp. 39–40, 157–159).

Most of the essays collected by Karen Callaghan and Frank Schnell also examine politicians and other policy elites in a framing battle with one another. For example, their analysis of gun control frames in the aftermath of 9/11 shows that political entrepreneurs used the disaster to engage in an effort to reframe the issue. This activity led to changes in media frames because reporters pay close attention to such political entrepreneurs. Drawing on a content analysis of the ABC and CBS evening news, Callaghan and Schnell show that shifts in media frames of gun control closely track with the activities of policy entrepreneurs.

Farnsworth and Lichter's study also buttresses a long-standing finding that of all policy elites, the president has an extraordinary advantage in shaping media frames of issues and events. Nearly 80% of network nightly news coverage in their corpus, and 70% of national and regional print news, concentrate on the executive branch. In the aftermath of 9/11, these numbers were, if anything, slightly more exaggerated. If public officials dominate news about the federal government, then the presidency takes the lead in this exercise.

Two contributions to Callaghan and Schnell's volume take the next step to connect media frames to public opinion. Paul Kallstedt correlates American public opinion on racial policy preferences with a content analysis of *Newsweek.* Over a 40-year period, he finds that many factors have influenced public opinion on this issue, but "over-time drifts back and forth in public opinion on race are, to some extent, a function of the way that the national press has framed its coverage of racial issues" (p. 177). In an experimental study of public opinion regarding juvenile crime, Frank Gilliam and Shanto Iyengar demonstrate the same point. By varying news accounts, the authors succeed in stimulating different opinions about the issue.

Donald Kinder and Thomas Nelson's essay in Callaghan and Schnell turn the conventional worry about the power of media frames over public opinion on its head. They argue that strong media frames are not a problem at all; in fact, such frames are essential if ordinary people are to form opinions about most issues. Drawing on question-wording experiments inserted into the 1989 National Election Study (NES), Kinder and Nelson show that respondents were more likely to offer stable opinions when confronted with strong issue frames. Their explanation is that, on most issues, people struggle to form coherent judgments, not because they have no opinions but because they have too many. "The American political mind," they write, "is not so much empty as it is teeming with potentially relevant considerations" (p. 105). In such situations, strong issue frames help people organize and articulate their views.

Of course, as the negotiation-of-worthiness concept implies, the media place limits on the ability of elites to define issues and events. In their contribution to the Callaghan and Schnell volume, for instance, Kim Fridkin and Patrick Kenney argue that challengers in Senate election campaigns have almost no hope of shaping media coverage of their campaign messages. Reporters either ignore challengers entirely or frame their campaigns according to criteria that lie outside their control. Perhaps more surprising, incumbents fare little better. Unless incumbents broadcast extremely negative portrayals of their opponents, Fridkin and Kenney conclude, news frames generally respond to the closeness of the election and the presence of competing campaigns (pp. 74–75). Once in office, elected officials also face a seemingly intractable hostility from the press. Farnsworth and Lichter find that, over time, roughly two-thirds of the coverage in their sample contained a negative tone. This negativity was present over time and across presidencies. As the authors point out, the majority of these evaluations were based on job performance criteria rather than personalities, but still, when reporters express hostility toward policymakers two-thirds of the time, issue framing becomes difficult, to say the least.

In another contribution to the Callaghan and Schnell collection, Teena Gabrielson demonstrates that the ability

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of policymakers to frame issues and events can be narrowed by still other factors. These include the public mood, partisan composition in Congress, and the organization of interests around issues. These variables contribute to what she refers to as the institutionalization of frames around particular issues. "Once a frame is institutionalized," Gabrielson argues, "it becomes more difficult [for elites] to dislodge or replace it" (p. 87).

Taken together, these data support, and perhaps refine, the idea that elites may set the public agenda, but they do not control precisely how that agenda will be framed. Contextual variables, such as the public mood and other political actors—including the news media—have a role to play in issue framing.

Perhaps the most startling finding in these two books, however, belongs to Farnsworth and Lichter: From 1981 to 1993, news coverage of government dropped by half on network news. It recovered slightly after the events of 9/11, but even then, total coverage was 30% less than in 1981 (p. 32). These numbers are roughly the same in the *Washington Post* and *New York Times,* and even lower in regional newspapers (pp. 148–49).

The finding raises the question of why news media are paying less attention to government. Farnsworth and Lichter argue that a shrinking news hole on the nightly news is surely one cause. From 1981 to 2001, the television news hole shrank from just over 22 minutes to 18 minutes 30 seconds (p. 33). Presumably, the extra four minutes have been given to advertising, suggesting that economic forces are one explanation for the decline in coverage. There may be other factors at work as well, but the simple fact is that we do not know. Research on news production lags behind work in other areas of media analysis. It is telling, for instance, that only one of the essays in Callaghan and Schnell's collection—Michael Delli Carpini's comparison of mainstream journalists' conception of news to that of public journalism—focuses on dynamics within the news media. Even here, Delli Carpini relies on a reading of secondary sources.

The finding also implies that the negotiation-of-newsworthiness concept may be in need of some revision. At some point, the term "negotiation" loses meaning when one participant in an interaction begins to systematically ignore the other. Whether the interaction between elites and reporters has reached that point is an empirical question.

Overall, these books are excellent additions to the literature on media politics. Neither tackles perhaps the most important pattern of national political news coverage—the disengagement of reporters from officials. Indeed, in their discussion of the future of media framing research, Callaghan and Schnell do not even address the issue. Having said that, Farnsworth and Lichter's study presents one of the most systematic content analyses of national political news to be published in recent years. Moreover, Callaghan and Schnell's collection ably probes the notion of media frames—a concept that, given present trends in national news coverage, will likely remain vital to the study of news media for the foreseeable future.

Who Leads Whom? Presidents, Policy, and the Public. By Brandice Canes-Wrone. Chicago: University of Chicago Press, 2005. 192p. $47.50 cloth, $18.00 paper.

— Georgia Duerst-Lahti, *Beloit College*

The good news: The public actually does influence policy decisions at least some of the time, and presidential demagoguery is not pervasive. The unremarkable: We can only judge these facts for issues within the scope of polling, and presidents act strategically when they attempt to influence policy.

Brandice Canes-Wrone takes up important issues of presidential demagoguery and pandering, and convincingly brings coherence and clarity to often contradictory findings from a large field of scholarship. She directly addresses multiple sets of literatures and a range of plausible alternatives to explain presidential decisions to have strategic incentives go public, along with the normatively important question of when doing so constitutes pandering. Using formal modeling and extensive empirical testing, she provides evidence contrary to recurring themes in presidential persuasion scholarship. She demonstrates that presidents rarely cater to ill-reasoned and transitory public opinion that they believe is not good for the country, and that in going public, presidents attempt to shift Congress and/or the public on policies they want, not just manipulate public opinion. Saying so far too simply, Canes-Wrones identifies circumstances under which presidents have strategic incentives to go public because doing so leads to legislative success. She also finds that presidents pander only in certain circumstances.

The author succeeds in her wish to say something novel and precise about presidential demagoguery and pandering by exhaustively examining processes of policymaking and crossing subfields dedicated to institutional elites and the mass public. Here, formal theory successfully meets data, in an approach to emulate. This book also contributes to the growing literature that applies formal theory to political history, improves upon explanations of presidential behavior, and addresses the enduring normative concern of presidential pandering and executive demagoguery. The methodology she employs is particularly suited for future applications, although the evidence used may need to be adjusted. Her approach and findings certainly offer a potent tool for political strategists to predict when a president should go public and a means for pundits and analysts to critique presidential behavior with greater accuracy. Her primary contribution is to make formal and precise the ways that structural conditions shape the politics of public appeals (p. 21).

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Organizing her book into two parts and eight chapters that include lucid summaries, Canes-Wrone systematically demonstrates the merits of her formal theories. Part I addresses a theory of public appeals and Part II a theory of contingent pandering. Despite being inescapably dense, the writing is remarkably comprehensible for this genre of study. After laying out each formal theory in detail, specifying all assumptions clearly, and addressing every conceivably relevant literature, she tests the theories using national speeches of presidents from Dwight Eisenhower through Bill Clinton, recurring budget issues, polling data, three case studies, the election cycle, an enormous data set of presidential decisions, and more. She distinguishes between domestic and foreign policy, traces several policies over several presidents, and combines veto models with a role for the electorate. One is left with a firm sense that she has covered all bases and reached important and valid conclusions.

Who Leads Whom? nonetheless suffers three shortcomings worth noting, although they do not unduly harm its contributions. First are the obvious problems that derive from formal theory. This approach gains precision by simplification. For example, the model relies upon a unitary voter and singular Congress, assumes that actors prefer choices equally, and does not take into account the congressional capacity—albeit it a weaker one—also to appeal to the public. Findings such as "success from appealing to the public depends on strategically choosing the initiatives he advocates" (p. 48) or "he has the incentive to publicize initiatives that are already popular" (p. 49) seem much ado about something far simpler than would require this high-powered methodology.

Second, the study relies upon discretionary national speeches as its primary measure of presidential attempts at influence. Since Ronald Reagan, television networks have been more discriminating as to when or whether to cover prime-time speeches. Further, so many channels are now available that speeches no longer achieve "national" status. While appropriate for historical analysis of the time frame, changes in broadcast technologies, and presidents' responses to them—taking a message to an entertainment venue, moving speeches out of the White House but still achieving national coverage—this measure likely will prove not only a product of its time, but also of technology and reverence for the president. The methodology nonetheless remains sturdy enough to insert other national-attention events, such as widely covered speeches "of the week," employed often by G. W. Bush. The form of public appeal will need to adjust to carefully orchestrated settings, with greater network mediation if the approach is to remain potent. This observation does not, however, detract from the findings here.

Third, the title of this book misleads. Readers will not learn much about leadership. Only indirectly is the topic covered, and then, not until page 105. Canes-Wrone uncritically employs the term "lead" in the title and in naming Part II and Chapter 6. In doing so, she keeps company with many presidential scholars who seem to assume that anything a president does constitutes leadership. Given the title, one could wish for more thought about the elements and processes of leadership. The author juxtaposes policy pandering with policy leadership and defines the latter as a president pursuing "the policy he believes will promote societal welfare despite a lack of public support" (p. 105). Hence, she reduces leadership to the act of pursuing a policy even though the public does not yet support it. She contrasts this with "democratic leadership," "which occurs when a politician moves public opinion toward his or her policy position" (p. 105, n. 6). Yet, to move public opinion is invariably part of policy leadership as defined, precisely because—as she found in Part I and in her case study of Reagan (pp. 150–56)—public opinion can influence Congress on policy. Instead of focusing upon leadership per se, she distinguishes between pandering and leadership. Given the difficulty of ascertaining presidential motives and discerning pandering from presidential and public concurrence, this research remains important, even though it says little about leadership.

Choosing Your Battles: American Civil–Military Relations and the Use of Force. By Peter D. Feaver and Christopher Gelpi. Princeton: Princeton University Press, 2005. 268p. $22.95.

— Emily O. Goldman, *University of California, Davis*

In their book, Peter Feaver and Christopher Gelpi have several agendas. They succeed at some better than others. Their first goal is to establish the fact of a civil–military opinion gap over the use of force. They find that civilian elites are more willing to expand U.S. foreign policy goals to include nontraditional missions related to human rights, whereas military elites are more supportive of a traditional Cold War foreign policy agenda (p. 42); and that nonveteran civilian elites are more willing to use force incrementally, whereas elite military officers support the decisive use of force enshrined in the Powell Doctrine. Both insights have been documented in the case study literature, but Feaver and Gelpi bring large-n analysis and original survey opinion data to bear on the topic.

The authors' second goal is to link the civil–military opinion gap to foreign policy behavior, and it is one of the major theoretical contributions of the book. They demonstrate that systematic differences in the ways civilian and military elites view the use of force have had a profound effect on the propensity of the United States to initiate conflict between 1816 and 1992: "As the percentage of veterans serving in the executive branch and the legislature increases, the probability that the United States will initiate militarized interstate disputes [MIDs] declines by nearly 90 percent," and "once an MID has

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been initiated, the higher the proportion of veterans in the government, the greater the level of force the United States will use in the dispute" (p. 7). There is an extensive body of literature that argues how other variables—including distance, military capabilities, polity type, system polarity, and alliance ties—affect the propensity to initiate international conflict. Controlling for these alternative explanations, Feaver and Gelpi demonstrate a strong correlation between the civil–military opinion gap and initiation of the use of force. The impact of elite military experience often outweighed the impact of these other variables. The authors have introduced an explanatory variable that must be accommodated by the larger literature on conflict initiation and behavior.

The third goal of the book is to connect civil–military preferences over the use of force to the willingness to accept casualties. Feaver and Gelpi argue that the view of a casualty-phobic American public is a myth; "the public is defeat phobic, not casualty phobic" (p. 97). Members of the mass public are more willing to tolerate casualties than are elites, and this is consistent across mission types. Military officers show high support for realpolitik missions, and higher tolerance for casualties in those missions as well. The mass public shows markedly higher tolerance for casualties in those missions than the conventional wisdom would lead one to expect. On interventionist missions, civilian elites and the public are more willing to tolerate U.S. casualties than are military officers. Their data show that the gap in casualty tolerance is civilian versus military, rather than mass public versus elite.

The authors argue that civilian elites are tying their own hands by assuming that the public is reflexively casualty phobic. The public does not demand cost-free military operations, just successful ones. This is one of the book's more provocative claims. It flies in the face of a vast literature, including some of the classics on public opinion and war, which assert that public support declines as casualties rise, as well as a more recent study that found the public to be more concerned with casualties than defeat in cases where the U.S. stakes were judged to be relatively unimportant. (Eric V. Larson and Bogdan Ssavych, *American Public Support for U.S. Military Operations from Mogadishu to Baghdad,* 2005).

This is also one of the least satisfying assertions of *Choosing Your Battles.* What does it mean to say that the public supports missions that are successful? Perceived success could be "if the mission is successful, or there is a reasonable expectation of success, or at least the political elite make a plausible case to the public that success is within reach" (pp. 156–57), or "when victory is in sight" (p. 197). But these are decidedly different. Feaver and Gelpi even concede that "[m]uch depends on our coding of successful and unsuccessful military operations" (p. 144). Their formulation begs the question of how the public knows a mission is succeeding when they are in the midst of it. In questioning respondents about their willingness to tolerate casualties, the authors asked them to assume that the mission would be a success (p. 107). We can assume that the public is rational, and can make cost-benefit analyses. The question is what information the public is using to gauge operational and strategic success.

Feaver and Gelpi would have us believe that it is mostly a question of presidential messaging, of a president mobilizing public support by framing casualties as a necessary price for victory (which Bill Clinton failed to do in Somalia). This belief underlies the current Bush administration's National Security Council document *Our National Strategy for Victory in Iraq,* a largely public relations document designed to mobilize public support by demonstrating that the administration has a strategy to win. (Incidentally, it is also a document widely attributed to Peter Feaver, who joined the Bush NSC as a special advisor in June 2005.) Absent effective presidential framing, casualties will signal that the operation is failing.

The problems with this formulation are several. First, it depends on what expectations the president creates, how he defines success, and whether the administration's definition of success shifts over time. Second, it begs the question of where the public is getting its information and how those other sources frame casualties. The public does not take cues only from political leaders. The media are a crucial source of information, and at least with respect to the current war in Iraq, have been accused of overreporting bad news, underreporting progress that is being made, and reporting facts but not putting them into context.

This argument is not the crude version of the CNN effect—the public responding viscerally to gruesome pictures—which Feaver and Gelpi reject (p. 152). Rather, it hinges on the reporting of facts without context, or exaggerations and misinformation that are often the hallmark of chaotic situations. Yet these reports are likely to be seen by the public as less biased because they come from nongovernmental sources. Finally, it is just not clear how well the public can judge the importance of various military engagements or arrests, nor how they can judge progress in a global war on terrorism that is waged largely in the shadows with only occasional evidence of success.

So what it means when the authors say that the public will tolerate casualties if the mission is a success remains ambiguous. This may be why they published "Success Matters: Casualty Sensitivity in the War in Iraq" (*International Security* 30 [Winter 2005/6]), where they present evidence on the public's understanding of success in Iraq. But there is still a difference between the public's beliefs about what success is and their beliefs about whether we are succeeding. That depends on the information they are using to judge success.

Feaver and Gelpi end up being quite cautious in their claims, noting many caveats in what they can extract from the data. The crux of their argument on casualties and

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public support is that "the baseline of support is higher than elites believe—even civilian elites who themselves express a high support for casualties" (p. 146). It consists largely of 15%–40% of the pubic that are "defeat phobic." This is the group that the president must reach when he lays out his strategy for victory. However, the criteria for success must be clear, and more importantly, verifiable by nongovernmental sources.

Death by a Thousand Cuts: The Fight over Taxing Inherited Wealth. By Michael J. Graetz and Ian Shapiro. Princeton: Princeton University Press, 2005. 392p. $29.95 cloth, $19.95 paper.

— Erik M. Jensen, *Case Western Reserve University*

Michael Graetz and Ian Shapiro ask an intriguing question: "Why would anyone who could see straight imagine that a tax paid by only 2 percent of the wealthiest Americans [the estate tax] could be abolished?" (p. 12). But legislation in 2001 did just that, at least temporarily. (If Congress does not act again, however, the estate tax, scheduled to expire in 2010, will revive in 2011.)

To be sure, the 2% figure does not include those who spend time and money successfully avoiding the tax. Still, for most of us, having an estate large enough to be taxed is only a nice dream. Until recently, "reform" had been directed not at repeal but at things like protecting family businesses. Then, in the late 1990s, all of a sudden—or so it seemed—complete repeal became a possibility. And those who should have supported the tax were unprepared.

Similarly counterintuitive results have occurred before. For example, Susan Murnane ("Selling Scientific Taxation: The Treasury Department's Campaign for Tax Reform in the 1920s," *Law & Social Inquiry* 29 [Fall 2004]: 819–56) has chronicled how, when the income tax applied only to high-income taxpayers, Treasury Secretary Andrew Mellon was successful in making lower rates politically popular. Even Mellon, however, was unable to get the estate tax repealed; doing so took another 75 years.

Graetz and Shapiro describe how this occurred. Parts of *Death by a Thousand Cuts* are difficult slogging—taxation can be sleep inducing—but, for the most part, the book is riveting. It is a brilliant case study, and it is packed with essays on public opinion, the rise of conservative think tanks, and much more. It is a particularly rich source for students of legislative process, including a discussion of policymaking in the shadow of budget reconciliation demands.

Some of the reasons repeal succeeded have become part of conventional wisdom. Characterizing the levy as the "death tax" was a stroke of genius. Death affects us all, and slogans like "You shouldn't have to visit the undertaker and the taxman on the same day" (p. 81) resonated. And repeal proponents artfully used "horror stories" (p. 51) to illustrate the potentially destructive effect of estate taxation on family businesses. For example, a black Mississippi tree farmer's concerns about the tax (even though it could not possibly have applied to him) made for powerful politics. The public cannot evaluate technical arguments—economists were divided on the merits anyway—and "stories trump science" (p. 221). (Opponents of repeal could have used stories, too, of shiftless rich kids who do not deserve silver spoons, but they did not.)

The labels and storytelling were not everything, of course. The public did not care much one way or the other about the estate tax. (One reason Democrats were lulled into thinking repeal could go nowhere was the low priority taxpayers gave to the tax.) But those who did care cared very much and put together an imaginative strategy for repeal. They separated consideration of the estate tax from overall budgetary matters and treated the tax, standing alone, as an issue of *fairness.*

The importance of coupling fairness with the economic case for repeal cannot be overstated. There are good reasons to be skeptical of the tax—resources are wasted in avoiding it, the tax affects incentives to save for the kids' futures, and so on—but it was mind-boggling that repeal came to occupy the moral high ground.

Yet that is what happened, accompanied by some surprising demographics. One of the pervasive themes in the book is the extent to which Americans overestimate their places in the economic hierarchy and accept a conservative vision of social mobility. Many lower-income persons thus opposed the estate tax on fairness grounds, as did many African Americans. For those who had just made it, or hoped to do so soon, the tax was just unfair. And Bob Johnson, the first black billionaire, made a practical (as well as self-serving) argument: The tax takes away seed capital that black businesses require to succeed.

Did money matter in repeal? Yes, of course, but not because votes were being bought. In fact, Graetz and Shapiro conclude that campaign finance laws had, and could have had, no effect on their story: No significant amount of campaign contributions related to the estate tax. Nevertheless, many congressmen have powerful constituents who care about the tax, and, most important, money funded the organizations, including think tanks like the Heritage Foundation, that carried the political message across the land.

From a lawyer's perspective, *Death by a Thousand Cuts* teaches a valuable lesson about the limits of legislation. No matter how well intentioned, a statute must be comprehensible to be administered. Recognizing that the estate tax could destroy small businesses, Congress in 1997 had provided protection for so-called Qualified Family Owned Business Interests. That goal was praiseworthy, but the result (a myriad of rules distinguishing qualified from unqualified interests) was a monster (or, if you prefer, a tax lawyer's opportunity). The failure of piecemeal corrections actually furthered the push for complete repeal.

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This is all interesting, a cynic might say, but why should the 98% of us who are not subject to the estate tax care? It is not as though a lot of federal revenue is at stake. Here is Graetz and Shapiro's biggest point: What is dying by a thousand cuts is not the estate tax itself but the very idea of progressive taxation. (Graetz, formerly Assistant Secretary of the Treasury for Tax Policy, wrote eloquently about this in *The Decline [and Fall?] of the Income Tax,* 1997.) Repeal of the estate tax was but one step in a process of reorienting American government, of shifting taxation away from wealth and capital income. That goal is debatable—Graetz and Shapiro are skeptics—but if progressivity is to disappear, it should result from a discussion on the merits, not from a thousand discrete cuts.

Prophetic Politics: Christian Social Movements and American Democracy. By David S. Gutterman. Ithaca, NY: Cornell University Press, 2005. 236p. $34.95 cloth, $18.95 paper.

— James R. Martel, *San Francisco State University*

Are religion and democracy hopelessly incompatible forces in our time? David Gutterman would like us to believe that they are not. In this book, he examines both recent historical and contemporary examples to show how religion can either thwart or support democratic politics, depending upon how it is practiced. He looks at four Christian "social movements": the revivalism of Billy Sunday in the early twentieth century, the ministry of Dr. Martin Luther King, Jr., in the mid–twentieth century, and the Promise Keepers movement and Jim Wallis's Call to Renewal in our own time. Gutterman shows that whereas the movements of Billy Sunday and the Promise Keepers are essentially antidemocratic (and also "antipolitical") in nature, the examples of Martin Luther King, Jr., and the Call to Renewal demonstrate that religious social movements can enhance pluralism, mutual respect, and dialogue in American politics.

The author's helpful insight is that these movements can best be understood via an analysis of the rhetorical narratives that they communicate. Specifically, he notes that all four shape their self-understanding through the story of Exodus. Each of these movements has a wildly different take on this story: Sunday stresses the correct upbringing of Moses by his mother, King stresses how Moses learned leadership from his experiences, the Promise Keepers see Moses as a "man's man," and the Call for Renewal stresses Moses' ordinariness, how he is just like anyone else (and can therefore be emulated). It is these various articulations of the story of Moses that constitute the terrain of Gutterman's "prophetic politics."

For Gutterman, the crucial distinction among these narratives lies in whether they are framed in terms of already being in the promised land (as Sunday and the Promise Keepers do) or being still in the "wilderness" (as King and the Call to Renewal do). To consider America as "the promised land" implies that the great political questions are already settled; the only danger is corruption and degeneracy, the solution to which is personal salvation and a strong sense of "us" (saved) versus "them" (damned). To consider America to be the wilderness, on the other hand, is to see that collective action and social responsibility remain necessary and to recognize other citizens as fellow "sojourners" who require respect and recognition.

The main theorist that Gutterman evokes for his analysis is Hannah Arendt (partially as received via Lisa Disch, among others). In particular, he takes from Arendt the notion of "visiting," a kind of political interaction whereby we "recognize the constructedness of our own and others' governing narratives—and still [are] able to engage in politics" (p. 38). Rather than override the other with our projections (as Sunday and the Promise Keepers implicitly do), visiting allows us to maintain a sense of both our mission and value while we engage in meaningful dialogue with others in our midst.

In general, *Prophetic Politics* is a very engaging text; the framework of using the Exodus narrative helps make for clear comparisons, and this book is exactly what political theorists should try to write more often: a work that engages with contemporary politics using political theory (in this case the work of Arendt) as a basis of discernment. The book is also very timely insofar as it reflects a growing belief on the part of more progressive elements in the United States (or at least the Democratic Party) that it is not wise to cede religion and "values" to the right wing (an argument that is encapsulated by Wallis himself via his best-selling *God's Politics: Why The Right Gets It Wrong and the Left Doesn't Get It,* 2005).

Before we all jump on this bandwagon, however, Gutterman's book offers, perhaps unintentionally, as many cautions as it does means by which to make discernments among various religious movements in this country. Besides the ennoblement of the not-uncontroversial Wallis via the analogy to King, *Prophetic Politics* expresses some deep ambivalence on the author's part. He expresses this ambivalence directly when he writes that he is "ultimately ambivalent" about the "challenge religion offers democratic politics." Yet he is "unwilling to cede prophetic politics . . . to anti-democrats" (p. 19).

This ambivalence is also expressed through a series of hesitations on the author's part. For example, he is indecisive about whether to consider the negative characters in this book to be "political actors." He tells us, for example, that Sunday's theology is "surely political" but not part of "democratic politics." He ends up stating that Sunday offers us an "anti-political prophetic politics" (p. 69), a term redolent of the author's ambivalence.

Another hesitation comes when Gutterman remains uncertain as to whether the movements he prefers are revolutionary or reformist. He argues that there is nothing wrong with being reformist, but he seems to accept King's

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claims to being a kind of revolutionary and offers (against Sacvan Bercovitch) that something (radically?) new can be produced via prophetic politics.

This ambivalence on the author's part may even extend to the notion of visiting itself, insofar it remains uncertain how much visiting allows for a genuine encounter with the other. As we have seen, Gutterman tells us that visiting involves "recogniz[ing] the constructedness" of our own and others narratives, something that Billy Sunday and the Promise keepers clearly do not do. Yet, given the primacy of rhetoric in this analysis, it might not be so much that visiting allows a genuine openness to others' stories as it might serve as a construction that makes us seem open to others' stories. In such a case, openness itself is part of what we construct (just as Sunday and the Promise Keepers construct a relationship with the other that seems closed).

This book reflects much of the bafflement and worry of the liberal Left about the issue of religion in American life, and it is a brave attempt to face that issue head-on. But in many ways, in its ambivalence, the book shies away from its own rhetorical insights insofar as it continues to treat the darker variants of American religious practices as somehow mysterious and opaque. We might recognize that the very idea that Sunday and the Promise Keepers are antipolitical (even if "politically" so) is part of the rhetorical power of their variant of religious discourse, a power to delineate a particular version of publicness that works to their own advantage. Rather than see religion as being somehow different from politics (in which case we dismiss or tolerate it, but also fear it), we might see it as a style of politics, a style of persuasion that is as vulnerable to critique and counterpersuasion and as fungible as any other. We do not have to argue that someone's practices are antipolitical to dislike their politics, and we do not have to think we have actually encountered the "other" in order to have a sense of relationality that is politically meaningful (to us, anyway). Nor do we have to write off great masses of Evangelical Christians from the public realm (which Gutterman clearly does not want to do) when it is clear that they are busily acting in and upon the very realm that we uncomfortably share with them.

Political Disagreement: The Survival of Diverse Opinions within Communications Networks. By Robert Huckfeldt, Paul E. Johnson, and John Sprague. New York: Cambridge University Press, 2004. 272p. $70.00 cloth, $26.99 paper.

— James L. Gibson, *Washington University in St. Louis*

The political science world today is filled with "must-read" books, and so it is with trepidation that I seek to add another tome to the list. Nonetheless, this new study by Robert Huckfeldt, Paul Johnson, and John Sprague addresses an important problem and does so with such innovative and well-executed theory and data that I have no doubt whatsoever that the book deserves such recognition; indeed, it should is a must-read for all social scientists interested in how democracies can be sustained. This is a bold claim, so let me explain.

The problem the authors address is one of the most important challenges to democracy today: the need to keep discussions among competing political viewpoints alive. The marketplace of ideas in all democracies is threatened by homogeneity (of a compartmentalized sort), the cornerstone of which is strong (or "bonding") network ties. Unless some mechanisms can be identified by which weak ties ("bridging" ties) can be created across extended and diverse social networks, the vitality of the discussion essential to democracy is at risk.

The research reported in the book is extraordinarily innovative. First, the authors conduct a statistical analysis on what seems to be one of the best social networks surveys ever conducted in political science (in part because the authors interview not only individual respondents but also other network numbers named by the respondent). Many innovations can be found in the analysis, including an effort to measure the accessibility of information other members of the social network (via the speed of responses), and an effort to incorporate campaign dynamics into their equations. Throughout, the reader cannot help but be impressed with the great care and insight with which various processes are disassembled and then rendered into microlevel concepts and hypotheses tested through rigorous analysis.

Many challenges to conventional wisdom emerge from this portion of the book. Disagreement seems not to produce dissonance for citizens, leading to the avoidance of political material (even if this is so because many people do not care enough to be alarmed). Nor is political disagreement inevitably associated with shunning. The authors' conclusion challenges conventional wisdom: "In short, political differences and disagreements are easily reconciled, readily accommodated, and simply explained by many citizens, and hence they are unlikely to be dissonance producing" (p. 96). And while debate will surely surround their claim that unexpectedly widespread network heterogeneity characterizes discussion groups in the United States today, the authors develop an important enigma: More network diversity seems to exist than would be expected on the basis of theories of network homogeneity, political persuasion, and Elizabeth Noelle-Neumann's "spiral of silence."

Yet *Political Disagreement* is more than the sophisticated statistical analysis that we have come to expect from the reports of Huckfeldt and his colleagues. Beginning with Chapter 6, the authors employ agent-based models in an attempt to capture the dynamics of network homogeneity. The modeling effort is designed to "ascertain the aggregate impact of various individual strategies, behavioral rules, and responses" (p. 128).

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Chapter 6 presents network analysis that is based on what I consider to be an entirely implausible assumption about persuasion. Chapter 7 is vastly more interesting, owing to the use of a different assumption about persuasion under the instance of heterogeneity. The authors rely on the idea of an autoregressive process—"people may understand political messages within a context created by their own preferences as well as by the preferences of others" (p. 74). The driving assumption in this model is that people change their political views only in conjunction with (or in interaction with) the views of those in their social networks. In short, "people respond [to diversity in their environments] by comparing new and divergent opinions against the opinions of others within their communications network," and individuals become evaluators "of information received through a successive autoregressive process of social interaction" (p. 161). With this (and a few other) assumptions, the authors are at last able to produce an interesting model consistent with the empirically based view that network homogeneity does in fact exist.

Of course, one can still quibble with the assumptions of the model, and especially the inattention to individual differences, but one cannot help but be impressed with the care and insight with which the model was developed—as well as the tenacity of the authors in tweaking the model until the benchmark outcomes were produced.

The overall conclusions of the book are uncharacteristically optimistic for a study of mass political opinion and behavior. Democracy can be sustained so long as network ties remain weak (low density networks) and political minorities do not become isolated. Few network analysts are so sanguine about the ability of citizen interactions to contribute to the democratic marketplace of ideas.

Of course, as good as it is, this is not a perfect book. The writing, for instance, is very redundant. One wonders about some methodological details, especially with regard to sampling and the complexities (which are substantial) of network analysis. Perhaps the most important substantive omission of the book—which is a natural consequence, given the focus on networks—is the inattention to the socially atomized (which is about 20%–25% of the American people). These folks are not just interesting because they may be relevant to politics and since we understand little about how they form their preferences. They may also perform an important function related to the homogeneity of social networks. Since atomization likely changes over one's life span, when atomized individuals join social networks, they can perform a useful role in upsetting equilibria. More generally, further thinking about social networks would profit from conceptualizing the process in more dynamic terms. Consider the very important role of marriage and divorce. Individuals get married and join social networks, which have a tendency toward homogeneity. Then along comes divorce, rendering some individuals in a state of atomization for a time. Then recoupling occurs, with the strong possibility of joining a new social network, in which one might well hold minority viewpoints. And important gender differences undoubtedly exist in these processes. I admit that these dynamics may be difficult to study and model, but they are important to analyze.

Finally, the primary, if not exclusive, focus of this book is on electoral campaigns. One wonders whether many of the findings reported here extend to other, more threatening and dangerous types of political discussion, as in topics of political tolerance and intolerance. From that literature, we know many things that seem to be at odds with the authors's assumptions and findings (e.g., it is easier to talk tolerant people into an intolerant viewpoint than vice versa). Future scholarship will surely find it valuable to expand the model of this book to more incendiary forms of political discussion and interaction.

Huckfeldt, Johnson, and Sprague have not provided the last word on network homogeneity. Contrary views exist among network analysts on many important aspects of their inquiry. But no one interested in deliberative democracy can ignore this brilliant work by three gifted social scientists. *Political Disagreement* is a stellar exemplar of how social scientists should use theory to develop hypotheses and collect and analyze empirical evidence, while also developing formalized models consistent with and explicating the empirical evidence. The book is a profound contribution to our understanding of democracy and its continuing efforts to sustain the ever-so-crucial marketplace of ideas.

The Talk of the Party: Political Labels, Symbolic Capital, and American Life. By Sharon E. Jarvis. Lanham, MD: Rowman & Littlefield, 2005. 304p. $80.00 cloth, $27.95 paper.

— Richard Herrera, *Arizona State University*

How did the term "liberal" become a dirty word in American politics? Why are Democrats "accused" of being liberal as a campaign tactic? These types of questions motivate Sharon Jarvis's book. The language of politics, especially partisan politics, is a topic visited by political scientists and other social scientists with modest frequency, but Jarvis offers a different, and fresh, perspective that bridges public opinion approaches with rhetorical analysis. This method should prove appealing to a variety of social scientists and students and illuminate new ways of studying political language.

Jarvis makes four assumptions that also serve as themes for *The Talk of the Party*: "[First,] citizens come to know politics through discourse[;] second, citizen discourse is led, but not determined, by elite discourse[;] third, the uses of labels function as powerful shortcuts in modern life . . . and fourth, the meanings of political terms shift

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with time" (p. 3). To students of public opinion, Jarvis's framework for analyzing the branding of political parties is a familiar and useful one. This theoretical foundation is laid out in the first two chapters of the book. The next four chapters take these themes and emphasize the role of elites in shaping the perceptions of the masses with regard to the party labels, symbols, and brand names. In the final chapter, the author returns to questions that motivated the book and offers her explanation, given the preceding analysis, of how we come to see the uses of political language and interpretation that are evident in recent campaigns.

Jarvis employs an extensive content analysis as her methodology. She relies on discourse from campaigns (from journalists and presidential candidates), government (debates in Congress), and education (civics textbooks). The analysis covers five decades, beginning in 1948. She focuses her content analysis on six key terms: "Democrat," "Republican," "Independent," "party," "liberal," and "conservative." This approach allows for an in-depth treatment of the discourse surrounding the terms and their uses, and a longitudinal analysis to detect change. Implicit in this design are assumptions about the role of certain elites in "managing" the discourse of politics, or at least these terms, and the extensive reach of that discourse to the mass public.

In addition, and particularly impressive, is a valuable methodological appendix. Jarvis provides a comprehensive description of the content analysis, from sample selection to coding to interpretive tables of data. The decision making is clear and reasonable. The appendix is an excellent example of how to "show one's work" and should prove a useful resource to scholars and students alike.

One of the most insightful of Jarvis's observations is that party discourse is hierarchical. She demonstrates that political parties are mentioned, described, and associated with political elites. As she notes, this "elite nature" of party labels provides an interesting way to deal with the seeming paradox presented by scholars and others about why Americans hate politics and politicians when they are so patriotic (e.g., see E. J. Dionne, *Why Americans Hate Politics,* 1991). As the author explains in Chapter 3 and details later, "As long as these voices continue to regard party labels as elites do, then party labels' role in government is, in a semantic sense, secure, even if occasionally vilified" (p. 80).

In Chapter 4, Jarvis claims that four periods of party representations emerged from the data she collected: monolithic, liminal, fragmented, and resurgent. The usefulness of these terms to describe the over-time changes in how parties are portrayed in the media and how party leaders portray themselves is difficult, at best, to appreciate. Her labels lack identifiable and portable features. What constitutes a "fragmented" state of parties, for example? The list of societal characteristics and technological developments do not define a fragmented or any other party or period of party politics. Likewise, while the data show a high level of "party language" in the period between 1948 and 1960, why that constitutes a monolithic period is unclear. An alternative, and not new, interpretation of the data in Figure 4.1, for example, is that they support the observation that parties were strong in the late 1940s (and probably in the decade prior), gradually declined into the 1980s, and have recently transitioned (resurged?) to better fit the needs of modern politics (i.e., becoming conduits for campaign funds, providing campaign resources for candidates, etc.).

It is also tempting to conclude that it is difficult to discern a difference between Eras II and III (liminal and fragmented party periods), for example, because that difference appears to be largely in the eye of the reader. The departure from the statistical analysis and reliance on examples from presidential campaign speeches and journalistic accounts proves a much less persuasive tack. Evidence to show the usefulness of demarcating periods in the way suggested is not marshaled.

In the fifth chapter, Jarvis examines how different types of elites view political parties and how they portray them to others. This is, perhaps, the most interesting of the chapters in the book. When we read or hear legislators, journalists, educators, and presidential candidates communicate about political parties, they do so with varying descriptors and tones. In the process, the meanings they attach to the labels are a cacophony of messages to the mass public. Jarvis shows not only that the labels elites use to talk about parties change in frequency and meaning over time, a reasonable expectation, but that even at any one historical period, they do not share the same rhetoric. The implications are intriguing as much for political scientists as they are for the mass public. Stated well by the author, "political scientists feed on a richer diet of party meanings than do citizens" (p. 154).

One minor shortfall of the book is that a significant number of new concepts and variables in the author's analysis are introduced with little explanation as to their meaning and measurement. While the methodological appendix is excellent, this is one of those cases where more of the type of information contained there should be placed in the text. Terms such as "quality," "context," and "task," to name a few, are integral parts of the analysis but require a trip to the back of the book.

For political scientists versed in social psychological theories of public opinion formation (e.g., political socialization, opinion formation and change, etc.), Jarvis's contribution lies in providing views of elite discourse in real terms. For students of political parties, this book offers a rich store of detail about the way parties are portrayed by the managers of political discourse and how those portrayals have changed over time. Scholars of discourse, political or otherwise, will find an excellent example of a

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well-executed analysis of political text and the process of branding. For both groups, this book is useful as a supplement for undergraduates in courses that emphasize understanding and appreciating the importance of symbols in American political life.

The Politics of Attention: How Government Prioritizes Problems. By Bryan D. Jones and Frank R. Baumgartner. Chicago: University of Chicago Press, 2005. 304p. $70.00 cloth, $25.00 paper.

— Paul E. Johnson, *University of Kansas*

This book is about policy change in the U.S. national government. Drawing on their exhaustive collection of policy data for the last three decades, Bryan Jones and Frank Baumgartner make a very persuasive argument for their "punctuated equilibrium" theory of public policy. The authors detail a catalog of reasons why policy changes are typically small, including (among other things) individual-level "bounded rationality" and institutional properties like "friction." These many factors combine to produce policy that appears to change incrementally most of the time, but there are infrequent moments in which major change is observed. The amount of nonincremental change is linked to the nature of political institutions and to the stages of the policy process. The final stage of the process—legislative change—is costly and slow, producing infrequent change, while changes in the earlier stages are less costly and therefore more frequent.

According to the statistical argument that frames the entire book, the theory of incrementalism predicts that the distribution of policy change ought to appear "Normal." Supposing that a sum of separate random variables is a description of the policy process, as the incrementalists seem to imply; then very general statistical results indicate that the frequency distribution ought to be bell shaped. (This is one of the central limit theorems.) With that null hypothesis clearly pinned down, the authors then offer a wealth of data to indicate that the distribution of observed change is not Normal. Their whole argument is nicely summarized in Figure 4.14 (p. 111), which shows that the distribution of U.S. national budget changes is a sharply spiked distribution with long tails. As they demonstrate, the severity of the deviation from normality is predictable according to their theories of institutions and policy process.

The authors have wrestled with a set of very difficult research problems that have taken them off the beaten path in political science. They are apologetic in their presentation, asking for the reader's patience in considering the shape of statistical distributions. Possibly fearing the alienation of readers who lack an interest in mathematical details, they make their statistical argument in an entirely verbal format. Their effort to keep the presentation clear and readable is certainly successful, but some of the more mathematically inclined readers may feel that a more rigorous presentation would be suitable. Even key statistical concepts, such as the Normal, Pareto, or double-exponential distributions, are not given explicit mathematical definitions. The terms "kurtosis" and "leptokurtotic" are introduced on page 111, but formal definitions are not found until a footnote on page 181. One might wish that there were a technical chapter or appendix with formal definitions of the distributions, a mathematical derivation that justifies the use of these particular distributions, and a formal comparison with similar models from other fields (especially Zipf's Law). In some sections of the book, the authors seem to hint that policy change might reflect a mixed stochastic model, one which combines draws from a Normal distribution during "incremental times" and some other distribution during the infrequent periods of change. A formalization of such a characterization would be most welcome.

Without a doubt, this book will be very useful in graduate seminars. It touches on many of the biggest ideas in social science, ranging from theories of individual behavior and rationality, to "middle level" phenomena like political organizations, to the highest level, institutions and policy change. It is also written beautifully. The simple elements of writing—paragraph construction and word selection—are accomplished with so much grace that the presentation is more reminiscent of a trade paperpack than an academic monograph. The central contentions are clearly spelled out, without mathematics. The early research on incrementalism is described in such a rich and insightful way that the presentation should serve as an example for dissertations for some time to come.

Some comments in the early portion of this book might distract the reader from its central themes, or even give the reader the wrong impression. There are derisive comments (without argument or citation) about the Bush administration's policy toward Iraq (p. 8) and former Attorney General John Ashcroft (p. 15) that seem out of place. The book is framed around the idea that humans are uncertain about the truth and have a difficult time managing decisions, and yet the authors seem certain of their own political judgment. Similarly, on page 17, one finds the assertion "This means that learning is not Bayesian" without an effort to explain what the Bayesian approach might be or how exactly it is inconsistent with the authors' description of learning. The frequency of these distracting comments is much lower after the first chapter.

The most resounding theme of the book, which is established persuasively, is that the distribution of observed change (in a variety of indicators) is not Normal. With that contention firmly established, we are left with plenty of research to do. While we can safely reject the elementary version of the incremental theory, we are not confident about what factors cause spiked distributions. Political scientists, along with economists, sociologists, ecologists, physicists, geologists, and geographers, are confronted with

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the apparent ubiquity of power law distributions in aggregate data. In a very real sense, the existence of spiked distributions is an overdetermined outcome, one that is consistent with many different models of the behavior of individuals, groups, and institutions. Like all truly valuable research projects, *The Politics of Attention* thus answers some questions definitively and also clearly frames questions for future study.

It Takes a Candidate: Why Women Don't Run for Office. By Jennifer Lawless and Richard L. Fox. New York: Cambridge University Press, 2005. 220p. $65.00 cloth, $22.99 paper.

Women and Elective Office: Past, Present, and Future. Edited by Sue Thomas and Clyde Wilcox. New York: Oxford University Press, 2005. 344p. $21.95.

— Joyce Gelb, *City University of New York*

In comparative perspective, the United States ranks fifty-seventh among the world's nations in terms of female legislative representation. Even elections to state positions, where women have gained over 20% of seats, have lagged in recent years, reflecting a kind of stagnation in the opportunity structure for women at all levels of the political system. However, when women run for office, they win in the same proportions as their male counterparts.

What, then, explains the poor showing of American women in elective office? These two volumes seek to explain the underrepresentation of women in American politics, but from different perspectives. While to all outward appearances the present-day electoral process is "gender neutral," a pervasive, subtle form of discrimination continues to exist in the electoral arena.

In *It Takes A Candidate,* Jennifer Lawless (herself a former congressional candidate) and Richard L. Fox premise their analysis on the hypothesis that men and women are not equally interested in running for office. Why does female electoral representation lag so far behind comparable professional attainment, say, in the field of law and higher education? On the basis of the Citizen Political Ambition Survey of 3,800 eligible male and female candidates in professions most suited to political office, the authors identify a gender gap in political ambition between men and women. In doing so, they discount other, conventional explanations for women's low representation, such as discrimination and incumbency. They suggest that discrimination against women in political office has become less obvious, with majorities of men and women supporting female candidacies. However, the war on terror after 9/11 may have weakened their appeal, at least for higher executive-level positions. (In contrast, Jean Roth Schroedel and Marcia Goodwin in Sue Thomas and Clyde Wilcox emphasize continued voter hostility, gatekeeper resistance, and electoral arrangements as obstacles for women who seek elective office.) Nonetheless, despite the apparent decline in the overt discrimination of the past, subtler and still pervasive obstacles to women's electoral attainments persist.

Lawless and Fox's approach emphasizes the gap in political ambition that affects women's reluctance both to run for electoral office and to seek higher-level political positions. They point to a two-stage process, in which potential candidates decide first to consider and run for political office and then to seek higher-level positions. In general, as other studies have also shown, women are less likely to be politically involved than men, when their meeting attendance and contributions to campaigns are assessed. They appear reluctant to be involved in a government structure designed and dominated by men. Lawless and Fox stress the extent to which traditional family socialization and roles reflect the gendered private and public spheres that still pervade the political process.

What is the explanation for the lag in women's political ambition? For Lawless and Fox, women are less encouraged by political "gatekeepers," including party leaders, and more self-doubting in terms of political efficacy. It is the latter that comprises the crux of their argument. Women undervalue their skills and experiences, perceive major obstacles to political participation, and feel that they are judged more harshly than men by the public and media. Socialization emerges as a major factor in explaining low numbers, with family, school, and local political arrangements contributing to the marginalization of women as candidates. It would be helpful to learn more about the role of mentors and role models in recruiting female candidates, referred to only briefly in the narratives of both books.

Factors cited that do positively affect women's willingness to run include prior political experience in school and college, as well as endorsement of issues such as gay rights, abortion, and environmental policies. Perhaps surprisingly, although far more women who do participate find a home in the Democratic Party, they are no more likely than Republican women to seek political office—suggesting that the gender gap in perceived political opportunity has a nonpartisan cast. Articles in both volumes point to the greater ambition of black women seeking political office, apparently comprising an exception to the patterns of reluctant office seeking that is characteristic of other female candidates. According to these studies, black women are more likely to run for political office—perhaps because they are more ambitious and can count on support from black voter coalitions. Is this always the case? What would happen if a black man ran against a black woman?

The authors' research suggests, alarmingly, that the female antipathy to political office is still evident today among undergraduates and young women. The barriers erected by a candidate-centered system of electoral recruitment remain formidable, particularly in the absence of

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quotas and/or positive discrimination that have increased women's representation in other nations. It would be useful to gauge the effectiveness of groups that seek to encourage the recruitment and financial support of women candidates—EMILY's List, the White House Project, and the Women's Campaign Fund—in altering these patterns.

In *Women and Elective Office,* edited by Sue Thomas and Clyde Wilcox, the authors similarly address barriers to political office for women, as well as assess their effectiveness once they are in office. At least one article in this anthology, by Heather L. Ondercin and Susan Welsh, broadens the framework for analysis of the barriers, both institutional and psychic, that affect women's candidacies. This volume also addresses institutional and other obstacles that continue to subtly discriminate against women. These include funding, support from party and other elites, media treatment, and mode of entry into politics. Among the disparate factors addressed by the contributors to the volume is inequitable fund-raising for men and women, which includes differential sources of support. For women, these are more likely to be women's and community-based groups, rather than the professional and business groups that support men. Women who choose to enter political life do so in a gendered manner, usually when they are older and have raised their families. A disproportionate number are unmarried, are childless, or have fewer children than their male counterparts. Still Michael J. Epstein, Richard Niemi and Lynda Powell emphasize the similarities between male and female candidates related to office seeking and the likelihood of reelection. How can these disparate findings related to women's candidacies be explained? Perhaps they relate to the level of office seeking: local, state, or federal.

There is considerable controversy remaining about two factors related to political success for women. One relates to media coverage: Is it more negative for women or do women benefit from being political novelties and potential innovators? Another deals with women as fundraisers. Are there more barriers for women seeking campaign finance? In their chapter on paths to Congress, Ondercin and Welch emphasize women candidates as political innovations whose success rates may be improved if there are open seats, if they are running outside of the South, and if they are following in political cultures that have been hospitable to female candidates in the past. The latter point begs the question of how female candidates in such areas were initially able to successfully gain political office.

The significance of term limits as a factor constraining women's opportunities is dismissed relatively quickly in both volumes under review. Here, further analysis would be helpful, since the authors assert that the number of incumbent women forced to vacate seats has exceeded the number newly elected.

As noted, several articles in the Thomas and Wilcox collection deal with the role of women once elected to office. Since women elected are a heterogeneous group, does their presence in office bring a gendered agenda? Michelle Swers and Carin Larson emphasize the importance of women in leadership positions on the Hill; even female Republicans are more likely to support some women's issues than are men, although their issue positions are mediated by their party status and committee position.

One issue that might be explored further is the extent to which women voters support women candidates; the data in both books are inconclusive. The Thomas and Wilcox book would benefit from section headings; the present arrangement lumps all aspects of women in the political process together, which is difficult for the reader to follow.

What is the future of elected women in American politics? The prospects seem far from sanguine. Given the present ascendancy of Republican politics, the outlook for more women candidates and victors, who are primarily nominated and elected through the Democratic Party, seems to call for pessimism. With the leveling off and/or decline of women in elective office, will women be able to achieve any kind of critical mass through which to influence policy? Electoral factors unique to the United States, including single-member districts and the power of incumbency, as well as the surprising persistence of gender roles, continue to limit women's parity with men in elective officeholding. In the absence of political reforms that help to end the "political glass ceiling" for women, the prospects for more than slow and incremental change in political representation appear limited, at best.

Indian Gaming and Tribal Sovereignty: The Casino Compromise. By Steven Andrew Light and Kathryn R. L. Rand. Lawrence: University Press of Kansas, 2005. 240p. $29.95.

— Bruce E. Johansen, *University of Nebraska at Omaha*

Is gambling the answer to reservation poverty—the "new buffalo," as some Native Americans have called it? In some places, such as the Pequots' Foxwoods in Connecticut, small groups of American Indians have been enriched. In other places, such as the New York Oneida Nation in Upstate New York, gambling has provided a rather vaguely constituted polity the means with which to hire police who force dissident antigambling traditionalists from their homes. Among the Mohawks at Akwesasne, people have died over the issue.

Non-natives have always been prone to stereotyping American Indians. During the past quarter century, as many reservation Indians have parlayed their limited legal sovereignty into casinos and associated businesses, a new stereotype has emerged: the "casino Indian," rich and slick, hobnobbing with the rich and powerful and sometimes corrupt, from Las Vegas mob dons to the K Street Project's

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Jack Abramoff. Never mind that the newly rich have been a tiny minority of all Native Americans, often members of small tribes cobbled together to take advantage of huge casino clienteles near large urban areas.

The main premise of *Indian Gaming and Tribal Sovereignty,* coauthored by Steven Andrew Light and Kathryn R. L. Rand, is that gambling involves compromise for American Indians. The book is mainly a legal analysis, and so the compromise is phrased in terms of federal regulation that has been required to permit gaming. Federal recognition (and some degree of control) is required in most cases in order to establish gaming operations. In some cases (an excellent summary of California's situation is provided on pp. 66–69), state interests often come into play. While Native nations' semisovereign status limits taxation, many of the more affluent casino operations have supplied payments to states, the most notable examples being the hugely successful casinos operated by the Pequots in Connecticut.

By 2003, according to the authors, more than 350 gaming venues were being operated under Native American auspices in 30 states. Indian gaming had expanded to $17 billion a year in revenues in the United States by 2003, as gambling has permeated the entire country, emerging from enclaves in Las Vegas and Atlantic City. By 2004, citizens of the United States were placing $70 billion in bets.

Chapter 4 ("Is Anyone Winning?") contains an acute analysis of gaming's socioeconomic impacts. Discussion of gaming's impact on reservation quality of life (pp. 98–104) is especially penetrating, along with accounts of how gambling has supplied jobs and start-up capital for other enterprises. On some reservations, the authors say, gaming has helped reduce unemployment from 70% to nearly zero (pp. 87–88). Welfare expenses also have dropped sharply in such areas.

The book provides a salient description of controversies surrounding the Pequot casinos in Connecticut, Foxwoods, and the Mohegan Sun (pp. 108–10), which tap the New York City–Boston market and have become among the largest gaming venues in the world. Both are operated by small Native polities that have been widely criticized by some non-Indians (the best-known is author Jeff Benedict) as faux "Indian tribes" constituted mainly to profit from casinos. Casinos have brought problems, too, including widespread gambling addiction (pp. 93–96), domestic troubles, and crime (pp. 96–98).

As a legal analysis, this book does well. In a broader sense, however, it falls short. It has an institutional, antiseptic air about it—the feeling of motel meeting rooms and Powerpoint presentations—and so seems to lack a certain sense of reservation street smarts.

The authors miss a great deal of controversy surrounding gambling operations. The New York Oneidas' Turning Stone Casino, for example, is touted here as one of the most successful at converting casino profits into benefits for enrolled members. That is true—to a sharply defined political point. However, this account completely misses a great deal of internal dissent. A sizable number of New York Oneidas get no benefits at all because the government has disenrolled them for exercising their critical faculties.

This book also misses conflict over gambling among the Mohawks. Mohawks are mentioned once, and in error, on page 101, as subject to an agreement that was reached "to settle the tribe's land claims and to allow the Mohawks to open a casino in the Catskills." That "agreement" fell apart.

Additionally (and this is hardly the sole province of this book), too-frequent use of the words *tribe* and *tribal* as generic terms for Native Americans can be irritating. Other cultures have been (or are) constituted as tribes around the world. A "tribe" is generally a small unit of sociopolitical, legal, and economic organization. The term is often used without thinking (in this book, with unusual frequency) for peoples better described as "nations" or "confederacies," especially in an historical dimension. "Tribe" was used in the nineteenth century to justify replacement of Native peoples by "civilization" under the rubric of Anglo-American real estate law ("highest and best use"). Legal analysis should be more precise with language.

Yes, certain Native peoples have improved their material lives through casinos, but some traditionalists are concerned that the cooperative, egalitarian nature of many American Indian societies is being subverted by the hypercapitalistic, get-rich-quick assumptions that underlie gambling culture. Thus, while Native sovereignty provides a legal basis for casinos (and while such businesses have been used to jump-start other ventures), concern exists that the casino culture may undermine the attributes that make Native American ways of life unique.

Uncompromising Positions: God, Sex, and the U.S. House of Representatives. By Elizabeth Anne Oldmixon. Washington, DC: Georgetown University Press, 2005. 262p. $44.95 cloth, $26.95 paper.

— Raymond Tatalovich, *Loyola University Chicago*

Elizabeth Oldmixon rightly observes that heretofore few, if any, books have been devoted exclusively to the study of congressional voting behavior and legislative sponsorship on moral conflicts. The existing body of research, including my own work, is based on scholarly articles that analyze roll call voting on abortion, mainly, with some attention to gay rights. I was not aware of any systematic analysis of roll calls on school prayer, that is, until the publication of this volume. In sum, Oldmixon's purview includes roll calls and legislative sponsorship of bills on abortion, homosexuality, and school prayer in the 103rd through the 107th Congresses.

The heart of the empirical analysis in Chapter 4 could have been published as a stand-alone article in a leading journal. Much of the remaining volume details rich case

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studies of the contested cultures of "progressive sexuality" (Chapter 2) versus "religious traditionalism" (Chapter 3) in modern America. This material breaks no new ground for scholars with expertise in these social movements, but for the novice, they are a good starting point. The author's rationale is to provide contextual background for the empirical analysis that follows. What makes this book so much richer than a simple presentation of logic regression models is Oldmixon's inclusion of personal interviews with House members and their staff. The interviews are utilized not only to inform the variable list and the direction of causal relationships but also to give credence to her results. Reading those interviews was especially informative and brought this analysis to life.

A properly replicated study over more than one Congress, like Oldmixon's, enhances the validity of the empirical findings, and undoubtedly her substantive conclusions will stand the test of time. Students of morality policy are moving toward conceptual and methodological consensus about why this policy domain is different and what variables operationalize those differences. One key distinction is that economic cleavages should not matter but that religious cleavages should. That paramount assumption is verified here. The overall empirical conclusion is that after controlling for party and ideology (Republicans and conservatives are more opposed to abortion and gay rights but more supportive of school prayer), religion matters both at the elite and mass levels.

Catholicism matters a lot more than conservative Protestantism, however, and this disconnect is the major paradox identified by Oldmixon. Both being a Catholic lawmaker and having a Catholic presence in the congressional districts exert a conservative influence on roll call voting and legislative sponsorship. Being a conservative Protestant (broadly defined) has no effect on the behavior of representatives, nor does the size of those denominations in the congressional districts. Oldmixon properly assumes that conservative Protestants are much more committed to school prayer and express a less nuanced opposition to homosexuality than Catholics, but both faiths are allies against abortion. Surveys do show that Catholics and Protestants are not dissimilar in their attitudes toward abortion. So why is conservative Protestantism not the more salient variable?

I share Oldmixon's instinct that religiosity may be relevant, but as she points out, we have no measure of religious commitment among members of Congress. On the other hand, it is not obvious to me that rank-and-file Catholics would be more religious than conservative Protestants. Ultimately, she speculates about the hierarchy of the Roman Catholic Church as compared to the more democratic and eclectic grouping of conservative Protestant denominations. If the answer to this paradox is organizational, then perhaps that could have been addressed by controlling for the location of bishops or cardinals, as well the presence of Catholic adherents in each district.

What some readers will find problematic about this study is the conceptualization of culture, admittedly a long-standing methodological issue in political science. On the one hand, the author draws from sociologists of religion to define culture as a "moral order" (p. 15) based on a shared sense of how people ought to live their lives. On the other hand, she agrees with T. Alexander Smith and myself (*Cultures at War: Moral Conflicts in Western Democracies,* 2003) that moral controversies are essentially "status" conflicts between the forces of hierarchy (of which religion is one big influence) and pressures for egalitarianism. Those "cultures" (along with individualism) were identified in the writings of the late Aaron Wildavsky. The conceptual problem of tying culture too closely to religion became obvious very early in this book. The first chapter discusses race relations and gun control as cultural conflicts, and while I agree that both involve status conflicts, most observers would be hard-pressed to identify a religious underpinning to the debate over guns.

At the same time, the concept of culture is so stretched that it seems little more than an analogue for values. Oldmixon hypothesizes that the primary influences on legislative behavior will be party, ideology, and religion "because the phenomena are reflective of underlying cultural values" (p. 28). But surely religion evokes a sense of "moral order" in ways fundamentally unlike party or ideology? There was no need to frame party or ideology as cultural phenomenon to justify their inclusion in the analysis. They should be included to assess whether the long-standing predictors of legislative voting (on primarily economic disputes) extend to moral controversies and, if so, whether they augment or displace any effects from religion. Clearly they augment religious effects, which offers a prophetic conclusion to this book. That is, the historic left–right continuum on economic issues has accommodated the "new" cleavage based on "progressive sexuality" and "religious traditionalism." Although cultural conflicts may have caused a realignment among voters, they have served more to reinforce (not to realign) political elites, not only in Congress but, I suspect, across Western Europe as well. This caveat aside, Oldmixon has authored an important book that speaks to a future research agenda for congressional scholars.

Courts, Liberalism, and Rights: Gay Law and Politics in the United States and Canada. By Jason Pierceson. Philadelphia: Temple University Press, 2005. 264p. $68.50 cloth, $22.95 paper.

The Case for Gay Rights: From *Bowers* to *Lawrence* and Beyond. By David A. J. Richards. Lawrence: University Press of Kansas, 2005. 256p. $29.95.

— Carlos A. Ball, *Penn State Dickinson School of Law*

If one assesses the maturity, as well as legitimacy, of a field of study through the scholarship that it produces, these

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two excellent books—one by an established scholar (David A. J. Richards) and the other by a relative newcomer (Jason Pierceson)—show just how far gay rights, as a field of scholarly inquiry, has come in the last few decades. Both books provide compelling arguments on behalf of the view that liberal political theory, when coupled with vigorous judicial review, can provide previously marginalized groups, like sexual minorities, with meaningful rights of equality and autonomy.

Richards's book is the more explicitly personal of the two, as he introduces *The Case for Gay Rights* with a brief autobiographical chapter describing his struggle as a young gay man, in the 1960s and 1970s, to find an ethical voice. As Richards tells it, his effort to find an ethical voice in his personal life, as he coped with his sexual orientation while living in a largely homophobic society, mirrored his struggle to find an ethical voice in his academic work, first in philosophy and then in law, which sought to argue on behalf of basic human rights for lesbians and gay men.

The use of personal narrative in an academic book raises the possibility that some might dismiss the scholarly arguments contained therein as nothing more than dressed-up personal polemics. But there is nothing of the kind here. In part, this is because the main focus of the book is not autobiography, but is instead the link among personal, philosophical, legal, and psychological analyses of the fundamental injustice that inheres in the treating of lesbians and gay men as second-class citizens. It is also because Richards is utterly convincing in analogizing between his efforts to find a mature ethical voice living as an openly gay person in a loving relationship with another man and his efforts to find such a voice in his intellectual and academic pursuits.

The primary focus of the book is on the ways in which the U.S. Supreme Court has slowly but steadily interpreted constitutional provisions and values in ways that have provided greater equality and personal freedom for lesbians and gay men. As Richards notes, it all began, shortly after World War II, with the Court's expansion of constitutional principles of free speech to further protect political activism and to narrow the definition of obscene speech. This provided lesbians and gay men, as individuals and as members of an emerging community, the space necessary to develop voices of their own. It also, crucially, gave them the opportunity to join their voices with those of other minorities who were seeking to free themselves from the chains of prejudice as codified into law. The author repeatedly returns to the basic irrationality of social prejudices (what he deems "moral slavery"), whether they be expressed in terms of racism, anti-Semitism, sexism, or homophobia. The same constitutional principles that have made it more difficult to advance the first three prejudices through public policies are now doing important work in lessening the harmful effects of government-sponsored homophobia.

In many ways, the fundamental shift in the Supreme Court's jurisprudence took place during the 17 years that elapsed between its upholding of sodomy laws in *Bowers* v. *Hardwick* (1986) and its reversal in *Lawrence* v. *Texas* (2003). So long as *Bowers* remained good law, that is, so long as the state had the constitutional authority to penalize lesbians and gay men for the most personal of choices in their intimate lives, there was no possibility that their basic human dignity would be accorded social respect. In many ways, the Court in *Lawrence* adopted the position that Richards had been advocating for many years, namely, that the state, as a matter of constitutional law, must respect the relationships and personal lives of lesbians and gay men.

Richards ends the book by placing the legal story of gay rights in a broader context that looks at the psychological question of why repressed voices sometimes seek to resist. Here, he returns to his own life, hypothesizing that his mature ethical voice is rooted in the voices of his parents, in particular his mother, who was a feminist "in practice though not in theory" (p. 148). He then suggests that other men, such as Tolstoy, Gandhi, and King, also gave ethical weight to the voices of their mothers by questioning "the role of manhood in rationalizing forms of political violence . . . and the many forms of structural injustice that depend on such violence" (p. 153).

Richards's hypothesis about the role of mothers in the development of their sons' ethical voices is interesting, though it is clearly not universally applicable. There are many men (and women), after all, who have developed such voices despite (rather than because) of their parents' views on questions of gender and sexism. It would also be interesting to take the author's hypothesis one step further by exploring the role that the growing number of lesbian and gay parents may play in the development of the ethical voices of *their* children, given that they must both "mother" and "father" those children.

While Richards's book is in some ways more ambitious than Pierceson's because the former seeks to place the legal story of gay rights in a broader personal and psychological context, the latter is nonetheless just as successful. Pierceson's main objective in *Courts, Liberalism, and Rights* is to counter the view, which has become somewhat fashionable in both political science and legal academic circles, that courts in the United States have played a relatively minor role in the advancement of social reform. In the past, those who have sought to minimize the role of courts in effecting social change have rarely addressed gay rights issues; going forward, however, proponents of that view will have to address the persuasive evidence set forth by Pierceson regarding how gay rights litigation has been instrumental in moving the American society toward a greater tolerance and acceptance of those with a same-sex sexual orientation.

Pierceson points to litigation involving sodomy laws (in both federal and state courts) and same-sex marriage

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to explain how the cases not only have provided victories to many lesbian and gay litigants but have also had positive spillover effects in the political arena. The principal effect of the cases has been to help frame the political debates over gay rights around issues of basic equality and fairness. In the particular context of the legal recognition of same-sex relationships, Pierceson notes that the same-sex marriage litigation in Hawaii and Vermont, which eventually led to a reciprocal benefits law in the former and civil unions in the latter, "resulted in a discussion of lesbian and gay civil rights that demanded a consideration of equality. . . . Without the legal arguments and decisions to frame this debate, it can be argued that only arguments based on tradition and the desire to preserve the institution of marriage would have had any salience" (p. 192).

As Pierceson notes, a majority of Americans today support civil unions for lesbians and gay men. What was to many, only a few years ago, a radical idea—that lesbians and gay men should have the same rights and benefits afforded to married couples under state law, albeit under the auspices of an alternative legal regime—is now supported by a majority of citizens and many elected officials. As this author persuasively shows, it is exceedingly unlikely that civil unions would have the support of so many Americans today *but for* the ability of the same-sex marriage litigation to place the issue of the lack of legal recognition of lesbian and gay relationships squarely before the American public. The political debate in many (though clearly not all) states is no longer *whether* same-sex relationships deserve legal recognition; instead, the debate is about *what type of* legal recognition they deserve.

For most of the book, Pierceson seeks to establish the link between, on the one hand, litigation strategies and arguments in gay rights cases and, on the other hand, political gains for lesbians and gay men in the United States. (There is also a brief chapter on Canada that serves as a useful introduction but is not meant to be comprehensive; a reader who wants to delve into the details of the political impact of gay rights judicial opinions in Canada will have to look elsewhere.) Pierceson, however, also dedicates two chapters to more theoretical questions that seek, in part, to defend a conception of liberalism that concerns itself not only with protecting individuals from the state but also with the rights to positive freedoms that impose affirmative obligations on the state to recognize and support, for example, the relationships and families of lesbians and gay men.

The two books could be criticized for focusing so intently on both liberalism and individual rights as enforced by the courts. The emphasis is justified, however, because as Richards and Pierceson compellingly show, the combination of liberal values and judicially enforced rights has produced remarkable gains in the United States for lesbians and gay men in a relatively short period of time. It is perhaps for this reason that at the end of the day, the vast majority of those who support the personal and political aspirations of lesbians and gay men, as well as of bisexuals and transgendered individuals, return to some form of liberalism in fashioning their arguments. In fact, even postmodernist queer theorists rely to a certain extent on the libertarian view that the state should not interfere with the consensual acts of individuals so long as others are not harmed. As Pierceson aptly puts it, when it comes to gay issues, "[t]he liberal paradigm appears hard to escape" (p. 42).

Like a Loaded Weapon: The Rehnquist Court, Indian Rights, and the Legal History of Racism in America.
By Robert A. Williams, Jr. Minneapolis: University of Minnesota Press, 2005. 312p. $57.00 cloth, $18.95 paper.

— David C. Williams, *Indiana University*

For two decades, Robert Williams has been developing a powerful critique of federal Indian law, calling for his readers to recognize its irredeemably racist roots. In Williams's view, if we try to build Native American policy from the racist materials of the past, we will inevitably build racism into Indian law of the future. Ultimately, we must therefore recognize that racism lies at the foundation of the American legal system. To many, this message is a bitter pill. But in his view, only after we swallow it will we be able to construct a better foundation for federal Indian policy. And precisely because many hope to avoid this medicine, his call is enormously important. Much as we might like to, he will not let us look away.

In his latest book, Williams continues this prophetic exhortation. Through careful reading of landmark Native American law cases, stretching from the nineteenth century to the recent past, he argues that the Supreme Court routinely invokes racist images of Indians, and it often bases its holdings on these racist premises. Early on, this racism was overt, and it remains so in some more recent opinions. But these days, the racism is usually covert: The judges merely cite racist precedent, purged of racist language, to reach a conclusion that subordinates Indians.

In particular, Williams argues that Indian law rests on the doctrine of discovery: the idea that because Europeans were "superior," they could rightfully claim sovereignty over native tribes and lands. He believes that John Marshall put this doctrine at the center of Indian law in his foundational trilogy of Indian law cases. When later justices rely upon the Marshall cases—as they always do—they perpetuate the sins of the past, though they may clean up the language.

Many experts believe that the Supreme Court is currently so unsympathetic to Indians that tribes should refrain from bringing cases before it, because each case merely gives the justices opportunity to do harm. In an ironic twist, Williams argues that this attitude stereotypes the

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justices as unable to change. He argues instead for a confrontational approach. He relies on cognitive science to argue that racism is often a matter of unconscious generalization, which we absorb when growing up in a racist society to help organize our cognitive world, but of which we are often unaware. It is, in short, a bad habit. When we become aware of this habit, we can overcome it by an act of will. The author believes that the justices are capable of this transformation, with the assistance of confrontational lawyers. Once they become self-aware, they can be persuaded to jettison the racist roots of Indian law. They should, however, retain one element: Originally, Indian law, even the doctrine of discovery, was part of international law. Today, much international law demands protection for the rights of indigenous peoples, and he proposes a new Indian law built on this foundation.

Because of Williams's radicalism, his optimism about the justices is striking, but it may not be universally shared. Supreme Court justices have life tenure and are unreviewable. Though criticized in print, in person they generally receive tremendous deference. Even if lawyers confront them with their own racism, they need pay no attention unless they want to. And it appears that the justices do not wish to be so confronted. As Williams explains, the justices seem far more ready to use racist images of Indians than of other racial groups. In recent years, they have apparently worked harder to correct their habitual racism with respect to others than with respect to Indians.

This discrepancy calls for explanation. It seems that the justices' reliance on racist images of Indians is not merely a bad cognitive habit, a way to organize the world into intelligible packages. Instead, the images serve some psychic need, and so the judges will not easily give them up. Judges can always avert their eyes from their own racism if they want to, and in this case they apparently want to.

In my view, judges can more readily surrender racist views of other groups because those other groups do not threaten the judges' sense of the legitimacy of the American legal system to any extent. African Americans generally want to be included in the American system, to be treated the same as others, to participate in the same hopes and dreams. Even affirmative action is a device of integration, to bring about a future when race does not count. Other than Indians, racial minorities ask the legal system to reform itself, to live up to its own current ideals.

By contrast, Native Americans typically want to govern themselves in their own separate polities, which predate the United States and raise questions about the legitimacy of U.S. sovereignty. In this scheme, race really does count, and in a way that might induce trauma in one who stands at the pinnacle of the judicial system. Sovereignty somehow passed from tribal polities to the United States; even in Williams's international law approach, the United States still acquired some sovereignty over this continent. And the Supreme Court's own legitimacy derives from this process of acquisition. Moreover, the continuing existence of tribal polities means that this transfer of sovereignty cannot be regarded as a closed book, with no present-day implications. If the transfer was unjust, perhaps the tribes are still the rightful sovereigns here. Perhaps the American legal system has roots so poisonous that the tree should die.

Supreme Court justices will avert their eyes from this possibility. Psychologically, judges need to believe that they are people not only of power but also of reason. For them, it is not enough to assert that the United States has gained power over this continent; they need also to believe that the acquisition of sovereignty was just. The doctrine of discovery has served them well in this regard. In the beginning, judges could assert that the United States took sovereignty for the (in their view) good reason that Europeans were civilized. Today, judges do not make such bald assertions; instead, they merely cite precedent rooted in the doctrine of discovery. As good judges should, they are merely following the law, not making it up. They thus are able to assert the legitimacy of the U.S. legal system without ever acknowledging its racist roots—as Williams himself recognizes. But unlike the author (no relation), I do not think they will give up this delusion merely because they are confronted with its racist underpinnings. They need it too much. It allows them to sleep at night. It allows them to do their job.

I do not mean to suggest that the justices should be allowed to live uncontested under cover of this self-deception. But as a practical matter, if they can wean themselves of racism toward Native Americans only by conceding the illegitimacy of the U.S. legal system, they will never do so. What is needed, therefore, is a strategy that will allow them a sense of their own legitimacy but that will also encourage them to recognize their own harmful stereotyping of Indians. Such a strategy may be less radical than Williams's; it may require the justices to change and grow less, and it may in a sense end up compromising with racism. These are severe moral dangers, and at the end of the day, Williams may be right to urge us to avoid them. In any event, we are very lucky that he will never let us forget the dangers.

The Powers of War and Peace: The Constitution and Foreign Affairs After 9/11. By John Yoo. Chicago: University of Chicago Press, 2005. 378p. $29.00 cloth, $19.00 paper.

— William E. Scheuerman, *Indiana University*

Most readers of this journal will recognize the name of John Yoo, legal architect of the Bush administration's most controversial policies in the war on terror. In the aftermath of the 9/11 attacks, Yoo played a decisive role in the Office of Legal Counsel to the U.S. Department of Justice, where he regularly defended the most extreme legal options under consideration by the administration. In

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August 2002, he penned a memo arguing that both international and domestic legal prohibitions on torture represent illegitimate restraints on presidential power. His legal views rapidly gained the upper hand because they told the president what he wanted to hear: As commander in chief, the president can do pretty much whatever he wants in order to protect the American people.

Yoo's volume outlines the constitutional theory behind this expansive conception of executive power. For those trying to make sense of the jurisprudential views grounding the Patriot Act, Guantanamo Bay, or the domestic spying scandal, this is the place to look. President George W. Bush typically justifies his legal positions in the war on terror by noting that the Constitution makes him commander in chief and thus chiefly responsible for defending the American people. Yoo's book fleshes out this simple idea. In his view, the Framers always intended for the executive to dominate foreign and military affairs. Of course, the Constitution appears to place the authority to "declare war" unambiguously in the hands of Congress. However, the author interprets this phrase to entail nothing more than a diplomatic courtesy: It offers no check on the president's authority to initiate and conduct most wars, merely requiring an official announcement by Congress of the existence of hostilities after their commencement. Although the conventional reading of the treaty-making power describes it as shared between the Senate and president, Yoo offers a similarly creative reinterpretation, according to which the executive possesses vast power to interpret and terminate treaties at will.

If Yoo is right, not only is the mainstream view of the president and Congress as uneasy but equally indispensable partners in foreign and military affairs misguided, but many political leaders in U.S. history have been badly mistaken about our system as well. As he concedes at various junctures, it is really only during the last century that the president has become a demiurge-like force in foreign affairs, regularly launching military hostilities without bothering to ask Congress for permission. In other words, it is only the imperial presidency that does justice to the Framers' original understanding, and perhaps only Yoo's former boss, George W. Bush, who has most fully achieved it. In this account, Congress is at best a junior partner, maintaining negative rights (e.g., the power of the purse, since Congress can always refuse to fund presidential wars) but destined to play second fiddle to a powerful executive modeled directly on the example of the eighteenth-century British monarch. Since the author grasps that globalization inevitably tends to blur any meaningful divide between foreign and domestic affairs, he occasionally concedes that his defense of executive power has far-reaching implications for domestic affairs as well.

Not surprisingly, Yoo's tendentious views have already ignited significant controversy among legal scholars. In leading academic journals and political periodicals, they have argued that Yoo misrepresents the Framers' views about the war powers. Congress's authority to declare war was envisioned as a strict constitutional rule according to which the executive simply could not launch war without formal congressional authorization. Even in the case of foreign aggression against the United States, the executive might need to act immediately in order to repel concrete physical attacks, yet Congress would then be expected to convene as soon as possible in order to provide proper constitutional guidance to the executive. As his critics have observed, Yoo's readings of the Framers range from eclectic to idiosyncratic. He tends to discount or even ignore familiar materials that work against his argument (see my following comments on Alexander Hamilton), while offering highly peculiar readings of those sources he discusses. In light of the fact that his own presentation places so much emphasis on uncovering the proper historical origins of the U.S. system of war powers, for example, it is odd that he devotes no real attention to the obvious historical question of why a proper understanding of the original constitutional system apparently was lost until the emergence of the imperial presidency. Why did so many of our sage political ancestors—including the Framers' immediate political progeny, who arguably dominated political life in the early republic—miss what only John Yoo has been able to discover?

The Framers also seem chiefly to have had in mind *life-or-death physical violations of the body politic* (e.g., violent attacks by foreign powers or civil disorder) when debating the president's authority as commander in chief, whereas Yoo tends to operate with a broader conception of the emergency situation, in which many undeniable *threats* (e.g., terrorism) to the political order tend to get conflated with traditional forms of warfare or violent rebellion. This may seem like an academic distinction, but its implications are huge. If every significant threat to the political status quo (an outbreak of avian flu virus, for example, or another terrorist attack on U.S. territory) necessarily requires massive augmentations of poorly regulated executive power, Americans indeed will probably have to bid farewell to the rule of law.

Let me point to another troubling consequence of his general position hitherto ignored by most legal critics. If Yoo is right, an American *Revolution* never really took place. To be sure, ragtag American patriots waged heroic battles against British redcoats and their Hessian mercenaries, but the final result was nothing more than the establishment of a political system based in crucial respects on the British monarchy.

According to most scholars, the Framers abandoned one of the central prerogatives of traditional kinship, namely, the monarchist view that the king was, as William Blackstone famously described it in his *Commentaries on the Laws of England,* "the generalissimo, or the first in military command" ([1765] 1979, Vol. 1, p. 254). Part of

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traditional monarchist ideology was always the idea that kings played a predominant role in foreign and military affairs because they possessed divine or charismatic powers that made them best suited to military leadership. Monarchist legitimacy was intimately tied to the belief that kings represented, or at least descended from, the leading warriors of the realm. The first Norman king of England, of course, was the aptly named William the Conqueror. According to many scholars, the Framers abandoned this tradition, in part because their reading of European history suggested to them that royal preeminence in foreign affairs opened the door to political and military adventurism. As noted even by the greatest defender of a strong presidency among the Framers, Alexander Hamilton, an elected executive "would be ambitious, with the means of making creatures; and as the object of his ambition would be to prolong his power, it is probably that in case of war, he would avail himself of the emergence, to evade or refuse a degradation from his place" ("Speech in the Constitutional Convention on a Plan of Government" [1787], in *Writings*, 2001, p. 158). How better to augment executive power than by taking advantage of a foreign or military crisis? Because of this peril, the Framers circumscribed the president's war-making powers, and even Hamilton arguably would have worried about the modern tendency for presidents to undertake military action without express congressional approval.

Yoo insists that we need worry no more. A central theme of *The Powers of War and Peace* is that the Framers designed our constitutional system strictly in accordance with British models of war making, in which Parliament possessed little more than a series of negative budgetary powers against the royal "generalissimo." The Framers built directly on the "English system [which] gave the executive leadership in the initiation and conduct of war and the making of treaties, while the legislature primarily played a role by funding the wars, enacting implementing legislation, and impeaching ministers" (p. 32). He concedes that Anti-Federalists like "Cato correctly concluded that in the realm of practical politics, the president's authority under the Constitution did not differ in important measure from that of the king" (p. 113). But the Anti-Federalists were wrong to worry so much. To be sure, Yoo notes, the U.S. system jettisoned Britain's system of *hereditary* monarchy. Yet his analysis of the presidency repeatedly underscores its continuities with monarchical power. By insisting that the presidency ultimately preserved a *core* royal prerogative, he begs an obvious question: Might not an apt characterization of the constitutional system he defends be *elective monarchy,* in which we periodically choose some individual outfitted with vast and effectively unlimited discretionary power in order to protect us?

Yoo never openly concedes that this is an immediate implication of his argument. Nonetheless, if he is right, somebody ought to tell the Daughters of the American Revolution, or at least the little kids waving flags on the Fourth of July. They might be surprised. We dumped one King George two centuries ago, but we now have a new one. The republic is dead; long live the king!

COMPARATIVE POLITICS

Transforming Latin America: The International and Domestic Origins of Change. By Craig Arceneaux and David Pion-Berlin. Pittsburgh: University of Pittsburgh Press, 2005. 280p. $27.95.

— Eduardo Alemán, *University of Houston*

In this book, Craig Arceneaux and David Pion-Berlin examine the impact of international influences on policy change in Latin American countries. As the authors correctly point out, "the scholarship on political change in Latin America provides little direction for those seeking answers to the relative impact of domestic and international forces" (p. 29). This ambitious project seeks to explain the conditions under which international actors influence governmental decisions, as well as the degree to which government policies are or are not shaped by foreign forces. To this end, they advance an "issue-guided analysis" that attempts to identify the important actors, capabilities, and modes of influence associated with policy change. The book offers a stimulating overview of foreign influences in domestic policy decisions, yet its contribution to the literature on policy reform in Latin America is limited.

The authors present their theoretical perspective in the first two chapters and their empirical evidence in five separate case studies. They begin describing an ongoing tension between a government's need for decision-making autonomy and the desire of foreign actors to erode some of that autonomy (p. 9). The issue-oriented approach attempts to highlight how different policy matters represent distinct constellations of opportunities and barriers for prospective participants. Issues belonging to the realm of *high politics*—where states have vital interests at stake—are differentiated from issues belonging to the realm of *low politics*—where states have little or no incentive to interfere in the domestic affairs of others. Powerful states, which have overwhelming financial and coercive resources at their disposal, have a greater incentive to intervene in high-politics issues (economics and security) than in low-politics issues (human rights and the environment). Foreign powers are portrayed as being able to successfully

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exert pressure on most issues, but strategic considerations lead them to concentrate their efforts on high-politics issues. Although "domestic moments" may occur in high politics (when foreign powers lose their motivation to intervene), policy change in those issues is primarily driven by foreign actors (p. 31).

How can we evaluate foreign influence in policy change? The book does not present systematic quantitative analysis or a rigorous comparative investigation of the effects of specific variables, and as a result it is difficult to evaluate the authors' propositions. The case studies that comprise the empirical chapters illustrate the participation of foreign and domestic actors in a variety of policy areas; however, they present only modest support for the propositions advanced in the theoretical chapters.

The principal high-politics issue addressed in the book is "neoliberal reform," which is analyzed in Chapter 3. The authors claim that market reforms associated with the Washington Consensus belong to the realm of issues where the capacity of international actors to influence domestic politics is massive and where "there are no moments in which the balance of influence unexpectedly tips to the domestic" (p. 42). As a result, the forces of political change can largely be found in the self-interests of powerful outside forces (mainly the United States, multinational corporations, and international financial institutions). Since foreign actors are capable of determining the outcome of these reforms, the decision to provide (costly) international support is determined by their motivations. Political utility explains international support for Chile, and economic importance does the same for Brazil. Argentina, in contrast, lacks both qualities, resulting in the indifference of the U.S. government and the rise of the International Monetary Fund as the lead international actor (p. 77).

Arceneaux and Pion-Berlin present one plausible rationale for the different strategies of foreign actors (market size and political utility), but the chapter is unconvincing on matters central to the authors' theoretical arguments. The origins of these reforms stem from domestic sources, which is something that even the authors concede. And we cannot evaluate the role of foreign actors because the narrative fails to systematically compare the relative influence of the foreign and the domestic in the actual development of these reforms. It is unfortunate that the authors do not engage the vast theoretical and empirical literature on economic reform in Latin American countries that examines the power of those domestic actors that actually struggle to pass and implement these difficult reforms. Institutional characteristics and domestic actors (e.g., political parties, presidents, labor unions, industrialists, and consumers with voting power) play no crucial role in the authors' explanation of market-oriented reforms.

The fourth chapter focuses on democratic defense and democratic deepening, with particular attention to the role of the Organization of American States (OAS). Democratic defense is considered a low-politics issue that has recently moved to the realm of high politics. Democratic deepening, in contrast, continues to be a low-politics issue where foreign actors pay scant attention and domestic actors rule. The authors correctly point out that there has been a strengthening of electoral monitoring missions, but they also make the less substantiated claim that "the OAS had acquired the political will, legal machinery, and institutional wherewithal to rescue democracies under siege and to deter future conspiracies against them" (p. 93). The rest of the chapter examines the role of the OAS in the Peruvian elections of 2000 and the Ecuadorian coup of the same year.

The next two chapters address human rights issues in Chile and regional security arrangements in Central America. The intention is to illustrate deviations from the rule: when low-politics issues (human rights) have "international moments" and high-politics issues (security) have "domestic moments." The chapter on how Central American nations proceeded to build regional arrangements in the 1990s provides a particularly valuable account of regional cooperation and domestic initiative on an issue generally dominated by U.S. concerns. The last chapter before the conclusion combines short discussions of the environment, immigration, and drug trafficking in order to illustrate how to classify issues that cannot be straightforwardly labeled within the high–low politics framework.

In sum, the book makes a good case for incorporating foreign actors into analyses of political change in Latin America. It illustrates how these foreign actors sometimes become important players that could potentially affect the strategies of national governments. In the end, however, it does not deliver a successful model for systematically assessing the degree of foreign influence in policy change.

Institutions and the Fate of Democracy: Germany and Poland in the Twentieth Century. By Michael Bernhard. Pittsburgh: University of Pittsburgh Press, 2005. 310p. $29.95.

— Karol Soltan, *University of Maryland at College Park*

In this book Michael Bernhard asks two questions: Why do new democracies pick the institutions that they do? How do certain patterns of institutions affect the chances of democratic survival? And his theoretical framework, in its initial formulation, makes two claims. First, institutional choices are a product of powerful agents pursuing their narrow interest. If you know who is powerful and what they want, you will be able to predict the institutional outcomes. Second, the key to democratic survival, at least in the initial posttransition phase, is the breadth and strength of the original institution-forming coalition. A key conclusion of the study is that democracies are in trouble when the coalitions that brought about their

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institutional arrangements lose power too soon, as happened in prewar Poland and in Weimar Germany. And this can occur for a variety of reasons: New actors may be brought in, for example, or the salient issues may change.

After briefly presenting this framework, Bernhard elaborates, testing its usefulness on four cases from two countries: Weimar Germany and interwar Poland provide examples of democratic failure; the Bundesrepublik and post-1989 Poland provide examples of democratic success. The core of *Institutions and the Fate of Democracy* consists of detailed analyses of these four cases, and the book is worth reading just for those examples. But there is the further issue of the value of combining the two questions he asks, of the usefulness of his theoretical framework, and, finally, of whether considering these four cases together adds anything we could not get by studying each case separately. I am not completely convinced on any of these counts. The framework seems to be missing some important ingredients. And actual cross-country comparisons play a surprisingly small role in the author's argument.

Nevertheless, the book shows how large n-studies can be complemented by more detailed historical accounts, allowing us to see better the actual politics that brings democracy into being, or destroys it. One can only hope there will be more and better such studies in the future. With Bernhard's approach, we can learn things about democratization even sometimes despite his framework, with its narrow empirical focus on the executive and legislative institutions and on electoral rules. A number of elements struck me as important in these four stories, despite not being featured in the theoretical framework.

First, peace treaties and the development of "boundary regimes" play an important role in these stories of democratization. Constitutions are also in part peace treaties, as I recently had many occasions to repeat in discussions of the Iraqi constitution. And peace treaties are also in part constitutions, we might add, thinking of Versailles and Riga as constitutive of the short-lived democratic systems of Germany and Poland after World War I. Constitutions are not to be identified with legal documents that bear that title.

Both external and internal boundaries in federal systems form a large part of the nature of the regime. In Germany after both wars, much time and effort was expended on deciding the nature of the federal regime. Bernhard tells the story, yet it is not reflected in his framework. And he neglects completely the debates for and against federalism that were so important in Polish politics during and immediately after World War I, because they were settled more by peace treaties and boundary decisions than by constitutional choice narrowly understood (that legal document again). Yet the way these issues were settled made a federal consociational regime extremely unlikely in Poland in the 1920s, and without that, democracy was going to have a hard time.

The peace treaties after World War I and the Soviet-Polish war left Poles with 69% of the population in Poland, with various minorities dividing up the remainder. This demographic split is perhaps the most difficult for democracy to work (think of Cyprus or Iraq). In such countries, minorities are big enough to require special protections, but the majority is sufficiently large that granting the minorities veto power is unlikely to be accepted as democratically legitimate.

A related lesson about democratization that emerges from Bernhard's account, without being reflected in his conclusions or his framework, is the importance of consociational and consensual institutions (Lijphart is almost completely ignored in this book). The presence and absence of consociational or consensual features in a system, such as federalism or constitutional courts, emerge as significant elements of the story. But they are not fully told, because they are not recognized in Bernhard's framework. As I read the German story, contrasting the Weimar Republic with the Bundesrepublik, it seems that one key cause of the change in democratic prospects is the shift in location of the "constitutional guarantor." In Weimar, the monarch's role as guarantor is replaced by the president, with well-known disastrous consequences in the 1930s. After World War II, the constitutional court replaces the president, significantly improving (or so it seems) the capacity of German democracy to survive emergencies.

Bernhard's own conclusions focus on how well his framework has performed. It is not enough, he writes, to know the self-interested preferences of the most important actors to know the political outcome. Some political actors act on the basis of values, he continues. And we may add that the evolution of ideas about democracy is an important feature of the contrast between the two pairs of cases (before and after World War II). What constitutes narrow interest is not a simple question, he adds; preferences may change during the process of institutional choice, and the relevant actors can also change. So self-interest matters, but a flexible notion of interest is required, and context matters as well. We should be careful in building parsimonious models of politics. All true enough.

Bernhard's conclusions seem to me more striking on the question of democratic survival. The four cases strongly confirm, he writes, the importance of the political fortunes of the initial institution-framing coalition in the posttransition phase. This is a striking conclusion, because it suggests that the nature of the institution-forming coalition may be more important than the institutions they adopt. He does not discuss in any depth why this might be so. Let me suggest a possibility. Legitimacy matters (this would be a nice addition to Bernhard's framework), and to be seen as legitimate, the institutional arrangements of a democracy must be perceived as impartial or neutral. Over time, this perception develops as different groups win at different times. But at first the

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perception of neutrality is more difficult to establish. Perhaps the first winners are so because the system is biased in their favor. Hence, the initial importance of maintaining in power a broad institution-making coalition. For them, the rules of the game are neutral because they all agreed to them.

Political Choice in Britain. By Harold D. Clarke, David Sanders, Marianne C. Stewart, and Paul F. Whiteley. New York: Oxford University Press, 2004. 392p. $95.00 cloth, $39.95 paper.

— Robert M. Worcester, *MORI/LSE/Warwick/Kent*

This book is a useful contribution to the psephology of British elections, using the 2001 British general election as its test bed. The authors examine the history of "sociological" and "economic" models of voting behavior, test and find them wanting, explaining that movement to issue, party, and leader perception (Worcester 1991) coupled with economic performance assessment (Sanders et al. 2001) is found to be more effective.

The core claim of the book is that although the economic and social context of electoral choice in Britain has changed considerably since the 1960s, the basic calculation of voters' decisions has not, and that *valence,* the ability of governments to perform in those policy areas that people care about most, that is, salience, crystallized by their perception of party leaders, is the best indicator of voter behavior.

The authors' analyses compare models of electoral choice using the sociological and individual rationality frameworks. They found the latter the more powerful, observing the long-term decline of social class as a key determinant of voting behavior over four decades of studies, accelerated by the emergence of "new" Labour with the election of Tony Blair as leader of the Labour Party in 1994 and his stunning victory, "Labour's Landslide," in 1997.

Clarke and his colleagues are a cross section of U.S. and British scholars with both political science and economics as their disciplines. Their tools are primarily the 2001 British elections survey carried out by National Opinion Polls, supplemented by a Gallup rolling cross-section telephone campaign survey and follow-up postelection survey.

The project was mainly funded by the British government's Economic and Social Research Council, supplemented by a grant from the National Science Foundation, which provided funding for 1992–99 monthly Gallup surveys that tracked the evolution of party support during the years between the 1992 and 1997 elections and on until the cessation of the monthly Gallup polls in Britain in 1999. In addition, some funding was provided by the authors' universities.

Political Choices in Britain builds on the work of Butler and Stokes (1969) and many other psephological studies since, and even from before, but fails to make use of other freely available relevant data from ICM's and MORI's regular monthly surveys during the periods of their study. Both firms carried on regular monthly tracking after the closure of British Gallup's polling in 1999. Nor did the authors make use of both firms' extensive (and freely available) work during and following the 2001 election. There was also a switch of methodology by Gallup during the tracking period following several years when the monthly Gallup polls, which were being conducted face to face, were clearly out of line with other published polls; when a switch was made from personal interviewing to telephone, the Gallup monthly voting intention figures pulled right into line with other available data. This is not mentioned.

The authors also observe an increase in tactical voting of electors to obtain the outcome they wish to see, keeping someone out rather than voting to keep that person in. In the 2001 election, British turnout fell by around 12 points, from 71.5% in 1997 to 59.3%, which they mainly attribute to large preelection leads in the opinion polls, but do not explain why the large 16% lead enjoyed by Margaret Thatcher in 1983 was accompanied by a higher turnout (72.7%) than in the quite close elections of 1970 (72.0%) and October 1974 (72.8%).

The key sentences summing up their findings are as follows: "Electoral politics is largely valence politics, and public debate and inter-party competition focus on which party and which leader are best able to deliver. Voters in Britain (and elsewhere?) rely on heuristic devices for cues, and party leader images are among the most important of these devices" (p. 326). Elliott (1998) has commented that political scientists always seem to assume rationality in voter decision making, not irrationality. Marketers like J. Sheth and B. Newman have suggested otherwise, and there is not, finally, a clear movement in marketing models taking in more emotional-driven choice.

The book's usefulness is principally in the work that the authors have carried out into motivations of voter participation/turnout. For many of the postwar years, the levels of turnout varied within a fairly narrow band around 76%. The 71.5% in 1997 was at the bottom end of the chart, but a drop of 12 points in 2001 was unprecedented, and so much research has been done on the causes of this. Their work will feed into work in this area, especially as the 2005 turnout in the recent (May 5) election was up only three points, from 59% to 62%, and actually fell by two points, from 39% to 37%, among the 18–24-year-old cohort, where most effort to increase turnout had been targeted.

Reforming European Welfare States: Germany and the United Kingdom Compared. By Jochen Clasen. New York: Oxford University Press, 2005. 264p. $90.00.

— Jennifer Fitzgerald, *University of Colorado at Boulder*

This book traces welfare policy developments in Germany and the United Kingdom from the late 1970s to 2003.

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Focusing on three policy dimensions—unemployment policy, public pension policy, and family policy—Jochen Clasen seeks to understand the scope, nature, and timing of welfare reform. To his credit, the author avoids the usual trap of characterizing welfare reform exclusively in terms of retrenchment. Recognizing the complex nature of shifts in social policy over time, his work details and explains patterns of retrenchment, restructuring, and expansion. To understand the pathways of policy transformation, he considers three types of explanatory factors: party politics (mainly actors' preferences), contextual conditions (such as major events and social trends), and institutional parameters (including legal constraints, existing policy programs, and the characteristics of political economy structures). He also considers the relationships among these causal forces.

Clasen's approach can be described as comparative historical analysis, through which he traces the intricate developmental processes of these three different policy families in Germany and the UK. To establish the direction and scope of each reform trend, the author considers qualitative indicators, such as changes in eligibility regulations, in conjunction with statistics on public spending by policy domain. By unpacking social provision indicators, he demonstrates a sensitivity to claims by Francis Castles (see *The Future of the Welfare State: Crisis Myths and Crisis Realities,* 2004) and others, who rightly contend that aggregate data on national welfare expenditure can be misleading.

Clasen's process tracing yields a series of complex narratives. In spite of similar trends of unemployment policy retrenchment in Germany and the UK, the author finds that each country's distinctive policy profile is rooted in preexisting program structures that are linked to characteristics of the national political economy. He details similar trends of retrenchment in German and British public pension provision, but also finds differences between the two national policy profiles. Here, he argues that national trajectories are mainly conditioned by "national programme-specific institutional parameters, changes therein, and their impact on the dynamics of party policy preferences" (p. 131). With respect to family policy expansion in Germany and the UK, Clasen highlights the importance of sociocontextual factors and party platforms, which appear to be more relevant than institutional factors for family policy trajectories.

The book's content is presented in an accessible manner that capitalizes on the comparative aspect of the study. Chapter 1 sets up the juxtaposition of the German and British cases, which are typically conceptualized as alternative models of the modern welfare state. Chapter 2 situates Clasen's contribution within the academic debates over the direction and causes of welfare reform in industrialized democratic states. Chapter 3 presents the three main explanatory perspectives: preferences, context, and institutions. This chapter also provides a 25-year overview of socioeconomic trends in Germany and the UK.

Chapters 4 (on unemployment support), 5 (on retirement pensions), and 6 (on family policy) offer comparative analyses of developments in the three policy arenas. In these chapters, Clasen describes and empirically substantiates the trajectory in each country for the policy domain at hand, highlighting cross-national commonalities and divergences in policy outputs. He then explains the progression in Germany, followed by a parallel account of the changes in the UK. In summary, each of these analytical chapters offers comparative conclusions that entail consideration of the role played by actors' preferences, broad situational factors, and institutional settings. Finally, Chapter 7 reunites theory and empirics for a review of the findings, and considers the effects of Europeanization on welfare state reform.

The key strength and weakness of Clasen's book are both rooted in his overall approach. The topic under study is extraordinarily complex, and by means of fine-grained analysis, the author manages to capture the nuance of social policymaking in three different policy domains, in two different countries, over 25 years. Through historically sensitive case studies, he is able to provide a rare and detailed view of the mechanistic processes that shape outcomes. When considering the influence of preferences, contexts, and institutions, however, he notes that "their respective relevance has varied across programmes" (p. 188). What he is offering, then, is evidence of a set of processes that seem to fit Arthur Stinchcombe's definition of a mechanism as a "sometimes-true theory" (see "Monopolistic Competition as a Mechanism: Corporations, Universities and Nation-States in Competitive Fields," in Peter Hedström and Richard Swedberg, eds., *Social Mechanisms: An Analytical Approach to Social Theory,* 1998). For instance, the author finds that the concept of path dependency is supported mainly in the case of public pensions policy, but less so when examining unemployment provisions. Further, this research suggests that path dependence does not play a major role in the advancement of family policy in Germany and the UK.

Comparative scholars who put little stock in broadly generalizable conclusions about welfare state reform will certainly applaud the author's efforts to present a contextually defined narrative. Others will find the fragmentation of his explanatory account somewhat unsatisfying. On a related note, Clasen's advancement of multicausal explanations may closely reflect the reality of policy development, but his resistance to prioritizing one set of explanations over another leaves the reader without the usual take-home message. If we consider this work in terms of a broader research agenda, however, the book offers many hypotheses that can be tested in other national and policy contexts.

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Another trade-off involves conceptual clarity. The author is methodical when it comes to clearly defining his dependent variables, contributing to a focused and valuable study. In contrast, such key explanatory concepts as political economy and context are less theoretically delineated. The inclusion of contextual factors is important, particularly when considering the strains of German unification or the effects of female labor participation in both countries. However, the diversity of factors that qualify as contextual (ranging from the Falkland Islands war to political scandals, normative orientations, and public opinion) suggests a residual category. This interest in context should figure into future work on social policy, but its conceptualization should be more rigorous and theoretically grounded.

Clasen's book will be very useful for academics and policy experts concerned with the current condition of the modern welfare state. Certainly, the author's rich knowledge of his cases offers a tremendous resource for any researcher interested in employment, pensions, or family policy, or in welfare state politics more broadly. Finally, his comparison of policy shifts in these two important national arenas is not simply an investigation of outcomes; it provides a research model that is sure to shape future studies of the welfare state in advanced democracies.

Engaging the Law in China: State, Society, and Possibilities for Justice. Edited by Neil J. Diamant, Stanley B. Lubman, and Kevin J. O'Brien. Stanford: Stanford University Press, 2005. 256p. $49.50.

— Jeanne L. Wilson, *Wheaton College, Norton, MA*

This book, an outgrowth of a 2002 conference held at the University of California at Berkeley, takes as a given the importance of law as an operational concept in China. Rather, the fundamental questions for investigation are 1) how, when, and to whom law matters, and 2) how to study the relationship between law and society (p. 3). Traditionally, the study of Chinese law has been the preserve of legal specialists, predominantly lawyers, who have focused their attention on the institutional development of the Chinese legal system. This book takes an alternative approach in that it seeks to broaden the scope of investigation of Chinese legal issues. The contributors are primarily young scholars in the social sciences—notably political science and sociology—who report on their recent field research in China.

A major aim of the volume is to bring an interdisciplinary approach to the study of Chinese law, making use of insights and approaches derived from social science methodology, and broadening the range of inquiry to include the interaction between formal legal structures and Chinese society. To these ends, the book makes use of three concepts as overarching themes for inquiry. First, the volume addresses the means by which law is mobilized in China, and the extent to which Chinese citizens make use of legal structures to express interests and air grievances. Secondly, the book examines the extent to which a legal culture has evolved in China, especially with reference to the question of citizens' consciousness of their legally endowed rights. Thirdly, the book assesses the status of Chinese legal institutions, raising the question of the extent to which they function independently of political controls. It rejects the view, common to the traditional legal approach, of the state as an unitary actor for a perspective that disaggregates the state into a multitude of actors, operating at different levels, often with conflicting goals.

The book's contributors tackle a wide-ranging array of issues. Neil J. Diamant, Stanley B. Lubman, and Kevin J. O'Brien (Chapter 1) set out the theoretical approach and main themes of the volume in their introduction. O'Brien and Lianjiang Li (Chapter 2) examine administrative litigation in rural China; Mary E. Gallagher (Chapter 3) investigates the impact of the 1995 Labor Law on labor mobilization; Isabelle Thireau and Hua Linshan (Chapter 4) report on the expression of labor grievances in China through arbitration committees and letters and visits to offices; Mark E. Frazier (Chapter 5) looks at China's pension reform; Diamant (Chapter 6) discusses the struggles of veterans in the 1950s and 1960s; Andrew C. Mertha's (Chapter 7) research is on the role of foreign actors in the evolution of anticounterfeiting enforcement; Murray Scot Tanner (Chapter 8) assesses police analyses of social unrest; and Fu Hualing (Chapter 9) investigates the operation of a labor reeducation institution.

The research is of a uniformly high quality, and each chapter in itself contributes to the development of our knowledge about Chinese society and the operation of the Chinese political system. It cannot be said, however, that each chapter contributes equally to our specific understanding of the intersection between legal structures and Chinese society. It seems that the legal traditionalists, despite their myopic formalistic attention to legal structures independent of their impact on society, enjoy a certain advantage in defining their subject. In contrast, the broader, more ambitious approach of the social sciences means a certain diminution of clarity of focus and the blurring of conceptual boundaries, especially between legal and bureaucratic procedures. The linkages between law and society are most easily discerned in those chapters with an explicit legal focus; this includes O'Brien and Li's research on administrative litigation, and Gallagher's and Thireau and Hua's discussions of the relevance and application of the 1995 Labor Law. Frazier's chapter on pension reform, however, seems to fit equally well within the framework of public policy analysis. Diamant's discussion of veterans also poses a challenge; in the absence of any specific legislation relating to the treatment of veterans, he is compelled to make a sort of counterargument—unsusceptible to theoretical validation—that the passage

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of a "veterans' law" would have made a difference in ameliorating discriminatory treatment toward veterans. Whereas the case study approach used in many chapters yields valuable empirical information, it also raises the inevitable question of the extent to which conclusions can be generalized in a country as diverse and vast as China. One wonders, for example, if veterans in more provincial regions of China were treated with as much contempt as the veterans that Diamant describes in his case study encountered in Shanghai.

Some of the themes that emerge from this volume are addressed in the introduction, but others might have been summarized in a concluding chapter. It is apparent that the endeavor to establish legal structures serves an instrumental function for the Chinese Communist Party, providing a regularized outlet for the expression of discontent. The Chinese legal system has developed in incremental steps, in the face of tremendous challenges. Nonetheless, its steady institutionalization confirms the position of the legal formalists that the codification of law, even if imperfectly and partially implemented, is important. A number of contributors also emphasize the role of legal structures in providing a structural underpinning for a transition to a capitalist economy. Gallagher, in particular, argues that the specific provisions of the 1995 Labor Law and the implementation of a labor contract system have delegitimized the social contract of the Maoist era, and undercut workers' ability to file grievances through legal channels.

In summary, this book is a valuable contribution to research on contemporary China. It can and should be read by individuals with a specific interest in Chinese legal studies, as well as by those with a general interest in state–society relations in the Chinese context.

China's Democratic Future: How It Will Happen and Where It Will Lead. By Bruce Gilley. New York: Columbia University Press, 2004. 320p. $29.50 cloth, $20.00 paper.

— Andrew Mertha, *Washington University St. Louis*

Much of the scholarship on democratization in China falls into roughly two camps. Some argue that democracy in China is or will be a bottom-up process. Others aver that it will be an elite-driven top-down phenomenon. Bruce Gilley is firmly in the second camp. He argues that democratization is likely to occur as China's more moderate and liberal leaders recognize, in the face of "a multiple metastatic dysfunction," the tenuous hold they have on China and move toward democratization as a means of minimizing their losses.

In the first section of the book ("Crisis"), Gilley discusses the challenges faced by China (Chapters 1–3), as well as possible trends toward democratization (Chapter 4). His description of China's ills is set in extremely sharp relief. Chapter 4 is an improvement, and its more optimistic picture of China is more nuanced.

The middle section ("Transition") provides the linchpin for the analysis. Gilley weighs different democratization scenarios, concluding that China's leaders will most likely institute radical systemic change in response to economic and social crises. Here, he makes three useful, albeit largely self-evident, points. First, there seems to be a general consensus among the top leadership that the economic and social status quo is untenable (although this does not mean that they are in favor of democracy; the People's Republic of China [PRC] always took an activist approach to governance). Second, it seems likely that substantive political reform will be more evolutionary than revolutionary and will involve at least some degree of elite cooperation and accommodation. Third, the army is unlikely to allow itself to be pushed into another situation like that in 1989 when it was ordered to shoot indiscriminately at Chinese civilians.

These reasoned claims are overwhelmed by other, more controversial ones, however. The implosion of the political center is not likely to lead to chaos, writes Gilley: "[I]ndeed, the social atmosphere would likely be quite the opposite, recalling the heady feelings of mutual respect and love—a veritable Woodstock—that broke out during Tiananmen" (p. 141). This analogy betrays a misunderstanding of Tiananmen, particularly the very *un*democratic rise of radicals like Chai Ling and Wu'er Kaixi. Nor is Xinjiang or Tibet likely to be satisfied with a "high degree of autonomy." Arguably, they are likely to exploit the turmoil to secure their independence. Gilley also underestimates the problems of disentangling the Chinese Communist Party (CCP) in a post-CCP system: "China should be in a good position on all counts. A caretaker government could quickly deploy the old CCP system for new purposes" (p. 138).

The final section ("Consolidation") lays out the challenges that the new regime will face once the euphoria of regime change wears off. It contains chapters on the political challenges facing consolidation, the nature of post-democratic economic and social life, and China's kindler, gentler foreign policy and international behavior. It is a somewhat futile exercise to evaluate these longer-term predictions and prescriptions. The cavalier and presumptuous tone of this section, however, may be off-putting to some.

Gilley writes with flair and his prose makes for engaging reading. Nevertheless, there are some fundamental problems with his argument. First, in the shift toward democratization, everything hinges on what he describes as "the pact" encompassing "the motivations the parties have for embracing it," one normative ("the . . . belief that democracy is the best thing for the country" [p. 129]) and the other instrumental ("[a]ny attempt to recentralize power or repress protest would lead to fissures and breakaways because it would be illegitimate" [p. 129]). I agree with Gilley that the normative explanation is probably the

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"weaker of the two" (p. 129). However, his "instrumental" motivation is nothing more than a restatement of the normative one, that is, that only democracy is "legitimate." Such an a priori explanation of why China is likely to democratize courts tautology.

Second, Gilley identifies elite-led "retreat" as the principal dynamic of China's democratization. Although there is some merit to the argument, he avoids the far more intractable problem of how to extend this to the local levels. Indeed, he does not even ask the fundamental question of how local elites must necessarily follow the lead set by Beijing, especially if they perceive such democratization as against their immediate interests. Of course, some local leaders may go along, as he implies, but given the complexity of China's increasingly decentralized political infrastructure, there is a high probability that such efforts at elite-led democratization would run into a collective-action problem of a gargantuan scale. This is something the author assumes away by asserting that local governments will welcome a de jure federalism from what he curiously describes as "the unitary state in which virtually all power resides in Beijing" (p. 167).

Third, predicting what will happen in China is a thankless task because of China's oft-demonstrated unpredictability, from the makeup of the next Politburo Standing Committee to how the country might democratize. But Gilley insulates himself from this by asserting that "[o]ne of the risks of writing a book about the future is being wrong. The other risk is being right too soon" (p. 249). In so doing, he makes it practically impossible to falsify his claims.

Finally, there are a number of dubious statements in the book. Some are problems of interpretation. Gilley seems to fundamentally misunderstand the Hundred Flowers movement: "Democrats made a last-gasp effort in 1956 [*sic*] to force the CCP to make good on its promises" (p. 19). On the same page, he labels Deng Xiaoping a "sycophant" of Mao Zedong. Others are factual inaccuracies, such as the "strike hard" campaign beginning in 1995 (it started in 1983). There are also several instances where Gilley seems to contradict himself, or at least confuse the reader: "Today [the army] thinks independently of the Party" (p. 111) but "China's military has never been an independent political force" (p. 112).

Moreover, there is a dearth of the author's own personal observations of China, and as a result, the book has a strange at-arm's-length quality. In some cases, it strains credulity altogether: "When you step across the border into Guangdong from Hong Kong—Cantonese societies both—the joie de vivre of life in a free society is replaced by the torpor of life under dictatorship" (p. 46) is but one example. Such an observation is simply baffling. The same is true with the statement that "[t]he rule of law and the legislative and judicial organs were respected and workable in the late PRC era" (p. 138). And his overreliance on Chinese intellectuals as representative barometers of Chinese society adds to this disconnectedness.

At best, then, *China's Democratic Future* demonstrates a modest breadth of knowledge without the depth necessary to make it a credible scholarly work, even if the intention of the book is to constructively shake up the conventional wisdom by overstating its case. It might be appropriate for some undergraduate or graduate course syllabi as a thought-provoking, book-length think piece, but not as a serious scholarly analysis.

The Business of Lobbying in China. By Scott Kennedy. Cambridge, MA: Harvard University Press, 2005. 278p. $49.95.

— Victor Shih, *University of Chicago*

In this fascinating monograph, Scott Kennedy explores the nature of state–society relations through the lens of business lobbying. He begins his work by criticizing existing approaches in comparative politics, which often try to fit various countries into a few standard models of state–society relations: pluralism, corporatism, clientelism, and monism. Given the complexity and dynamism in China's economy, Kennedy makes the astute observation that no single model suffices to describe the complex business–government relations in China. As he puts it succinctly, "countries do not just have one political economy, but multiple political economies" (p. 160).

This work adds momentum to a new wave of studies that makes the point that Western obsession with a few signs of democracy has led to a neglect of the giant sea change occurring in China. Instead of focusing on a few exotic specimens of flowers, these studies—works by scholars like Dali Yang (2004) and Bruce Dickson (2003)—provide strong evidence that a forest has grown up in the meantime. Kennedy's work fits squarely in this literature that challenges the traditional ideal types of state–society relations. According to this author, although business associations do not generally have autonomy, they nonetheless manage to influence government policies in a variety of ways.

Kennedy then makes an innovative theoretical turn by focusing on firm and sector characteristics to explain the variation in lobbying activities in steel, consumer electronics, and the software industries. These characteristics include firm ownership, firm size, profitability, organization, geographical location of production, ownership of the sector, economies of scale, firm concentration, production intensities, level of pollution, product homogeneity, and price elasticity. Instead of focusing on ideal types, he moves the discussion forward by examining the causal effect of these variables. His material reveals that these firm characteristics are clearly important. Here, however, the reader might appreciate a better specified causal framework that links these characteristics to various specific outcomes. Although there are quite a few variables that might matter, a few

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seem especially relevant, given what we know about lobbying and cartel formation, two outcomes to which Kennedy pays special attention. For example, Olson tells us that a concentrated sector with a few large players tends to lobby more successfully and has greater ability to form cartels, while lower price elasticity of demand increases the success of a cartel (1965, 25–26). A focus on these two variables may generate a lot of explanatory mileage in the three sectors.

There are very few people in the China field who have conducted the same depth of field research as Kennedy. Without a doubt, the hundreds of businessmen and government officials he interviewed gave him unparalleled insights about business lobbying in China. For example, his discussion of the State Economic and Trade Commission's (failed) attempts to form multiple cartels during the deflationary period of 1997–98 reveals the messiness and contingencies in economic policymaking in East Asian developmental states. This account stands in sharp contrast to the usual picture of efficient and cooperative policymaking in East Asia. This account makes us wonder whether policymaking had been equally contentious in other East Asian countries. Theoretically, this tells an interesting story in which price elasticity of demand in a sector trumps both sector concentration (as in the electronics industry) and government coordination (in both steel and electronics). For this reason alone, *The Business of Lobbying in China* is an indispensable read for students of contemporary Asia.

Given this wonderful material, one is somewhat puzzled by an obvious gap in the theoretical discussion. For one, this work provides evidence that directly speaks to, if not challenges, the "embedded autonomy" literature, which began with Evans's seminal work (1995). First, Kennedy shows that association life differs between the three sectors, but there is no clear variation in the level of embeddedness between government and industry. In all three instances, government maintained regular contact with industries. In the dimension of embeddedness, China as a whole does quite well, which perhaps explains its relative success among developing countries, exactly as Evans predicts. One interesting challenge that Kennedy's work poses to Evans is the exogeneity of an embedded state. In the case of the software industry, the government at first seemed uninterested in establishing ties. However, as large international players got involved and as the domestic software industry matured, it seemed highly disadvantageous for the government to remain aloof. Thus, the government began to build institutions to engage the new firms. Kennedy shows a strong tendency for such a "state chasing after society" phenomenon in some sectors. Of course, Evans would respond that the right incentives still needed to be in place to motivate bureaucrats to maintain contacts with rapidly developing industries, but this certainly provides an interesting twist to Evans's argument.

In sum, Kennedy's work is a model of how careful field research can yield insights that challenge conventional frameworks and explanations. It further makes a pioneering attempt to describe and explain the novel forms of state–society relations that confound current conceptualization. This work reminds those of us who study China why we began to do so in the first place. Instead of applying off-the-shelf arguments, the complex and often contradictory realities in China challenge us to deploy existing concepts and frameworks in new ways and to invent new concepts and explanations.

New Institutionalism: Theory and Analysis. Edited by André Lecours. Toronto: University of Toronto Press, 2005. 380p. $65.00 cloth, $30.95 paper.

— Irene Ramos Vielba, *Indiana University*

There has been a lot of discussion over the past decade about the relevance of institutions in political science. This has led to the emergence of an explanatory discipline, *new institutionalism,* a prominent social theory that focuses on developing a comprehensive study of institutions, the way they interact, and the effects of institutions on society—thus providing a way of viewing how institutions evolve in very different ways, or how institutions shape the behavior of individual members and produce change. In this collection of essays, edited by André Lecours, a case is made for the ways new institutionalism helps reframe and renew the theoretical and analytical importance of political institutions in the determination of a variety of political phenomena and outcomes, as a group of scholars—mainly from Canadian universities—address a set of complex issues related to new institutionalist research.

New institutionalism does not constitute a unified body of thought. Instead, Peter A. Hall and Rosemary C. R. Taylor (*Political Science and the Three New Institutionalisms,* 1996) identify three schools of thought: historical institutionalism, rational choice institutionalism, and sociological institutionalism. Lecours begins the book with a review of the main conceptual, methodological, and epistemological differences among these three schools of thought and concludes that "there are different ways of conducting institutional analysis, and all of them bring their own insight to the study of politics" (p. 19). With this view that heterogeneity within new institutionalism does not preclude it from theoretical and analytical applicability, he sets the stage for the book's contributors to explore the value of different approaches in the debate over the place of institutions in political science.

There are five main sections in this book. The first includes three chapters and is devoted to theoretical reflections on new institutionalism. The contributions assess some of the main theoretical weaknesses inside the institutionalist framework. The authors revise and clarify crucial ambiguous concepts, starting with *institution,* and

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then considering others, such as *ideas, interests, rationality, strategy, identities,* or *culture,* to integrate them coherently with institutional analysis. Institutional change becomes another central aspect reviewed from an institutionalist perspective. The contributors also address questions regarding the theoretical distinctiveness of new institutionalism, including a critique of historical institutionalism (Chapter 4, "Institutions and Political Rationality," by Hudson Meadwell).

The second section explores institutionalist theory in the study of Canadian politics in both English and French language scholarship. From methodological and epistemological positions closer to historical institutionalism, the two chapters in this section examine the presence of an institutionalist tradition and the extent of the new institutionalism in Canadian politics.

The third section searches into new institutionalism in comparative politics, paying special attention to possible explanations of sociopolitical change. Democratic transitions are the first topic in comparative politics analyzed from a historical institutionalist approach. Nationalism and party aggregation are the other two political processes closely studied, this time from a rational choice institutionalism angle.

The fourth section deals with some connections between new institutionalism and public policy analysis in three different cases. One case centers on the structure of policy networks and the behavior of groups in Westminster parliamentarism. Another case discusses the welfare state redesign involved in the logic of current policy experiments in both Britain and Canada. The last case takes the institutional structure of the American and Canadian political systems together with the dynamic of group interests to explain the failure of both governments—American and Canadian—to amend the Migratory Birds Convention. All the cases, therefore, illustrate the influence of institutions on policymaking, the mobilization of actors, and policy outcomes.

The fifth section focuses on new institutionalist analysis in the field of international relations. This provides insight into intricate issues such as war and peace, sovereignty, international personality, or diplomatic recognition. Institutional elements become pertinent to democratic states' likelihood to engage in warfare (Chapter 13 by Norrin M. Ripsman). Additionally, institutional isomorphism—the tendency to be structurally identical—plays a role in the global institutionalization of "One China," as well as in the spread of the European concept of state among East Asian politics (Chapter 14 by Der-yuan Maxwell Wu, and Chapter 15 by Jeremy Paltiel, respectively).

The wide range of perspectives included in this book provides ample evidence to demonstrate the worth of new institutionalist research in different subfields of political science. Institutions depicted beyond material structures' constraints (old institutionalism) become a substantial vehicle and a quite-influential element in the practice of politics that the new institutional approach tries to apprehend. This extensive collaborative work meets the goal of showing the broad possibilities new institutionalism provides for analytical purposes. However, detractors to new institutionalism can interpret this supposedly positive aspect as one important drawback of the theory since it does not seem to represent a coherent and unified school, but rather a cumulative result of three distinct intellectual traditions embodying different methodological and epistemological positions, as well as separate ontologies and interpretations of political processes. Far from denial, the book takes such diversity as a bigger extent of understanding.

This edited collection makes some significant contributions. Working to confirm rather than extend new institutionalism theory, its primary contribution to the field is its implications for practice, especially to scholars in political science. The chapters offer a broad spectrum of analytical tools within new institutionalism in the scrutiny of the relationship between institutions and other variables. Furthermore, the focus on the study of institutions and their pervasive influence on human actions is of central significance to a number of disciplines, including the provision of, therefore, new interdisciplinary relevance to economics, sociology, and other social sciences.

Understanding the Process of Economic Change.
By Douglass C. North. Princeton: Princeton University Press, 2005. 187p. $29.95.

— Charles Tilly, *Columbia University*

When the Soviet Union collapsed and rapid integration into world markets failed to produce anything like an effective capitalist economy in its Russian successor state, many economists found that failure puzzling. Not Douglass North. So far as I know, North did not predict the Soviet Union's fall or the subsequent chaos of its economy. But he had long since put in place an account of economic change raising serious doubts that markets alone could even function reasonably well, much less transform whole economies, without extensive institutional underpinnings.

Unlike many major North American economic historians of his vintage (for example, his 1993 co-winner of the Nobel Prize, Robert William Fogel), North eschewed econometrics. Instead, alone and in collaboration, he wrote reflective institutional history, asking repeatedly how such innovations as marine insurance and property rights in land promoted economic growth in North America and Western Europe, while the absence of growth-promoting institutions handicapped most economies elsewhere. Recasting insights of Robert Coase, North became a vigorous, vigilant advocate of low or high transaction costs as the key to recognizing the difference between efficient and inefficient institutions. He insisted strongly on the

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distinction between institutions (basically rules of the game) and organizations (basically actors in the game). Effective institutions, in his account, lowered transaction costs. His analyses called for looking at every economy in terms of its institutions, assessing to what extent they both decreased the cost and increased the predictability of economic transactions and their outcomes.

North's *Understanding the Process of Economic Change* lucidly summarizes many of these older analyses. But it moves beyond them in three remarkable ways. First, it commits itself strongly to connectionist (rather than computational) cognitive science as a fundamental alternative to the thin representations of choice-bound rationality that long prevailed in neoclassical economics. North utterly rejects the assumptions of near-certainty built into a wide range of economic analysis in favor of the argument that the human environment and human comprehension of that environment remain irreducibly uncertain. They remain uncertain, he argues, both because human capacity for understanding complexity faces powerful limits and because human interventions themselves alter the world so unpredictably that any projections based on past experience soon run awry. (He repeatedly uses the exotic term "non-ergodic" to describe that discontinuity in human experience.)

Second, North's new analysis represents the accumulation of human beliefs about the world—both true and false—as the central process within which institutional innovation occurs, and therefore the engine of economic growth. "Increase in the stock of knowledge," he declares, "has been the fundamental source of increased human well-being" (p. 17). He portrays humans as incessantly exploring their environment, forming ideas about how it works, trying out procedures based on those ideas, receiving feedback from those interventions, and modifying both beliefs and procedures in response.

Third, North draws heavily on Friedrich Hayek's ideas concerning the (non-Darwinian) evolution of knowledge, belief, and culture. His borrowing from Hayek includes the notion that once humans expanded beyond small circles of acquaintance and coexistence in which they could know and control one another's behavior (and therefore created strongly redistributive economies), they necessarily entered a world that surpassed their ability, individual or collective, to understand and control their social environments.

North displays more ambivalence about Hayek's distinction between *made* and *spontaneous* orders, since the main lines of the Northian argument undercut any hope that the economy's spontaneous order, left to itself, will generate greater human benefits than continuous, concerted, experimental intervention. When it comes to political order, North displays even greater skepticism than the very skeptical Hayek. He argues implicitly that "political markets" necessarily resist coherent analysis more than economic markets do. They do so not only because they require behavioral assumptions concerning the interaction of ideology and self-interest but also because political markets shape economic markets while receiving strong feedback from them. North deftly inverts this last observation, however. Drawing on Hayek and Michael Wohlgemuth, he makes the case for democracy as the institutionalization of open-ended learning and discovery, hence the enemy of authoritarian control over beliefs and interventions.

North never resolves two deep tensions, one quite new and the other very familiar, introduced by his analytic program. First, even accepting that connectionist cognitive science provides a superior explanation of individual decision making than the microfoundations of neoclassical economics, what models of organizational behavior does it imply? After all, institutions supply the rules, but organizations do the crucial behaving. When giving examples of institutional innovation, he does not show us how they result from cognition as cognitive scientists now portray it, but resorts to just-so stories of invention and diffusion. Second, given the deep uncertainty, frequent falsehood, and fundamental "non-ergodicity" of human knowledge, how could we ever know ex ante which increases in knowledge and which institutional innovations would, in fact, reduce transaction costs, generate growth, and increase human well-being? For the most part, North simply returns to American experience, suggesting ultimately that the Soviet Union's economic catastrophe resulted from its failure to build an "institutional structure that in the face of the ubiquitous uncertainties of a non-ergodic world will flexibly try various alternatives to deal with novel problems that continue to emerge over time" and create an innovation-promoting belief structure (p. 154). Without a much more fully specified theory, you cannot get there from here.

The Defeat of Solidarity: Anger and Politics in Postcommunist Europe. By David Ost. Ithaca, NY: Cornell University Press, 2005. 238p. $39.95.

— Jack Bielasiak, *Indiana University*

"Whatever happened to Solidarity?" is an often heard question, echoing the passivity of organized labor during the transition to capitalism throughout Eastern Europe. The position of the working class is especially puzzling in view of Solidarity's role in bringing down communist power in Poland and beyond. Yet the mighty force of the 1980s became a shadow of itself during the construction of markets and democracy, losing members' loyalty, organizational power, and political influence as the country embraced the ideology of neoliberalism. David Ost offers a powerful and original answer to the question of Solidarity's demise, one that goes beyond the oft-repeated claims about economic and social dislocation rendering the working class the "losers" in the postcommunist reconstruction.

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Rather than adhering solely to structural explanations, Ost focuses on political agency as determining the fate of the working class. His bold, persuasive assertion is that workers were abandoned by intellectual allies and union leaders. The new Polish elite embraced a radical vision of the liberal, capitalist future that left little room for accommodation with the working class. Even worse, by overlooking the interests of labor, the liberal establishment pushed the workers to channel their resentment in a populist direction, ultimately embracing the ideology and political parties of the nationalist Right (pp. 66, 95–96).

The author's theory is based on a detailed ethnographic study grounded in repeated fieldwork in the industrial towns of Poland, where Ost visited factories, talked to union activists and rank and file, and interviewed community leaders. This is thick description at its best, painting a detailed, nuanced portrait of the industrial working class during the difficult days of economic transformation. His focus on political action goes beyond prior work on the emasculation of labor activism (e.g., Paul J. Kubicek, *Organized Labor in Postcommunist States: From Solidarity and Infirmity,* 2004). It also challenges interpretations of labor protest in the early 1990s as compatible with the democratic project (Grzegorz Ekiert and Jan Kubik, *Rebellious Civil Society: Popular Protest and Democratic Consolidation in Poland,* 1999). Instead, Ost argues that labor's anger in the face of economic hardship was deliberately ignored and silenced, resulting in the marginalization of the working class that, in turn, had a negative effect on the country's political development. Anger is "structurally intrinsic" to capitalism (p. 28) and must be properly channeled through the inclusion of the working class as an economic force to sustain political democracy.

How did the political and managerial class succeed in sidetracking organized labor and excluding it from politics? The argument looks to the substance of the policy adopted to reconstruct the country, the strategy selected to implement the program, and the political consequences of the choices. Together, these developments alienated the working class, deflated mass participation in politics, and undermined the consolidation of democracy.

In post-1989 Poland, an extreme neoliberal ideology equated the salvation of Poland with a laissez-faire capitalism that was an essential foundation of liberal democracy. At first, workers were persuaded that shock therapy was necessary for national recovery, and so, for example, the union protests of the early 1990s sought to assure the survival of enterprises in the market economy, rather than protect the class interests of industrial labor (p. 151). Even more damaging was the strategy employed to implement the reform. An elitist style was thought to provide the best guarantee for successful transformation without impediments posed by social and economic protectionism. The fear was that a strong working class represented by effective unions would derail market competition, economic globalization, and democratic progress (p. 38). The sentiment was rendered most famously by Lech Walesa: "We will not catch up to Europe if we build a strong union" (p. 53). For the political leadership, it was best to move ahead without consultations with the working class, and so instead of political inclusion, "capitalism by design" was driven by rituals, slogans, and promises that were detached from the emerging reality of social dislocations and economic hardships.

The failure to consult workers extended even to the Solidarity-led government coalition during 1997–2001. The political arm of the Solidarity union continued to advocate the liberal program without sufficient attention to the plight of workers, reinforcing the message of labor's neglect. Rather, to diffuse the growing discontent and anger, the politicians sought to rally workers around the values of solidarity, nation, and church (pp. 107–8). This strategy exposed the bankruptcy of the Solidarity program to protect the economic well-being of its constituents. The workers felt abandoned by former allies among the intelligentsia, betrayed by the national Solidarity leaders, and neglected by factory union activists. The result was a continuing decline of the union infrastructure and membership, eradicating Solidarity as an effective arm of labor's interests (p. 175). Under these conditions, significant elements of the working class had no recourse but to migrate to alternative political solutions, in many cases embracing the populist movement as an expression of workers' needs.

In the end, rather than bind labor to the political process of democratization, the neoliberal strategy had the effect of alienating the working class. Over time, the divide between the political elite and industrial labor undermined the very foundation of a liberal democratic society. Ost makes a good case that the failure to reach out to the workers, to develop mechanisms of consultation, and to listen to union voices endangered the very essence of a pluralist society. Instead, labor felt unwelcome both in its own union organizations and in liberal political institutions.

The value of the work rests in its emphasis on political choices as defining the place of class in building democracy, a contribution that goes well beyond the specific case of Poland to address the capacity to build inclusive, democratic societies (p. 185). The argument is tied to a general theory of politics that stresses the importance of responding to and channeling workers' resentment in order to establish open, pluralist societies. The inclusion of the working class in public discourse is essential for sustaining the democratic process. But in the Polish case, advocates of the neoliberal order feared that a worker's voice in the politics of transformation would culminate in overt opposition, derailing the capitalist future. As a result, they proceeded to silence and devalue the labor class, in the process turning workers away from adherence to the liberal project and into the embrace of a populist agenda.

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Some issues in the study remain underspecified, even while considered in the concluding chapter. Given the emphasis on agency as a determinant of political development and the strong participatory ethos of pre-1989 Solidarity, why did the working class accept the dislocations ushered in by shock therapy? After all, industrial labor proved to be a powerful actor in the past, but during the postcommunist transformation, it has remained on the receiving end of policies and strategies initiated by the intelligentsia and the political class. Perhaps there are features of capitalist liberalization that deflate the initiative of the workers and undermine organized opposition to economic dislocations.

Still, by moving beyond the confines of structural determinism that condemns the working class to the losing side of history in the neoliberal project, David Ost treats us to a provocative analysis that offers an alternative vision. Political leaders, economic managers, and union activists have political choices, and their actions can lead to the building of open, democratic societies that embrace all citizens. As such, *The Defeat of Solidarity* is much more than an account of the fate of the working class in Poland or the path of postcommunism. The book goes to the heart of democratic theory by tackling the issue of inclusionary politics, and persuades the reader that only the incorporation of the most vulnerable citizens in the political process can culminate in successful democratic societies.

Making the EMU: The Politics of Budgetary Surveillance and the Enforcement of Maastricht.
By James D. Savage. Oxford: Oxford University Press, 2005. 239p. $85.00.

— Jonas Pontusson, *Princeton University*

As all students of contemporary European politics know, the Maastricht Treaty of 1992 specified a series of criteria that member states of the European Union had to fulfill in order to join the single currency that would be launched in 1998–99. Most importantly, public debt had to be less than 60% of the GDP and government deficits were not to exceed 3% of GDP. When decisions on initial membership of the Eurozone were taken in 1998, the debt criterion was relaxed, but the deficit criterion was strictly enforced, and as result, Greek membership was postponed for several years. The Stability and Growth Pact adopted by the European Council in 1997, in turn, introduced a procedure whereby the European Commission would monitor the fiscal policies of Eurozone members and would be required to propose sanctions against states with persistent government deficits in excess of 3% of GDP. During 2002–4, the European Commission found Portugal, France, and Germany in violation of the Stability and Growth Pact, but the Council of Ministers declined to impose sanctions on any of these countries.

Thus summarized, the story of fiscal policy coordination under the auspices of the Economic and Monetary Union (EMU) is well known and pretty straightforward. James Savage's recent book brings out another side to this story, which is less well known and in many ways more intriguing. In order for the EMU project to be successful, the European Union had to devise a common framework for calculating government debt and deficits and to implement a "compliance information system" that all member states would consider impartial and therefore politically legitimate. As I learned from Savage's book, calculating public debt in terms of national accounts is not particularly controversial, but the size of the current government deficit very much depends on which organizations are considered to be part of "general government" (local and regional as well as central government) and whether transactions between such organizations and other sectors of the economy are considered to be financial or nonfinancial transactions.

How did the EU succeed in mobilizing member-state consensus around such matters? Much of Savage's account focuses on Eurostat, a fairly obscure body of the European Commission that came to assume the role of lead agency in budgetary surveillance following ratification of the Maastricht Treaty. The author describes how the director of Eurostat, Yves Franchet, managed to expand the agency's remit against the wishes of the powerful Directorate General for Economic and Financial Affairs (DG ECFIN). Apart from Franchet's entrepreneurial leadership and political connections, Savage attributes the ascendancy of Eurostat as lead agency to the fact that it had more expertise in matters pertaining to national accounting and was considered by member states to be a less political entity than DG ECFIN. He also describes Eurostat's long-standing and extensive ties with national statistical institutions and argues that the "epistemic community" of the European statistical profession played an important role in legitimating the process of budgetary surveillance. Not fully articulated by Savage, the implications of this account would seem to be that the launching of the single currency might have been less smooth—more fraught with conflict between member states—had the bureaucratic politics of budgetary surveillance played out differently.

In separate chapters, the author proceeds to discuss the key rulings that Eurostat made regarding the national accounts of specific member states in the lead-up to the decision on EMU membership, as well as subsequent rulings pertaining to enforcement of the Stability and Growth Pact. The most important of these rulings concerned the privatization of state enterprises, but Eurostat has also ruled on the sale of gold by central banks, the sale of mobile phone licenses, debt held by German hospitals, and Italy's one-off "eurotax" of 1997. In the matter of privatization, Eurostat ruled consistently that the sale of government assets is a financial transaction that does not reduce the

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deficit directly, since one asset (say, shares in a corporation) is simply swapped for another (cash). Only to the extent that the proceeds from such a sale are used to pay down public debt, thereby reducing interest payments, does it affect the government deficit.

According to Savage (p. 105), Eurostat rulings "produced marginal, though sometimes decisive changes in budgetary outcomes from what the member states intended." More generally, he argues against those who claim that the surveillance process essentially allowed member states to undertake whatever actions they deemed necessary to meet the Maastricht criteria. In his words (p. 106), "the Maastricht Treaty, in fact, imposed a regulatory structure and regulatory reform on member states' fiscal policies by way of the surveillance procedure, and reshaped rights by declaring ESA [European System of Accounts] to be the measure of government fiscal behavior with the Commission serving as ESA's interpreter." Relatedly, he sides with Eurostat's defenders on whether or not the agency rigged the surveillance process to ensure that certain member states would qualify for EMU membership, arguing that the surveillance process was sufficiently transparent that overtly biased rulings simply were not possible.

Savage's calculations (pp. 139–40) indicate that Eurostat rulings had the effect of bringing the budget deficits of France, Italy, and Spain below the critical 3% cutoff in 1997. Overall, Eurostat rulings reduced the size of the deficit in eight countries, while increasing the size of the deficit in five countries. On average, the countries that benefited from Eurostat rulings in 1997 achieved a deficit reduction of .35% of GDP, and the countries that were adversely affected by these rulings saw their budget deficits increase by .08% of GDP. These figures suggest some systemic bias in favor of across-the-board deficit reduction, which Savage seems to miss because he (like most everyone else) is preoccupied with the question of whether France or some other powerful country received special treatment. Along with the absence of overt favoritism, this bias might help explain the broad-based support for the surveillance procedure.

Savage's careful account of the politics of budgetary surveillance is a must-read for anyone who wants to gain a deeper understanding of how the EU works and the impact of EMU on budgetary practices in the member states. He frames his analysis in terms of principal-agent theory, but the empirical analysis does not seem to depend directly on this framing, nor does the empirical analysis yield new theoretical insights. As far as principal-agent theory is concerned, the take-away message of *Making the EMU* seems to be that "there are key decision points where the Commission enjoys autonomy that resembles trustee-like independence, but the overall process is one that combines moments of trusteeship and a principal-agent relationship" (p. 192).

Class, Race, and Inequality in South Africa. By Jeremy Seekings and Nicoli Nattrass. New Haven: Yale University Press, 2005. 464p. $55.00.

— Dean E. McHenry, Jr., *Claremont Graduate University*

Jeremy Seekings and Nicoli Nattrass present an immense amount of quantitative and qualitative information regarding inequality in South Africa during the apartheid and post-apartheid periods. Their descriptive findings involve assertions that both support and challenge existing knowledge. For example, they conclude that during the apartheid era, there was significant redistribution of income by the state to poorer sections of the population; that apartheid "converted the state-imposed advantages of race into the market-rewarded advantages of class" (p. 379); the "deracialization" of class began not with the assumption of power by the African National Congress in 1994 but rather in the early 1970s; the end of apartheid has not led to a reduction but to an increase of inequality; the "distributive regime" that produces inequality did not really change after majority rule was achieved in 1994; there are now as many rich black South Africans as there are white South Africans; and that the most significant contributor to inequality is the high level of unemployment.

The authors attribute inequalities to what they call the "distributional regime," basically the state policies that directly and indirectly impact the distribution of wealth, including labor market, economic growth, education, and welfare policies. In other words, the explanation for the persistence of inequality—and its consequences—is government's failure to devise policies that would properly address the "problem." To most political scientists, such an explanation is insufficient. It does not push back the "Why?" question far enough. The question "Why do public policies fail to foster a reduction in inequality?" would necessitate an examination of politics. But the authors are reluctant to do such an analysis. They do not address the question of "why or how the South African state adopted some policies rather than others" (p. 48). They also acknowledge that they "say little about the politics of inequality" (p. 48). In a sense, the book cries out to political scientists for an explanation of the state's policies.

Seekings and Nattrass's work is organized like a collection of articles, each of which might be read independently of the other, with an introduction and a conclusion added. Virtually every chapter in their book is concerned with inequality, though most have an associated focus, such as social change, distributional regimes, unemployment, income inequality, social stratification, and the underclass. They are arranged in chronological order starting with the "Eve of Apartheid" and ending with "After Apartheid," though the emphasis is on the associated focus more than the chronology. The result is that each chapter is not organized in a fashion that parallels the other, limiting explicit comparison. Nevertheless, the introduction

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and conclusion provide summaries of the insights developed in each of the chapters.

Some of the central findings—for example, that the end of apartheid did not bring about the end of class divisions or inequality—have been asserted previously by several authors writing from a Marxist perspective, such as Martin Murray (*The Revolution Deferred,* 1994) and Neville Alexander (*An Ordinary Country,* 2002). The most significant addition of Seekings and Nattrass is a variety of quantitative data and an assessment that focuses on the implications of inequalities. In other words, they provide an empirical base to support and extend several of the contentions of radical political economists.

The authors are aware of Marxist scholarship, yet there is some ambiguity in their perspective on such works. On the one hand, in places they debunk much of it. They say that Marxists focus too much on theory (p. 27); that Marxists have an "antipathy" toward quantitative research" (p. 27); that Marxists ignored the impact of education and welfare policies (p. 32); that Marxists "neglected to analyse more fully the ways in which the state shaped the overall growth path of the economy" (p. 32); and that Marxist scholars abandoned class analysis once they "moved into good positions in the post-apartheid state" (p. 29). On the other hand, they claim to build upon it. Indeed, in Chapter 7 (pp. 236–70), the authors delve into the Marxist conception of class as articulated by Eric Olin Wright (*Class Counts: Comparative Studies in Class Analysis,* 1997). In that chapter, they differentiate between Marxist relational and non-Marxist gradational notions of class. They show that inequalities do not correspond precisely with class defined in terms of the jobs held. Nevertheless, elsewhere it is difficult to distinguish between class and groups that they define in terms of inequalities. Whether this ambiguity reflects differences in the points of view of the authors, or whether it reflects uncertainty common to both, cannot be determined by this reviewer.

The authors frequently emphasize the tentativeness of their data and of the claims based upon the data. They use phrases like "the analysis remains preliminary" (p. 237); the "following analysis must be considered as exploratory" (p. 241); the analysis "is bedeviled by a lack of good data" (p. 43); and, surveys in South Africa "provide few data that allow us to demonstrate the consequentiality of class" (p. 44). Two conclusions might be drawn from such comments. First, the authors are meticulous in seeking data to support their findings. Second, the empirical basis for their findings is, in fact, tenuous.

Seekings and Nattrass view the persistence of unemployment as a deepening of inequalities—and as a problem that needs to be overcome. The solution they suggest is a social accord among the state, business, and labor that seeks to expand employment. While such a consociational arrangement might achieve the end they desire, a realistic portrayal of how this might be achieved is missing.

Although I have been critical of aspects of *Class, Race and Inequality in South Africa,* the book deserves applause as well. The authors bring to the reader an immense amount of detailed empirical data on issues related to inequality of importance to both political scientists and the peoples of South Africa.

INTERNATIONAL RELATIONS

The Healing of Nations: The Promise and Limits of Political Forgiveness. By Mark R. Amstutz. Lanham, MD: Rowman and Littlefield, 2005, 296p. $79.00 cloth, $29.95 paper.

International Governance of War-Torn Territories: Rule and Reconstruction. By Richard Caplan. Oxford: Oxford University Press, 2005, 304p. $99.00 cloth.

— Alex J. Bellamy, *The University of Queensland, Australia*

In the late 1990s, the World Bank conducted a far-reaching research program on the causes of civil war. Were civil wars inspired mainly by genuine unresolved political grievances, or were they precipitated by predatory groups (including governments) seeking to profit monetarily from violence and instability? Among the bank's myriad findings, many of them hotly disputed, came one assertion around which there was almost universal consensus: that the single factor that made future conflict likely was past conflict concluded within the previous five years. This finding, based on exhaustive empirical research, has two profound consequences for thinkers and practitioners engaged in formulating and assessing the transition from war to peace. On the one hand, it shows that the risks and costs of failure are high. Failure to foster political reconciliation, to tackle the root causes of conflict, to rebuild shattered economies and (among other things) provide a secure environment is more likely to lead to the outbreak of war and oppression than any other factor. On the other hand, the World Bank findings show that—contra some skeptics who argue that third parties have no part to play and that political conflicts are best resolved violently—long-term success need not be a chimera: Build peace effectively for over five years and the chances of future conflict decrease dramatically. There are clear lessons for Iraq here—civil war need not be inevitable, but to avoid it requires getting a wide variety of things right that are currently going wrong.

The two books reviewed here provide very different approaches to postconflict reconstruction and reconciliation and present sharply different views in several areas.

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Most notably, the context of their discussion is different. Mark Amstutz focuses on how a society might rehabilitate itself after suffering from a repressive government or from terrorism, focusing on the cases of Argentina, Chile, Northern Ireland, and South Africa. Although there was violence, and lots of it, there was no collapse of central authority, no generalized war, no pitting of national or religious groups against one another in open and sustained combat. Richard Caplan's cases are different. Caplan is interested in the reconstruction of polities, economies, and societies after generalized war, when government has collapsed and new states have formed, where there was widespread open combat, where ethnic and national groups had openly targeted one another, and where economies had utterly collapsed.

To that extent, there is a difference of degree that underlies the sharpest differences between their positions. Most notably, Amstutz's skepticism about the place of war crimes trials in a process of reconciliation stems from the context of generalized oppression, rather than generalized war of which he talks. Thus, he argues that evidence of state-sponsored crimes is often sketchy, that it is difficult to secure evidence against key decision makers, and that the crimes may have been authorized by the state that remains in power (p. 39). For these reasons and others, Amstutz prefers forgiveness to trials. By contrast, Caplan argues that the prosecution of suspected war criminals is pivotal for postconflict reconstruction. In the environments that he focuses on, war criminals represent an ongoing threat to individuals and communities already traumatized by war, inhibit intergroup reconciliation by breeding mistrust, and can distort local politics. In sum, a failure to prosecute might leave in place the very people and structures that contributed to the war and compounded the suffering in the first place (p. 66).

These two positions appear irreconcilable unless it is acknowledged that the authors, while addressing a common problem, are situating that problem in different locales. As Amstutz concedes, "favourable conditions may exist" (p. 39) to permit the prosecution of suspected war criminals—most notably "when former regimes are discredited and replaced"—precisely the context that provides the backdrop for Caplan's work. Thus, while the authors do differ in important respects, those differences may be more ones of degree than anything else. Recognizing this allows one to see these two books as complementary, addressing two different but related challenges that confront those engaged in building peace after war. Indeed, together they make an important contribution to the field.

The Healing of Nations addresses the question of political reconciliation, an issue only tangentially touched on by Caplan, who focuses more on the material aspects of reconstruction. The central argument is that reconciliation after oppression requires forgiveness and that alternative approaches such as collective amnesia or legal retribution are flawed in important respects, not least because they do not directly address the question of reconciliation. Amstutz argues that forgiveness is not an "easy way out" or a means of escaping justice, as some of its critics suggest. Instead, it fulfills at least three important functions: "[I]t provides the sole way by which victims can be healed emotionally" (p. 63), it creates a context for genuine repentance on the part of perpetrators, and it facilitates reconciliation by repairing broken relationships. Forgiveness involves the recognition and acknowledgment of crimes and a willingness to provide reparations; and the collective forgiveness itself, to be successful, requires a consensus on truth, genuine remorse, a renunciation of vengeance on the part of the victims, and a high degree of empathy. After assessing the role of forgiveness in the four case studies, Amstutz concludes that although collective forgiveness has not played a "prominent role" in the process of reconciliation, there is a correlation between the degree of forgiveness and degree of reconciliation, with South Africa standing out as the case with most of both.

This book puts forward a coherent, conceptually informed view of the role of forgiveness, but the overall approach is open to a number of criticisms, and Amstutz himself recognizes some of the limitations: Certain crimes simply cannot be forgiven without criminal punishment; sometimes the degree of state fragmentation caused by oppressive rule or war mitigates against the sorts of transition that Amstutz discusses; and—it could be argued—forgiveness does little to directly address the underlying political, social, or economic causes of conflict and oppression. While important, therefore, forgiveness alone does not capture the full range of concerns that animate those interested in building peace after war. Such concerns must be set alongside the wide range of material matters raised by Caplan.

International Governance of War-Torn Territories is an important book that focuses specifically on the recent phenomenon of international administrations in territories affected by war where the former system of government has utterly collapsed or is in need of transitioning to a new form of government (as in Eastern Slavonia). Caplan offers a detailed and insightful account of transitional administrations and lucidly demonstrates the sheer scope and complexity of such endeavors. As with Amstutz's book, Caplan's focuses on a small number of cases—primarily because there have only been a relatively small number of such administrations, in Kosovo, Bosnia, Eastern Slavonia, and East Timor—but this book is organized thematically rather than by case study. Caplan begins by distinguishing between transitional administrations and peace operations, noting usefully that the former involves assuming the full reigns of government. This makes life particularly difficult in cases where the same organization assumes responsibility

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for government and for the associated peace operation—effectively being simultaneously the host and the intervener.

The first half of the book outlines the five critical areas that transitional administrations must address if they are to create the building blocks for self-sustaining peace: the provision of public order and security, the return of refugees and displaced persons, the provision of civil administration covering the whole range of services provided by states, the need to build political institutions to assume the reins of government, and economic reconstruction and development. In the second half, Caplan focuses on five critical problems associated with this type of operation—most notably the question of accountability. Crucially, although the role of forgiveness and emotional recognition is downplayed, the paramount conditions that make for a successful administration are the status of the different belligerents and the degree of potential for political reconciliation.

Individually, these books are powerful testaments to their particular approach to developing strategies for building peace in those crucial years after war. The differences of those approaches are clear to see in areas where the authors disagree—most notably on the relative merits of war crimes trials. Acknowledging, however, that to some extent these books are prefaced on quite different contexts allows one to see the ways in which they offer complementary accounts. In short, no amount of institution building and economic reconstruction will foster long-term sustainable peace if such activities are not conducted in tandem with political reconciliation. The converse, however, is also true. Forgiveness and reconciliation without the material and institutional building blocks for legitimate and effective future governance is unlikely to succeed by itself. Together, therefore, these books deliver salient lessons for those charged with bringing peace to Iraq.

Rules for the World: International Organizations in Global Politics. By Michael Barnett and Martha Finnemore. Ithaca, NY: Cornell University Press, 2004. 240p. $17.95.

Institutional Change and Globalization. By John L. Campbell. Princeton: Princeton University Press, 2004. 264p. $55.00 cloth, $17.95 paper.

— Pepper D. Culpepper, *Harvard University*

Joined by their pursuit of the causal mechanisms that drive institutional and organizational change, these authors provide a window on the promise and perils of life on a research frontier. John Campbell details the shortcomings of institutionalist theory in the face of institutional change, using the empirical case of globalization for largely illustrative purposes. Michael Barnett and Martha Finnemore aim to put the "organization" back into the study of international organizations, using three case studies to locate the roots of mission creep not in the demands of states but in the bureaucratic cultures of organizations themselves. These books share the claim that current institutional and organizational theories fail to incorporate the ways in which ideas determine change and stability.

Campbell moves easily among the three new institutionalisms—rational choice, organizational, and historical—arguing that despite their differences, three shared problems obstruct their progress in explaining institutional change: failure to specify the meaning of institutions; failure to identify the mechanisms that underlie the concepts of path dependence and diffusion; and failure to incorporate ideas, beyond the idea of self-interest. These three areas are the subjects of the core chapters in *Institutional Change and Globalization,* and they are extremely worthwhile reading for anyone interested in these problems.

We cannot explain institutional change if we cannot operationalize it empirically. This is a frequent source of misunderstanding between, for example, rational choice institutionalists focused on institutions as strategic equilibria and historical institutionalists concerned with the regular behavior induced by existing political institutions. Campbell argues that the prerequisite to resolving such conflicts is to identify the different dimensions of an institution, as well as the time frame over which change is observed. Underlining this point raises the fundamental question of whether it is informal institutions that determine change in formal institutions or vice versa (Pepper D. Culpepper, "Institutional Change in Contemporary Capitalism, *World Politics* 57 [January 2005]: 173–99).

Similarly, although institutionalists have clearly specified mechanisms of persistence, they have often failed to specify the mechanisms that account for changes in the same institutions, falling back on the deus ex machina of the exogenous shock. Scholars such as Wolfgang Streeck and Kathleen Thelen have made similar observations, but their preferred analytical strategy is to chart the various types of evolutionary changes. Campbell urges instead a focus on the mechanism at work during the key moment of institutional change: "the search process . . . whereby actors figure out evolutionary or revolutionary changes to make in their institutions" (pp. 68–69). Identifying the important actors and the process by which they craft solutions to the problems confronting their institutions is the central explanatory goal in Campbell's enterprise, and the two causal mechanisms he identifies are bricolage and translation. Bricolage is the process by which actors "craft new institutional solutions by recombining elements in their repertoire" (p. 69); translation is the process by which actors take new ideational elements and combine them with existing institutional practices.

Concentrating on such mechanisms naturally leads to a set of propositions about the role of ideas in institutional change. Ideas can be either cognitive or normative, and Campbell suggests a further distinction between ideas that

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sit in the foreground (programs and frames) and those that remain in the background (paradigms and public sentiments). This latter distinction seems difficult to sustain: While the shift from Keynesianism to monetarism in Britain was certainly a change of paradigm, it is clear that the Conservatives under Thatcher put the paradigm shift to monetarism at the forefront of their political campaign (Peter Hall, "Policy Paradigms, Social Learning, and the State," *Comparative Politics* 25: 275–96). In the normative category, particular frames are likely to be more or less successful to the extent that they draw on widely shared public sentiments. Indeed, to follow the thrust of the mechanisms of both bricolage and translation, the more important variation in ideas we may want to track are the extent to which they are coordinative—that is, dependent for their effect on what actors think about what others think. We should expect different dynamics where institutional change involves a move from one coordinative equilibrium to another and where this is not the case.

Where Campbell's explanatory footing is much surer is in his strongly argued defense of the importance of agency in making ideas stick: "[I]deas do not emerge spontaneously or become influential without actors, and so it is important to situate these actors and theorize their roles vis-à-vis ideas in any account of institutional change" (p. 100). His review of the ways in which different sorts of brokers champion ideas thoughtfully synthesizes much of the existing literature on how ideas came to matter. Although the book's empirical work on globalization is itself cursory, Campbell lays out demanding empirical standards for institutionalist research to meet in order to answer the big questions of institutional change that top its agenda.

In *Rules for the World,* Barnett and Finnemore take aim at an object they find understudied in international relations: international organizations (IOs) qua organizations. Such IOs are assumed by many current theories to be either a tool of state interests or a functional instrument by which states solve their problems of cooperation. The authors' contribution is not so much to study neglected IOs—see the vast literature on the European Union—but to develop a theoretical approach that combines a Weberian understanding of the rationalizing tendencies of bureaucracies with a constructivist insight into the power of information. Disputing the tattered claim that IOs gain their power from information asymmetries—that is, knowing more than their member states—Barnett and Finnemore argue that IOs instead "are often the actors to whom we defer when it comes to defining meanings, norms of good behavior, the nature of social actors, and categories of legitimate action in the world. IOs are often the actors empowered to decide if there is a problem at all, what kind of problem it is, and whose responsibility it is to solve it" (p. 7). Their answer to that last question, frequently, is "IOs."

The role of bureaucratic rules is central in this theoretical framework. Bureaucrats owe their position to their expertise, and they see the world through the lens of a bureaucratic culture. These bureaucratic rules are especially important in explaining organizational action and inaction—and over time, how organizations change—in the face of uncertainty. The mechanism here is one that Barnett and Finnemore call translation: Faced with uncertainty, "bureaucrats will act creatively as they rearrange old organizational notions and import or invent new ones to solve pressing policy problems" (p. 19). Such movement is most clearly on display in their case study of the International Monetary Fund, whose goals moved from dealing with balance-of-payment crises to the domestic sources of such crises—including "good governance rules"—that seem far removed from its initial policy mandate. The monetary focus of the Fund's construction of balance-of-payment crises pinned the burden of adjustment on deficit states, but such an outcome was not inevitable, as their carefully argued counterfactual suggests: "[O]ne obvious direction that the Fund's work could have taken would have been to expand Fund influence over surplus states in some way as a means of promoting systemic adjustment" (p. 55). This may sound outlandish to our IMF-influenced ears today, but for the fact that the IMF announced in April 2006 that it was embarking on exactly such a systemic approach that would include multilateral surveillance.

Whether or not this policy change will have teeth depends on how the richest IMF member states respond. And it is at this point that Barnett and Finnemore flinch before what they call the statist challenge: Is not the direction of bureaucratic change fundamentally hemmed in by the definition of state interests? As the authors themselves observe (p. 14), there are "obvious and widely accepted statist explanations for the behavior of IOs" in the case of the IMF, as with the United Nations High Commissioner for Refugees and the UN Security Council in Rwanda, their other cases of mission creep (and mission retreat). Indeed, in the Rwandan case, their evidence on the importance of the Somalia "framing" effect is very compelling as to why the UN shied away from intervention despite clear evidence from UN personnel on the ground that a genocidal conflict was under way. Yet it is likely that the "Blackhawk Down" effect of Somalia was even more important in driving the behavior of member states of the Security Council, and notably that of the United States. While there is fertile ground here for ideational explanation, pointing the primary finger at the bureaucratic culture of the UN would need to be supported by much more evidence of the willingness of states to intervene, had the UN bureaucracy acted differently.

These two books make bold and creative arguments about the factors that drive institutional and organizational change. The causal mechanisms through which actors

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build and reconstitute their worldviews are at the root of how each book tries to explain change. Each shows that ideas play an especially important role when uncertainty is high, and actors are consequently open to new ideas. Clarifying the ways in which new ideas spread and the role of entrepreneurs and organizations in pushing these ideas, as these authors stress, are the sine qua non of a successful social scientific account of institutional change. Yet showing that ideas beyond self-interest matter for ultimate outcomes also requires showing that alternative hypotheses based on self-interest cannot explain the outcomes observed. The payoff of research on this frontier can be high, as these books show. The continued challenge they pose for others doing similar work is to deploy sophisticated and creative ways that demonstrate how ideational mechanisms provide superior explanatory leverage to those scholars instinctively skeptical of them.

The Evolution of the Trade Regime: Politics, Law, and Economics of the GATT and the WTO. By John H. Barton, Judith L. Goldstein, Timothy E. Josling, and Richard H. Steinberg. Princeton: Princeton University Press, 2006. 256p. $29.95.

— Kerry A. Chase, *Tufts University*

The scope of this book is impressive. Eight chapters detail the history and institutional design of the World Trade Organization and its forebear, the General Agreement on Tariffs and Trade (GATT), analyze their successes and failures over nearly 60 years, and outline challenges that the regime must manage to remain politically viable. As the subtitle suggests, the authors examine the politics that have shaped and sustained the trade regime; legal issues, particularly in the settlement of trade disputes; and economics as an underlying factor in the regime's evolution, along with the trade effects of its presence.

The book's core arguments focus on two key problems that the WTO faces: first, challenges to its legitimacy from underrepresented countries and newly mobilized nonstate actors; and second, the threat from the continuing profusion of regional trading arrangements. The legitimacy issue is multifaceted, and successive chapters effectively analyze its different sides. The threat from regional trading arrangements is less complicated, and this part of the argument recedes into the background, apparently overshadowed by the trade regime's larger problems.

Challenges to the legitimacy of the WTO have come from two corners: developing countries, whose influence in trade negotiations falls short of their numbers, and nonstate actors (now including environmental groups, organized labor, and antiglobalization activists, in addition to businesses), which lack institutional access at the multilateral level. Though the interests of developing countries and nonstate actors sometimes coincide—Oxfam has spotlighted the harmful effects of U.S. and European Community (EC) subsidies for farmers in developing countries; Médecins Sans Frontières has pushed for changes in intellectual property rules to make AIDS drugs more widely available to the poor—often their goals stand in opposition, as the book notes of environmental regulation, labor standards, and outsider participation in dispute settlement. Many institutional and policy choices, therefore, will dissatisfy a constituency whose support is essential to the WTO's effectiveness.

For developing countries, the legitimacy issue is that the most powerful players, the United States and the European Community, remain the core of the trade regime. Although members are formally equal under the "one nation, one vote" principle and decision rules operate mostly by consensus, trade cooperation occurs "in the shadow of power politics" (p. 205). Specifically, negotiating procedures such as "Green Room" meetings and the ability of the leading countries to extract concessions through threats of market closure mean that the interests of the United States and EC dominate in practice. A notable example is the assent of developing countries to the Uruguay Round's "single undertaking," which occurred after the United States and the EC issued credible threats to punish those who would try to pick and choose among the agreements. This formula, which bound developing countries to observe new rules for intellectual property, services, and trade-related investment measures despite limited concessions on textiles and agriculture, produced an asymmetric distributive bargain—the terms of which are enforceable in the WTO dispute settlement system. Fortifying the trade regime's authority with developing countries, the book implies, requires that the Doha Round revise this bargain to achieve greater mutual benefit.

For nonstate actors, the legitimacy issue is the "democratic deficit" at the WTO. Like its predecessor the GATT, the WTO is a member-driven organization; nonstate actor participation is limited to lobbying member states and filing amicus curiae briefs if invited by a dispute settlement panel. This exclusiveness worked well in the GATT, where negotiations traded off business interests to liberalize barriers at national borders. But in the WTO, where attention has shifted to "inside the border" regulatory rules, the spillover effects for labor, consumers, and the environment engage a larger number of actors and interests. If the trade regime cannot account for these new stakeholders, "they will likely remain in the streets, protesting against the system" (p. 16). However, readers seeking a more open and inclusive WTO will be disappointed: "[C]omplete democratization is infeasible" (pp. 182–83); added transparency is undesirable because imperfect information remains critical to forging difficult bargains that special interests might otherwise block; and while nonstate actors can play a useful role in dispute settlement, it is best not to cast too much light on how judicial decisions are sometimes packaged for political

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appeal. Rather, the institutional reform the book advocates is to establish new mechanisms for popular participation at the domestic level in order to prevent a recurrence of debacles like the 1999 ministers' meeting in Seattle.

Compared to its treatment of the trade regime's legitimacy problem, the book pursues the threat from regional trading arrangements with less conviction. Its preface worries that "bilateral arrangements could ultimately undermine interest in more global trade relations" and notes that "there are serious implications for developing nations that are left out of such free trade regions, and strict rules of origin may exacerbate the diversion of trade that arises from such preferential access" (p. xii). Rather than emphasizing the exclusion of developing countries, however, Chapter 6 lauds "developing and developed countries' success in coexisting in the same PTA" (p. 178). Moreover, there is no mention of how rules of origin distort trade. Instead, a more forgiving view emerges: Regional trading arrangements are "best understood as a functional part of the trading regime" (p. 54) because they "set the stage for multilateral arrangements by changing the political support for open trade," and they provide "test beds for multilateral action" on new issues such as services and intellectual property (p. 55). Though additional discussion of the ineffectiveness of WTO oversight may be warranted, it seems that regional trading arrangements are not a significant threat to the trade regime's efficacy.

The Evolution of the Trade Regime is an excellent study of the trading system, cohesive and robust despite its sharing among four authors. The familiarized reader will recognize the individual stamp of the different contributors, yet the book stands on its own—it is not a recapitulation of their prior work. And in critically evaluating the trade regime in all of its shortcomings, the book depicts a largely healthy, still vigorous WTO that likely will remain the system's central feature into the future.

Contesting Globalization: Space and Place in the World Economy. By Andre C. Drainville. New York: Routledge, 2004. 208p. $145.00 cloth, $41.95 paper.

— Aseem Prakash, *University of Washington, Seattle*

The literature on globalization has come a long way. The first-generation scholarship debated the "decline of the nation-state" thesis. Globalization critics predicted widespread regulatory races to the bottom. They blamed technological innovations that allow firms to decouple various stages of the production processes for privileging capital over other societal actors. While the globalization optimists agree with the causal story of the critics, they welcomed the emergence of a "borderless world." For them, capital mobility leads to good governance because it privileges economic efficiency over rent-seeking politics.

The second-generation globalization literature adopted a more expansive view by recognizing the interplay between economic globalization (that is, increased cross-border integration of factor, intermediate, and final product markets) and noneconomic globalization. Technological innovations that facilitate capital mobility also enable global diffusion of (Western) norms and the emergence of the global civil society whose transnational networks challenge transnational capital.

The third-generation scholarship examines the "democracy and equity deficits" in institutions and policies adopted to cope with globalization. While such deficits are likely to be pervasive in intergovernmental and private authority regimes, scholars suggest that the global civil society might accentuate these deficits as well.

As an important addition to the third-generation scholarship, *Contesting Globalization* argues for situating the globalization discourse in concrete locations. For Andre Drainville, global cities provide the appropriate terrain to investigate globalization politics for they constitute the sites of encounter among social forces. The foreword by Saskia Sassen, the leading proponent of the global cities perspective, is illuminating. She notes approvingly that "the global city is a site where global corporate capital and multitudes of disadvantaged can engage with each other, where place-based politics (e.g. gentrification struggles) becomes a form of global politics" (p. xiii).

The introduction and the first chapters are well written and lay out the book's main argument. Drainville is critical of the second-generation literature for it masks the politics and struggles of the disadvantaged. However, he is equally critical of the sociological and the Marxian approaches for they focus on abstract subjects functioning in abstract spaces. As he rightly notes, "Holy ghosts of all sorts are haunting global politics: 'global civil society', 'international public opinion', 'the people of the earth'" (p. 1). Emphasizing the importance of specific locations in structuring the political economy, he notes the role of Amsterdam during the Dutch hegemony, London during the Pax Britannica, New York during the Pax Americana. He goes on to suggest that in the age of Pax Planeta, "we will imagine the world economy as a city, to help us think critically about the making of the world order" (p. 7).

The second chapter, the most interesting part of the book, presents "three moments in urban history when the movement of urban social forces highlighted the very different modes of relation to cities at the core of the world economy" (p. 17). These moments are the dockworkers' strike in London (1989), the defeat of the Tammany Hall political machine in New York (1894), and the protests in the city of Quebec against the Summits of America (2001). London and New York constituted the central nodes of the world economy and therefore became the central locations for political struggles. I found these cases very

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illuminating. They successfully articulated the main purpose of this book: an examination of how social actors get created in *concrete* locations and the opportunities cities present in this regard. The third case, Quebec, was less compelling. This city hosted two rival "global summits" of actors who visited for a short duration of time, but its substantive importance in global capitalism was not clearly explained. Further, unlike the London and New York cases where social forces rooted in concrete places interacted and contested over a period of time, the rootedness of such forces in Quebec was not apparent. It seems that the abstract global actors whom Drainville criticizes assembled in Quebec to undertake decontextualized political action.

Chapter 3 first examines the politics of leftist internationalism from the 1840s (Pax Britannica) to the 1970s (the crisis of Pax Americana) and then the protest politics of the 1980s and the 1990s in terms of campaign-centered oppositional movements. Chapter 4 examines how over time, civil society has been incorporated in global and regional policymaking. Drainville argues that global capital responded initially to the Bretton Wood crisis by promoting universalist programs such as monetarism. When such global solutions failed, global capital advocated "global governance" as a way to ensure a reliable political and social order. The strategy was to co-opt its critics, elevate them to the status of global citizens, and arrive at nonradical solutions. The newly anointed global citizens, as the author suggests, become agents of order, not soldiers in political struggles. He finds the global governance projects problematic because they deradicalize politics. The global civil society creates fictitious, decontextualized citizens who owe their globality to forces that control the world order. I found the argument to be quite compelling. Indeed, scholars of NGO politics have been less attentive to the structural forces that have led to the emergence and the co-option of such organizations. Chapter 5 seeks to present new concepts for the critical articulation of global political economy. I found this less interesting and often difficult to understand. However, the concluding chapter is well written and carefully pulls together the key arguments made in the book. Drainville's point about the "unreal universality of the cosmopolitan ideology" (p. 160) is well taken.

Overall, *Contesting Globalization* is an ambitious book that I recommend to scholars interested in the study of globalization. In particular, I believe, it will be helpful to NGO politics scholars precisely because it identifies the limitations of their approach. One might argue, however, that global capitalism is taking a gamble by creating a global civil society that may become radicalized in the future. It follows, therefore, that a radical agenda might be to infiltrate and capture such groups with the objective of socializing them in the politics of protest instead of the politics of acquiescence.

Darfur: A Short History of a Long War. By Julie Flint and Alex de Waal. New York: Zed Books, 2005. 176p. $60.00 cloth, $19.99 paper.

Darfur: The Ambiguous Genocide. By Gérard Prunier. Ithaca: Cornell University Press, 2005. 232p. $24.00.

— Scott Straus, *University of Wisconsin-Madison*

In the last two years, the Darfur region in western Sudan has moved from relative international obscurity to become a symbol of humanitarian crisis and mass violence. Political scientists who research genocide, ethnic conflict, civil war, humanitarianism, and African politics all have taken interest in the region, and Darfur is likely to command scholarly attention in years to come. Yet the academic literature on the region remains thin. To date, scholars have relied primarily on journalistic accounts and human rights reports, which detail the violence but, by their nature, provide only cursory historical background. With the publication of these two short but informative books, Darfur's political history and the path to mass violence are substantially clearer. That said, the books are not designed to build theories of ethnic violence or genocide, nor do the authors explicitly engage in hypotheses testing. The books are useful primarily as detailed, lucid case histories from two sets of well-informed observers.

On the whole, the two books tell a similar story about the origins of the conflict. That story, however, is a complex one, with interlocking local, national, and regional factors. Both books stress a history of political manipulation of ethnicity by local and national actors; both highlight a central dimension of competition for scarce resources, recently exacerbated by drought and desertification; both trace how the government funded and supported local militias, who in turn unleashed large-scale violence; both posit a connection between the war in Darfur and the decades-long but recently ended war between Northerners and Southerners in Sudan; and both attribute a critical role to regional factors, in particular the involvement of Chad and Libya. Whether the recent violence in Darfur should be called "genocide"—a focal point of public discussion—is not central in either book.

The major differences concern style and emphasis. *Darfur: The Ambiguous Genocide* is written by Gérard Prunier, a French researcher on African politics who wrote an influential book on the Rwandan genocide a decade ago. *Darfur: A Short History of a Long War* is a collaboration by Julie Flint and Alex de Waal. Flint is a journalist and researcher for Human Rights Watch; de Waal is a longstanding analyst of Sudanese politics, and he wrote one of the few English-language books on the Darfur region before the latest crisis (*Famine That Kills,* rev. ed. 2004). Prunier takes more opinionated license in his prose than do Flint and de Waal, and, given Prunier's prior research on Rwanda, that conflict serves as an occasional reference point. Flint

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and de Waal explicitly seek to narrate Darfur's history on its own terms (rather than in reference to Rwanda or the North–South war in Sudan), and their account is especially strong on the local dynamics of the conflict. They provide a wealth of information about Darfuri politics and history, including biographical accounts of rebel and militia leaders, as well as nuanced discussions of political lineages and movements in the region. Prunier focuses more on the national and international dimensions of the conflict.

Roughly the size of Texas, Darfur is home to some six million people and as many as 90 different ethnic subgroups, according to Flint and de Waal. The region's political history is distinct from the rest of Sudan. An independent sultanate, Darfur was incorporated into colonial Sudan in the 1910s, later than the rest of the country. Both authors describe how Darfur was largely neglected under colonialism. Flint and de Waal claim that Darfur was a "backwater" (p. 16). The marginalization of Darfur continued into the postcolonial era, and indeed a near century of neglect vis-à-vis Sudan's center is a running theme in both books. Darfur's demography is also distinct from much of the rest of Sudan. While Sudan is characterized by crosscutting cleavages between North and South, Arab and black, and Christian and Muslim, Darfur's population is Muslim. That said, there is an important cleavage between Arabs and Africans in the region, and that cleavage shapes the contours of the recent violence.

Conflict between Arabs and Africans in Darfur became especially pronounced in the 1980s. A series of intense droughts in the region intensified competition for water and arable land, leading to violence. Civil war in neighboring Chad also was significant. To unseat the Chadian government in the 1980s, Libyan leader Muammer Gaddafi funded and supplied Chadian rebels, whose base was located inside of Darfur. Gaddafi's involvement prompted an influx of weaponry to Darfur, but, as Flint and de Waal recount, Libya's entanglement also led to the promulgation of Arab supremacist ideology in the region. The authors focus attention on the "Arab Gathering," a group that promoted Arab domination. Another source of conflict in Darfur was the North–South war in Sudan. To combat southern rebels, who at one point sought to make incursions into Darfur, government leaders backed Darfuri Arabs. The net effect of these various factors was to harden Arab and African identities in the region and to intensify tension between the two groups. Prunier claims that since the mid-1980s, "Darfur had been a time-bomb waiting for a fuse" (p. 86).

A major change came in 2002 and 2003 when Africans in Darfur launched a rebellion, seeking an end to Arab favoritism. Initially, government forces did not take the rebels seriously. But as Flint and de Waal recount in detail, the rebels won a string of military victories, prompting a hard-line response from the government, whose officials were worried, in part, about what successful armed resistance in Darfur would signal to other restive groups in Sudan. According to all the authors, the government's strategy became to crush the rebellion by arming militias and supporting them with military, in particular air, power. Militias and soldiers in turn launched waves of attacks on African civilians. Prunier (citing an article by de Waal) calls this "counterinsurgency on the cheap" (p. 99). The outcome was devastating: Joint militia and military forces destroyed villages, killed, raped, and looted. Flint and de Waal quote a document from a Darfuri militia leader claiming that the purpose of the violence was to "change the demography of Darfur and empty it of African tribes" (p. 39). Both books provide considerable detail about the violence, which ultimately led to the displacement of more than two million civilians and the deaths of several hundred thousand.

Is the violence "genocide"? That question was a focus of debate in the United States and ultimately led Congress, Colin Powell, and President George W. Bush to declare that genocide was happening. Prunier claims that Darfur is an "ambiguous" case of genocide. Under the United Nations Genocide Convention, it would be, he argues. Under his definition, which sees genocide as destruction of a group in its entirety (rather than in substantial part), Darfur would not be genocide. Prunier is skeptical of the genocide debate, claiming that it is motivated by the media's need for a "big story" (p. 156) and labels. His assertion goes too far: Because of the UN Genocide Convention, a genocide designation is significant. He also provides little evidence to support his claim that Powell "had practically been ordered" (p. 140) to use the term. Rather, Powell had commissioned a comprehensive and innovative study from an independent organization so that he could make a determination, and he studied those results before reaching a conclusion.

Prunier criticizes the American administration for being "confused" on Darfur, but his criticism is not limited to the Americans. He excoriates European Union leaders for "complete lack of resolve and coordination," the British for "blindly" following Washington, and the French for being concerned only with Chad (p. 140). UN leaders also come in for criticism. As for the African Union, which deployed troops but only with a weak mandate, Prunier argues that Darfur was its first major crisis but one that was a "mission impossible" (p. 145), given the limited number of troops and their restrictions. Flint and de Waal have a similar assessment of the African Union. As for other international actors, they fault Western leaders for not backing up strong words with strong actions. The authors also highlight how China and Russia, which are allies of Sudan, were obstacles to sanctions and other measures at the United Nations.

Both books include damning commentaries on Sudan's national leaders. Flint and de Waal compare them to a

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"cancer" (p. 129), fault their "withered" humanity (p. 129), and assert that there are "serial war criminals at the heart of Sudan's present government" (p. 134). Prunier finds that Sudan's policy "has kept verging on genocide" (p. 105) since the current leadership came to power in 1989. These are not value-neutral claims, but nor are the events described in these pages. However readers respond to the characterizations, the books are excellent introductions to a conflict that will continue to interest policymakers, students, and scholars working in a variety of political science subfields.

The Far Enemy: Why Jihad Went Global. By Fawaz A. Gerges. New York: Cambridge University Press, 2005. 358p. $27.00.

— William B. Quandt, *University of Virginia*

The 9/11 attacks have already produced a substantial literature that seeks to explain the roots of Al Qaeda's strategy of targeting the United States. Explanations range from simplistic statements—"They hate us," "They hate freedom"—to arguments about revenge or punishment for various American policy choices, to more complex arguments that Osama bin Laden and his associates sought to draw America into a trap of sorts. One problem, of course, is that no one really has much of an idea of precisely what led bin Ladin and his top associates to plot the 9/11 attacks. Scholars do not, quite frankly, have a lot to work with here.

Fawaz A. Gerges is convinced that most accounts of 9/11 are flawed. His own research relies heavily on reading available accounts written by Islamist activists—he uses the term *jihadis* to refer to those who see violent jihad as a legitimate means for advancing Islamist goals—and on interviews with many former jihadis. Like many others, he places emphasis on the ideological legacy of Egyptian writer Sayyid Qutb, whose 1964 book *Signposts* is a reference work for those who justify violence against fellow Muslims because of their apostate views and behavior.

What is most distinctive in Gerges's study is his insistence on the relatively marginal role that Al Qaeda and bin Laden have played within the larger jihadi movement. On the whole, his argument is convincing but needs to be examined carefully. He correctly notes that radical Islamism took root in places like Egypt and Algeria in the 1970s–90s timeframe largely as a response to repressive regimes. The targets of Islamist violence were primarily the "near enemy"—the existing governments that had often been very forceful in repressing Islamist currents at home.

The other and interrelated source of radical Islam during the 1980s was the anti-Soviet campaign in Afghanistan, which was supported by Saudi Arabia, Pakistan, and the United States. This is where bin Laden and many other of his generation got their first taste of jihad—this time with the full blessing of the United States.

By the mid-1990s, this current of jihad had suffered severe setbacks. The Egyptian and Algerian regimes, for example, cracked down brutally on the armed Islamists in their midst, forcing some of them to accept a truce, while others fled into exile. Among those who fled were a number of Egyptians who ended up in Afghanistan, alongside bin Laden. Most prominent was Ayman al-Zawahiri. He and bin Laden merged their respective followers to form Al Qaeda in the late 1990s. What was unique about Al Qaeda, according to Gerges, was its insistence on striking at the "far enemy," rather than to continue the futile struggle against entrenched regimes in Cairo, Algiers, or Riyadh. One of Gerges's most significant contributions is his demonstration, with ample quotations from relevant sources, that many jihadis felt that this strategy would be suicidal and would distract from the need to keep the focus on the home front.

Gerges correctly says that the personalities of bin Laden and Zawahiri counted for a great deal in setting the strategy for Al Qaeda. Neither of these men listened much to their associates. Both were intensely committed to the struggle against the United States and demanded deference from those around them. Gerges picks up more than a few hints that bin Laden and Zawahiri did not always see eye to eye, and that factionalism ran rampant within Al Qaeda and the broader jihad movement. He correctly points out that such factionalism is quite common within extremist movements.

For all the rich information provided by Gerges, I am still not sure what he thinks caused bin Laden to plan 9/11. He says that "transnational jihad," with America as the target, provided a new opportunity for jihadis after the heroic defeat of the Soviets in Afghanistan (pp. 132–33), and that bin Laden and Zawahiri "plunged into a confrontation with the United States, hoping that it would serve as a galvanizing and unifying experience" (p. 190). In short, the decline of jihad after the victory in Afghanistan and the defeats at the hands of the "near enemy" required something new and dramatic.

That explanation is fine as far as it goes. But did bin Laden also think a few steps further and imagine that the United States would react in ways that might produce a new generation of jihadis? That is not so clear from Gerges's analysis. Some have made such an argument explicitly, and Gerges seems to agree when he says, in passing, that the U.S. response to 9/11—declaring "a worldwide war against a nonconventional, paramilitary foe with a tiny or no social base of support and try[ing] to settle scores with old regional dictators"—was "exactly what bin Laden and his senior associates had hoped the United States would do" (p. 271). He also fears that Iraq may provide second-generation transnational jihadis with revived prospects. But overall, he is convinced that jihadis are an endangered species, that their moment has come and gone (p. 222, "the sinking jihadist ship"). If we analyze carefully what

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they say and do, we will see that they have little support among Muslims, and that the tide of public support has turned against them. I think Gerges is right, but even a small, alienated, unpopular movement that is determined enough can cause a great deal of trouble.

While readers will learn a great deal from this book, especially about the internal debates among jihadis, there are some shortcomings. I think that Gerges draws too much of his information from Egyptian sources, while paying little attention to Algerians, Palestinians, Saudis, or Pakistanis. In addition, some of the memoirs that he relies on were written by jihadis in prison, and some of what they say (praise of Anwar Sadat, pp. 208–9) must be taken with a grain of salt. He does not exactly say that some of his informants were also imprisoned, but it seems as if that may have been the case. That does not necessarily discredit their testimony, but it is important to know the conditions in which such interviews take place.

Gerges provides a generally convincing account of the why jihad went global. His book deserves to be widely read. But the topic is so much a part of today's headlines that I wish he and his editors had resisted the temptation to get into print so quickly and tried to address current policy issues, such as Iraq. The resulting text will show its age quickly, as current events take whatever turn they are going to take. And the book as it stands seems rushed, often repetitive, sometimes poorly edited, and a bit too insistent that the author is presenting a "nuanced" and "complex" account, presumably in contrast to the simplistic and obvious accounts written by others. The analysis is good enough to speak for itself without the self-promotion that introduces it. These are small matters in an important book, but they are an unnecessary distraction.

Nation-States and the Multinational Corporation: A Political Economy of Foreign Direct Investment. By Nathan M. Jensen. Princeton: Princeton University Press, 2006. 224p. $35.00.

— Stephen J. Kobrin, *University of Pennsylvania*

The dramatic increase in flows of foreign direct investment (FDI) during the last two decades of the twentieth century has been accompanied by an extensive literature dealing with the impact of multinational corporations (MNCs) on issues such as economic growth, poverty and inequality, the environment, and cultural diversity. As many of the concerns about these impacts, positive as well as negative, revolve around the question of markedly increased competition among states for MNCs, it is surprising how little attention has been paid to the political and economic factors that attract FDI.

Nathan Jensen has written an interesting, empirically grounded, and provocative book dealing with that topic. He focuses on political institutions, arguing that to attract MNCs, the institutions that underlie policy must provide credible commitments to favorable policies and be sufficiently flexible to adapt to changing economic conditions.

Jensen is concerned with three sets of institutions: democratic polities, veto players who have policy preferences supportive of multinationals, and the International Monetary Fund's conditionality agreements. He also looks at the impact of fiscal policy on MNCs' investment decisions, arguing that there is no evidence of a "race to the bottom." I find the first three arguments convincing (with some caveats) but the last—that dealing with the relationship between FDI inflows and fiscal policy autonomy—problematic.

One of the strengths of this book is that the theoretical arguments are subject to extensive and generally rigorous empirical tests—typically cross-sectional time-series regression analyses. That said, I would have preferred more concern about the problems of FDI inflows (FDI/GDP) as a dependent variable.

Measurement questions aside, three issues are relevant. First, net inflows are reported; one never knows the balance between new inflows of direct investment and divestment or outflows. As Jensen is concerned with the impact of politics on FDI, divestment could be nontrivial. Second, a breakdown of FDI by sector is not available; if one is concerned about political reactions to FDI, it makes a difference whether you are talking about an oil concession or a manufacturing plant. Third, there is increasing evidence that it may not be appropriate to pool advanced and developing countries in the same regression equation. (A frustration of this book is that country lists are not provided for most of the regressions.)

That said, FDI inflows are the best available measure of MNCs' activity abroad and are widely used in empirical research. Furthermore, Jensen makes a very reasonable effort to fully specify his equations controlling for all of the "usual suspects." However, I am concerned that by not taking the limitations of his dependent variable seriously enough, he may have overgeneralized the applicability of the results. Let me turn to some of his specific arguments.

The chapter dealing with democracy and FDI is the strongest in the book: Both the theoretical arguments and empirical analyses are convincing. Jensen refutes arguments that MNCs prefer autocrats and demonstrates a significant and positive relationship between democratic political institutions and FDI inflows. The results are robust to a number of specifications, including an attempt to control for selection effects resulting from the fact that democracy is less prevalent in poor countries.

Three mechanisms link democratic institutions and FDI inflows: information or transparency in economic and political affairs; representation that provides the means for foreign firms to exert influence on government policy; and "audience" or electoral costs of reneging on agreements with foreign investors. While the arguments about

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transparency and representation are reasonable and convincing, audience costs ignore the reality of the political process in many mineral-rich poor countries. Neither Hugo Chavez nor Evo Morales appears overly concerned about being thrown out of office for tightening the screws on MNC activity.

Jensen also appears on solid ground arguing that veto players whose interests correspond with those of the foreign investor are likely to increase the perception and probability of credible commitments. The argument rides on an assertion that an arrangement is most likely in a politically federal (as opposed to fiscally federal or politically decentralized) system. While I was not entirely convinced by the logic of the argument, it is certainly an interesting possibility. His empirical work supports the hypothesis.

I do have a number of concerns about Jensen's attempts to refute the "race to the bottom" argument. With some minor exceptions, he fails to find a negative impact of government consumption, social security spending, capital taxation, labor taxation, or a left-leaning government on inflows of FDI. He concludes that MNCs' investment decisions do not challenge fiscal policy autonomy, that "pessimism about the role of government fiscal policy in attracting foreign direct investment suffers from overexaggeration" (p. 54).

That conclusion rests on a very slender theoretical and empirical foundation. Jensen uses the notion that capital is footloose and mobile as a straw man, and that straw man is easy to blow over. The extensive literature on the impact of incentives on FDI decisions makes it clear that subsidies or tax rebates are rarely more than a marginal factor; my reading of studies on the impact of incentives on FDI is that they are inconclusive. Jensen does not deal sufficiently with this literature, which has to inform any discussion of the impact of fiscal policy on flows of FDI.

Perhaps more important, it is not clear that impacts on FDI inflows are a reasonable test of the race-to-the-bottom argument. The threat of a reduction in inflows or even the possibility of outflows may well be enough to constrain fiscal policy.

Furthermore, empirical tests of this proposition are limited to 15 countries of the Organization for Economic Cooperation and Development. While a country list is not available, the panel's starting point of 1963 makes it likely that the sample is restricted to a small number of relatively advanced countries. Given the complexity of this area of research, one simply cannot declare an argument false on the basis of a lack of statistical significance in a regression equation; at a minimum, one would want a positive and significant result for its inverse. Any conclusion here has to be limited to the old Scottish verdict of "not proven."

My quibbles aside, this is an interesting and provocative book. It is a serious effort to think about and test the impact of political institutions on multinational firms and flows of foreign direct investment. With the exception of the work on the race to the bottom (in many ways tangential to the book's main argument), the empirical tests are well specified and robust. This is worthwhile reading for anyone interested in the political economy of FDI.

The Impact of Norms in International Society: The Latin American Experience, 1881–2001. By Arie M. Kacowicz. South Bend, IN: University of Notre Dame Press, 2005. 232p. $40.00 cloth, $20.00 paper.

— Matthew J. Hoffmann, *University of Toronto*

With this book, Arie Kacowicz addresses the ongoing "norms debates" in the international relations literature and attempts a synthetic perspective on norms. The attempt at synthesis is an important one and well done, even if problematic at points.

The theoretical aims of the book are lofty and laudable. Kacowicz attempts to synthesize diverse perspectives on norms into a neo-Groatian approach, which, he argues, is a superior way to identify and assess the impact of norms on the behaviors of states. This approach is founded on the assumption that the norms inherent in societies of states matter for state behavior. The empirical questions that flow from this assumption and approach are the conditions for *how* and *when* these norms of international society will matter. The empirical target is explaining the puzzle of relative peace in Latin America in the last 120 years. The author argues that Latin America can be considered a distinct international society, and he uses a series of six territorial disputes and five security/peace initiatives to test his hypotheses on the impact of norms.

After lamenting "excessive epistemologism" (p. 2) in the international relations literature and advocating for a pragmatic neo-Grotian approach in the first chapter, Kacowicz turns to the development of his synthetic framework. Rather than glossing over different approaches, he provides an in-depth guide to norms perspectives and elucidates the common ground to be found. He is very explicit in his synthetic goal and Grotian approach, aiming to "take the best of all these schools" and reconciling them "through the empirical analysis of the instruments of international law" (p. 41). The definitional exercise and exploration of different perspectives lays the groundwork for his main theoretical contribution—outlining a framework for understanding the emergence, evolution, and impact of norms.

His ideas on emergence and evolution will be familiar to those ensconced in the norms literature, and so the real contribution is his discussion of norm impact. Here, he develops six paths through which the normative context influences states: constraining legitimate/possible behaviors, changing domestic discourse/behavior, acting as exogenous coordination mechanisms, shaping state interests/identity, defining the content of international law and

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institutions, and constituting the nonmaterial structures of international society. These paths are derived from different perspectives on norms, and the analysis of them signals Kacowicz's commitment to taking all perspectives on norms seriously. Because delimiting paths of influence only outlines possible ways that international norms could influence states, he takes the next step and derives hypotheses on when international norms are likely to have influence. His hypotheses rely on independent variable that are both domestic (salience of mobilizing actors, nature of the regime) and international (distribution of power, regional conflict, hegemonic presence, institutionalization of international society, fitness between normative framework and specifics of a case) in order to explain norm impact.

Kacowicz then justifies his assertion that Latin America is a distinct international society. This third chapter provides needed background for those unfamiliar with the region, as well as an opportunity to discuss the emergence and evolution of the particular "norms of peace" and "norms of security" that he sees as components of the Latin American international society. Specifically, the salient components of the normative framework for Latin American states are sovereignty, *uti possidetis,* territorial integrity, peaceful settlement of disputes, arms control, collective security, political legalism, democracy, and human rights. The two empirical chapters analyze the 11 historical incidents for norm impact. For each case, Kacowicz provides historical background, evidence for the impact of norms on international society, a discussion of the hypotheses on the conditions of impact, and potential alternative explanations for the behavior of states.

His analysis finds that the specific normative framework in Latin American international society has been relevant in keeping the peace and shaping state behaviors/relations—not surprising given that this was his stated working assumption. Exactly when the normative framework matters is a more complicated discussion, however. The author claims that norm impact has flowed through all six paths of influence to varying degrees in the cases. Further, the hypotheses on norm impact had varying levels of support—stable patterns across the cases are somewhat elusive. Nonetheless, he sees enough in the evidence to compare the Latin American case to other regions.

This is a careful, well-researched, and reasoned, serious study. However, at the risk of enacting epistemologism or perhaps perspectival chauvinism, I raise two theoretical concerns from a constructivist perspective. It is perhaps unfair to critique a synthetic enterprise from a single viewpoint, but if the essence of the perspectives being synthesized are lost, then the synthesis is problematic. The first issue is how Kacowicz ultimately provides a viewpoint on norms that makes them essentially equivalent to international legal instruments. This analytic move facilitates a clear assessment of compliance with the instruments as a (pseudopositivist) test for the impact of international norms. Yet in so doing, he freezes norms. International law contains within it many (if not most) of the characteristics of norms common to multiple perspectives, but the static conception developed here disconnects norms from actors in significant ways that constructivists would reject. While he is careful to note that it is problematic to treat norms and the paths through which they influence behavior as independent variables (p. 168), the hypothesis testing does this nonetheless. The norms being tested for impact are preconstituted, preexisting, and available for states to use, rather than being embedded in mutually constitutive social processes—a violation of the essence of the constructivist perspective.

Further and related, his understanding of norms as both inherent in international society *and* as independent variables that may or may not influence states in international society is problematic from a constructivist viewpoint. To claim that an international society with embedded norms exists is to presuppose the influence of those norms. If the norms did not have influence (i.e., were not expressed in the behavior that constitutes and reifies those norms), then they would not exist, and ergo neither would the international society. Constructivists will find it problematic that the "the degree of fitness between the *preexisting* international normative environment and the relative normative case" (p. 39, my emphasis) could possibly be an *independent* variable. Thus, while I applaud the attempt at synthesis, the enterprise cannot be considered a full success when it downplays a central aspect of the constructivist perspective on norms—their social quality (that they only exist when actors continually reproduce them through their thoughts, words, and actions).

These issues notwithstanding, I highly recommend *The Impact of Norms in International Society.* It appeals to those interested in both Latin America and the more general norms debates, and it combines theoretical innovation, methodological rigor, and empirical importance—a rare combination. The attempt at synthesis is likely to generate perspective-specific critiques like the one discussed here, yet the work is useful and significant in advancing the debate and our understanding of norms.

Trust and Mistrust in International Relations. By Andrew H. Kydd. Princeton: Princeton University Press, 2005. 284p. $39.50.

— Branislav L. Slantchev, *University of California-San Diego*

That cooperation is at best difficult and at worst impossible in the anarchic international system is an established idea. Analyses usually assume that state preferences put them in a prisoner's dilemma where defection is the strictly dominant strategy. Explaining cooperation means showing why actors forgo the short-term benefits of exploiting others. Mechanisms involve conditional play, institutional arrangements, and hegemonic leadership, which help

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overcome coordination, monitoring, enforcement, and free-rider problems.

Andrew Kydd argues that trust can play no role in such a world because cooperation is based on fear of being punished, rather than on a belief that it would not be exploited. Kydd defines trust as one's subjective belief that the other actor prefers to reciprocate cooperation rather than exploit it. While mistrust figures prominently in realist theories (e.g., causing escalatory spirals under the security dilemma), they have been vague about its origins, impact, and the ways actors can overcome it. To analyze these issues, the author uses a series of game-theoretic models in which actors are uncertain whether the other player is security seeking or expansionist. Trust is the minimum belief that the other is security seeking to the extent necessary for a security seeker to cooperate. Cooperation is possible when the two actors sufficiently trust each other.

The bulk of the book comprises three parts, each consisting of a theory chapter, which develops the formal argument, and an empirical chapter, which examines the historical record in light of theoretical implications. The empirical chapters are not meant as tests of the theory. Because any pattern of behavior can occur in some equilibrium, any observable event can be rationalized with a set of beliefs and preferences. Since many equilibria are not mutually exclusive, the fact that an equilibrium pattern fits the historical record provides only tenuous support for the theory. Instead, Kydd uses the theory as a guide to the type of evidence one would be most likely to find if the model correctly captured a significant part of real world interaction.

Part II shows that in a model where actors are uncertain about each other's beliefs, rational play can result in both tragic and nontragic spirals, but the latter are far more likely. The implication helps explain the origins of the Cold War. Whereas revisionists insist that the Cold War was essentially a tragic spiral between two security-seeking rivals, Kydd argues that the nontragic traditionalist account, which pits a security-minded and initially trusting United States against a potentially expansionist and suspicious Soviet Union, was fundamentally correct.

Part III extends the trust argument to multilateral settings. The model shows that hegemony can lead to cooperation, but only if the leading state is sufficiently trustworthy. Kydd then examines the interaction between Western European states and the United States in the first decade of the Cold War on key questions of defense, and German partitioning and rearmament. He argues that to understand the period, one must account for the steps the Americans took to reassure their European allies. Although a bit vague at times about what precisely the Western Europeans were fearful of (American abandonment? German revanchism? Soviet expansionism? all of the above?), Kydd effectively counters both alternatives of benign hegemony (the United States provided public goods unilaterally) and coercive hegemony (the United States forced contributions).

Part IV explains how players can overcome mistrust by making costly gestures to signal their preferences and shows how the Soviets were able to reassure the Americans that the USSR was no longer expansionist in the late 1980s. The Reassurance Game shows that a security seeker can take risks by cooperating while being unsure about the other actor. Since this behavior separates him or her from an expansionist, it builds trust that leads to more cooperation, provided the other side is similarly motivated. Mikhail Gorbachev's strategy was to act in a way that an expansionist leader would not have. This is why the series of dramatic, often unilateral, steps he took to disengage from Europe eventually persuaded even the most suspicious observers not to fear the Soviet Union. As in any game-theoretic account, the book is agnostic on the sources of preference formation and hence cannot speak to why this change occurred.

Three modeling choices suggest avenues for future work. First, all models restrict players to two actions: cooperation or defection. If an action can reveal something about preferences, information transmission can happen only when actors want it to and are able to play separating strategies that other types would be unwilling or unable to mimic. Within the limited action space, players cannot tailor behavior to signal preferences or elicit from others actions that would reveal theirs. Second, since trust is about beliefs, revising these beliefs is crucially important. Except for the Reassurance Game, learning is strategically irrelevant because (a) the games end before actors can act on their updated beliefs, or (b) new information comes from nonstrategic reports rather than the opponent's behavior. We cannot use such models to study bluffing, probing, and other forms of strategic misrepresentation that seem relevant. Third, even though most of the analysis is couched in dynamic terms, only the Reassurance Game embeds a meaningful time dimension in that players can update their beliefs and then act on the new information. (It is not even clear how one can think of spirals in a static setting.) One is left wondering why this game was not used as the basis for the entire book.

Trust and Mistrust in International Relations is an important book that does what a good theory book should do: offer a novel way of looking at a phenomenon and provide a solid basis for future research. It is an admirable piece of work because it develops the main ideas with logical rigor without sacrificing readability, a rare thing indeed in the often highly technical world of formal modeling. But perhaps most impressively, Kydd utilizes the models to illuminate crucial periods of recent history in a way that allows one to weigh, and dismiss, well-known alternatives.

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Electing to Fight: Why Emerging Democracies Go to War. By Edward D. Mansfield and Jack Snyder. Cambridge, MA: MIT Press, 2005. 300p. $32.95.

— Andrew J. Enterline, *University of North Texas*

Edward Mansfield and Jack Snyder's article "Democratization and War" (*Foreign Affairs* 74 [May/June 1995]: 79–97) arrived in 1995 like a thunderclap, riveting me to questions about domestic political changes and their implications for peace and conflict between states. The article appeared when the intellectual wars over the democratic peace were hot, and when research critical of democratic performance in general was receiving heightened scrutiny. In general, the early response to the authors' claim that democratization significantly increases the war-proneness of states focused narrowly on their research design. However, my review of Mansfield and Snyder's book-length treatment of the subject focuses on the broader weaknesses in the book, of which there are few, and the book's strengths, of which there are several.

In *Electing to Fight,* the central puzzle is as follows: "[W]hy do citizens in well-established democratic systems vote for governments that rarely wage war upon each other, while electorates in transitional democracies so often support aggressively nationalist policies, even against democracies?" (p. 23). The answer, the authors argue, is that while democratic voters in newly democratizing states do not start out preferring aggressive foreign policies, the incapacity of nascent democratic institutions to channel popular demands effectively makes democratic voters receptive to political distortions and, perhaps, calls to engage in war abroad.

For Mansfield and Snyder, the central link between democratization and causing war is the process of *incomplete democratization,* in which a volatile mixture of weak political institutions, protective elites, and popular involvement in the political process increase the attractiveness of aggressive foreign policies. In short, elites in the former authoritarian regime seek to slow the democratization process, a process that would ultimately weaken elite prerogatives. Appeals to nationalism centered on notions of national security enable elites to accomplish the twin goals of finding common cause with "the people" and overempowering the executive branch in an effort to restrain democratic reform.

The authors test their claim that incompletely democratizing states are more war-prone with monadic- and dyadic-level data for a global sample of states covering the modern interstate system (1816–1992), and in doing so they report substantial evidence that incompletely democratizing states are significantly more likely to engage in wars abroad. In turn, the authors "process trace" cases of war initiation by democratizing states, a method enabling them to gauge the relative importance of the causal mechanisms that their theory anticipated to be important precursors to war, as well as to explore false-positive predictions produced in their quantitative analysis. Additional qualitative analysis facilitates their exploration of six international wars that occurred between 1992 and 2000, the period absent from their large-n quantitative analysis. In general, the qualitative analysis supports the authors' theoretical claim regarding incomplete democratization.

There are few outright weaknesses in the volume, but three spring to mind. First, little attention is devoted to the origins of domestic regime change. It seems plausible that whether a political change is indigenous or imposed from abroad might have important implications for the democratization and conflict link, as well as for policies developed to minimize the strength of this link. Second, a survey of the appendix, which reports cases of complete and incomplete democratization and war (pp. 285–87), suggests some striking regional patterns. For example, not a single case of democratization, either complete or incomplete, occurs in Latin America or Africa when the authors rely on the *Polity III* combined regime index, a frequency that increases only slightly when the additional measures of regime change are calculated. A lengthier exploration of the potential theoretical implications of these patterns seems warranted. Finally, although the process-tracing methodology is instructive, it is not abundantly clear why one benefits from tracing only cases of democratization wherein war initiation occurred, rather than also examining a representative subsample of cases in which democratization occurred but war initiation is absent.

These issues aside, the volume exhibits several strengths. First, it remains provocative, even 10 years after the initial article in *Foreign Affairs.* To my mind, this provocation stimulates critical thinking about the research question, as well as many other research-related issues. Second, it is firmly grounded in the dynamics of political institutions, rather than the static approach that characterizes many studies of conflict. Emphasis on the dynamic qualities of domestic political institutions reinforces an important element of the scientific study of peace and war: Virtually every cause of war is cast in probabilistic language and estimated empirically as such. Thus, powerful states deter weaker challenges from initiating wars, but in some instances, weak states do attack stronger targets. The democratic peace is presented in similarly probabilistic terms. During regime transitions, the very institutional and normative factors that enable democratic pairs of states to avoid open warfare in resolving disputes might fail to prevent the occurrence of interdemocratic hostilities. Mansfield and Snyder's case study of the Kargil War between Pakistan and India certainly suggests as much.

Third, the volume provides an excellent illustration of the way in which scholarship can inform policy in a meaningful way and in a timely fashion. Specifically, if weak democratic institutions do increase the proclivity of new democracies to resort to war, then the current democratic

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community should seek to fortify fledgling democracies and prevent the resort to arms. Fourth, Mansfield and Snyder demonstrate that the emphasis on regime change in the comparative literature and on war and peace in the international relations literature are complementary, thereby underscoring the need to draw knowledge from across subfield boundaries to answer research questions that are of contemporary importance. Finally, their volume demonstrates the benefits of relying on research designs that combine qualitative and quantitative methods of theory exploration and hypothesis testing. Even if one does not subscribe to the process-tracing methodology employed here, the juxtaposition of large-n and small-n analysis in the same study is likely to be beneficial.

In sum, *Electing to Fight* raises a number of important issues both explicitly and implicitly that warrant further consideration. In particular, if we wish to encourage the proliferation of democracy, we should keep in mind that, much like economic development, the road to the ideal type is likely to be bumpy, even jarring, at times. Mansfield and Snyder encourage one to think about the dynamic systems that underlie this journey, and in turn, how scholars and policymakers alike might smooth this path together.

The Politics of the Global. By Himadeep Muppidi. Minneapolis: University of Minnesota Press, 2004. 130p. $60.00 cloth, $20.00 paper.

— Markus Lederer, *University of Potsdam (Germany)*

Himadeep Muppidi has written a short, sophisticated, and provocative book that pushes its readers to think hard about global politics. His main hypothesis is that a thorough analysis of the "contestations over the social meaning of the global" (p. xvii) allows us to explore new "spaces and strategies for resisting the colonialization of the global" (p. xviii). In the end, however, the reader feels a little puzzled regarding the fruits of the exercise, including whether we have really gained a better understanding of global politics or whether we have been offered new possible ways for doing politics globally.

This book is part of a growing international relations literature that has its origins in critical theory; in particular, in postcolonial studies (Edward Said and Gayatri Spivak are two of the references often quoted). This new theorizing is no longer content to challenge the epistemological foundations of classical IR thinking, but it is also much more politicized. It is therefore no surprise that Muppidi starts with a critique of traditional theorizing and how rationalist or weak constructivist thinking reproduces colonial conceptions of politics, instead of providing a new and more inclusive global focus. His "critical constructivism" (title of Chapter 2) is thus also a political statement that is supposed to offer new perspectives for how the global can be perceived in a world of multiple interpretations through the use of the concept of "social claim." According to the author, a social claim is always articulated against somebody and thus takes into account the intersubjective construction of the world. Taken further, social claims constitute actors but are also shaped by them and thus can tell us something about the production of a global order. In his own words: "The production of the global is a systemic phenomenon that necessarily has a mutually constitutive relationship with the situated practices of societal actors. . . . Any empirical analysis of globalization must therefore examine the embedded practices of specifically situated social actors to see how their actions are either productive or transformative of this systemic phenomenon" (p. 28).

Muppidi provides two short case studies—one of India and one of the United States—in which he shows how social claims toward the global are articulated and how they generally reproduce a colonial practice. He does this either by situating India within the Western liberal economic system or by taking the United States as an implicit role model that should be copied. The "productions of the global" (title of Chapter 5) are further analyzed by showing the interconnectedness between the U.S. political and the Indian political space and how social claims in one can have major repercussions in the other. Such "global conversations" (p. 79) are illustrated using the American Competitiveness Bill, as well as the discussion surrounding the Comprehensive Test Ban Treaty.

The author concludes by discussing possible means of "resistance and rearticulation" (title of Chapter 6). This discussion constitutes the book's most interesting part, as two alternatives of how one can approach global politics critically are closely scrutinized. The first is a series of ads in the *New York Times* that were published by various Western nongovernmental organizations, which are supposed to demonstrate that differences in the world are being lost, in short that McDonaldization is taking place everywhere. Muppidi convincingly criticizes such a view as not allowing a real alternative to the neoliberal mainstream but as assuming a priori that everybody else simply wants to stay different and that we in the West have a right to such a difference. He sharply shows us that global politics should not hypocritically perceive that the colonized simply like to keep up their traditions and that we think we know that this is best for them. In short, the voices of the colonized have to be taken seriously, even if their utterances are unpleasant for us.

The second political statement Muppidi discusses and contrasts with the ads of the NGOs in the *New York Times* is the articulation of the Indian intellectual Arundhati Roy, who proclaims herself to be in exile after the Indian government tested its nuclear bomb capacity. Muppidi has lots of sympathy for Roy's proclamation because he sees this as a step whereby a broader, nonnational political community is taken into account, and thus the voices of the colonized are also heard and understood. In short, such political acts lead "to other valuable forms of

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political association, possibly to an alternative understanding of the global" (p. 101).

Overall, the book has one significant strength and two major weaknesses. Its major strength is Muppidi's unorthodox reading of the global and the challenge he puts not only to mainstream rationalist thinking about IR but also to many constructivist understandings of the global. Scholarship like Muppidi's can thus make an impact, as we are pushed to think much harder about the political meaning of the "global" than if we only stay in our mainstream camps. From a reading of this book, it is obvious that it does not suffice to identify ideas as another important independent variable. The author's "bringing in" of the colonial and the postcolonial identities shows that political actors have to be contextualized within broad political structures in order to understand their actions. It is therefore not enough to discuss liberal economic policy as an idea, or even as an ideology, in order to understand the revolutionary impact liberalism has in India. However, a history of the colonial past of India—that portrays it as striving for Western modernity while trying to stay independent at the same time—allows us much better to see the revolutionary potential once the colonized is part of the global (that lies in the once-colonized becoming part of the global).

Unfortunately, there are also two weaknesses. The first is conceptual: Muppidi aims to enlighten us about the politics of the global, but he primarily speaks about the repercussions of social claims made in India and the United States. The "Other" is thus almost always perceived as a nonnational, and only at the end of Chapter 3 (p. 56) is the role of expatriate Indians and of the diaspora as postcolonial agents acknowledged. Here, Muppidi could have made an even stronger case and argued for it theoretically as, especially in the U.S.-Indian case, new transnational and maybe even transcolonial spaces and identities develop. The second weakness concerns coherence; the book leaves the reader unsatisfied overall because the final conclusion—building up a new and nonnational identity against the establishment—is neither worked out enough nor anticipated in the preceding pages. We can easily share his assessment that Arundhati Roy's imagined exile "transcends the limits of the national" (p. 100) and successfully strives for a new form of political association. However, the four chapters before did not prepare the ground for judging whether Roy's action is a generally recommendable one. In other words, should we all go into exile—even so it is only imagined—whenever the government does something to which we object? This can hardly be the *Politics of the Global* we all strive for.

In conclusion, there are thus many well-written and interesting sections that give readers pause for thought, but they do not speak to one another enough. In the end, we get wonderful pictures of the trees; we, however, are left alone to find the theory or the practice of the global forest by ourselves.

Renegade Regimes: Confronting Deviant Behavior in World Politics. By Miroslav Nincic. New York: Columbia University Press, 2005. 232p. $29.50.

— Richard Ned Lebow, *Dartmouth College*

The term *rogue states* gained currency in the 1990s, and was applied by policymakers, pundits, and a few scholars to regimes worthy of moral condemnation and political-military coercion. The Clinton administration introduced the less offensive appellation "states of concern," but the current Bush administration, not one to shy away from inflammatory rhetoric, brought "rogue states" back into use and made them a more central focus of its foreign policy. Iraq, Iran, North Korea, and Cuba head the administration's list, and the charge also has been leveled against Syria, Yugoslavia, and Libya (before its recent about-face).

"Rogue state" is an effective rhetorical tool that has been used by politicians, journalists, and special interest groups to brand and isolate their least favored regimes. Miroslav Nincic prefers the term "renegade regime" (RR), which he develops as an analytical category and uses to evaluate various forms of coercive strategies in international relations. He defines RRs as governments that seek to acquire weapons of mass destruction (WMDs), carry out acts of terrorism or support groups who do, conduct aggression against other states, and repress their citizens in ways that have international consequences. None of these regimes are democratic. He contends that leaders of RRs evaluate the consequences of domestic and foreign policies in terms of their implications for their hold on power. Leaders of RRs may expect to enhance their political security from confrontational policies with the outside world if they satisfy key internal constituencies on which they depend for support or stimulate a "rally round the flag" response by their publics.

Nincic identifies nine RRs (Iraq, Iran, Libya, Syria, North Korea, Pakistan, Yugoslavia, Afghanistan, and Sudan) on the basis of his defining behaviors, dates their emergence as RRs, and charts their evolving status. Using the concept of deviance from sociology, he theorizes that true RRs pass through two stages: initial flirtation with deviance and adoption of a deviant lifestyle. In the latter stage, they are more difficult to influence by coercive measures because such measures may unwittingly improve their domestic standing, as external threats did against Slobodan Milosevic. They can also reconfigure leaders' incentives in perverse ways by making compliance appear more costly than resistance. Although he does not use these examples, Franklin Roosevelt's oil embargo against Japan and George H. W. Bush's threats against Saddam Hussein in the aftermath of his invasion of Kuwait are well-documented cases in point.

The two most interesting chapters explore the dynamics of sanctions and military measures against RRs. Nincic

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evaluates the results of comprehensive (Iran, Iraq, Libya, Afghanistan, Yugoslavia, and North Korea) and partial (Pakistan, Sudan, Syria) sanctions, finding that they tend to reinforce rather than alter existing policies. They can strengthen the power of rent-seeking elites and reward recalcitrance by appearing to justify the "us against the world" rhetoric of leaders. Threats of regime change often fail for the same reasons, and because they are not perceived as credible even when they should be. Nincic documents his claims with mini case studies, and a particularly good one of NATO, the United States, and Milosevic's Yugoslavia. He also has a few thoughtful words to say about the difficulties of reconstructing countries after interventions to remove rogue leaders.

All definitions are approximations, and Nincic's is no exception. It relies on contributing behaviors (search for WMDs, terrorism, aggression, repression with external consequences), all of which are subjective, political, and not always self-evident, as we recently discovered in the case of Iraq and WMDs. By Nincic's definition, the United States is arguably a rogue regime. It has the world's largest nuclear arsenal, which it continues to modernize; has supported, and even conducted, terrorist activities in Latin America; has invaded two countries in the last several years; and has engaged in repression (i.e., Guantanamo) that has had important external ramifications. Much of the world considers George W. Bush's United States an RR or its equivalent, and opinion polls indicate that publics of even close allies consider it the greatest threat to world peace. It would have been interesting for Nincic to have analyzed the United States as an RR, and offer an assessment of whether it was merely experimenting with a deviant lifestyle or is a thoroughly committed RR, and what the consequences of this might be for its future behavior and efforts by others to restrain it.

To the extent that this work has a failing, it is inattention to the existing literature on economic sanctions and coercion (e.g., excellent studies by David Rowe and Daniel Drezner). Some of this literature builds on the same cases, and has interesting and important implications for Nincic's argument. Much of it addresses other cases, and not just RRs, and would allow some assessment of whether the negative aspects of sanctions are unique to RRs or a more general phenomenon, as I suspect they are.

Globalizing Critical Theory. Edited by Max Pensky. Lanham, MD: Rowman & Littlefield Publishers, 2005. 264p. $82.50 cloth, $29.95 paper.

— Fred Dallmayr, *University of Notre Dame*

This is a welcome and innovative book, at least in its basic intent. The innovation resides mainly in the correlation between a prominent strand in Western social and political thought—the Frankfurt School program of "critical theory"—and a major phenomenon of contemporary political life: the process of globalization. In many ways, this correlation goes against the dominant academic grain. At least as cultivated in American academia, political theory tends to be mainly retrospective and concerned with the rehearsal of time-honored texts, often with a pronounced slant against contaminating theorizing with mundane affairs. On the other hand, examination of current developments—including the process of globalization—tends to be left to journalists or else to specialists in international politics with little or no background in traditions of political thought. In this respect, the present volume is a breakthrough. It deliberately seeks to bring the insights of the Frankfurt School program to bear on ongoing discussions about global politics, economics, science, and aesthetics. All the contributors, including the editor, are distinguished scholars with solid reputations grounded in the traditions of critical theory and adjacent theoretical paradigms.

In his Introduction, Max Pensky ably pinpoints the challenges involved in the attempt at "globalizing theory and theorizing globalization." As he observes (p. 1), available literature demonstrates the extent to which work on globalization has "for the most part further entrenched rather than unified academic distinctions." As a result, theories of globalization tend to be "multiple and discordant, reflecting not only the mutual indifference of various disciplinary projects . . . but often incommensurate theoretical axioms and methodological assumptions." Looking at it from a different angle, Pensky discerns in the prevailing situation not only a problem but also an opportunity: the chance of preserving theoretical diversity against globalizing uniformity. In his words (p. 2), the "messy pluralism of current theories of globalization" can also be seen as "a very desirable development: the abandonment of the last remnant of an outdated ideology of absolute theoretical mastery, the idea that a single, unified theory is intrinsically preferable to a lot of disunited ones." Actually, in Derridean terms (not necessarily congruent with Frankfurt School premises), the very notion of globalizing theory exhibits a paradox: because of the ultimate "impossibility of anything fixed, total, comprehensible, or global." In putting together his volume, Pensky has tried to tackle this paradox by asking scholars steeped in the Frankfurt legacy to show (p. 4) "how globalization theory is critical theory and, conversely, how critical theory is a theory of globalization."

To achieve his goal for the volume, Pensky lists three requirements or desiderata: thoroughgoing "self-reflexivity," resolute "interdisciplinarity," and adoption of a "weak foundationalism" (p. 4). In terms of the Introduction, self-reflexivity is basically a synonym for "critical" and "dialectical" methodology. A self-reflexive approach, he notes (pp. 4–5), has to "include the social, political, economic, and cultural conditions and contexts for its production within the purview of things that theory needs

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to explain." Interdisciplinarity harkens back to the model of the early Frankfurt Institute where each participant represented a "different but not incommensurable set of disciplinary postulates and methods," while simultaneously being committed to a shared research project (p. 6). The notion of a "weak foundationalism" derives largely from the work of Jürgen Habermas and John Rawls, which suggests (p. 9) that "the weakest possible rational foundationalism is also the best and that such a weak version will consist of a set of justificatory performances rather than a precise taxonomy of moral truths." For Pensky, echoing again Habermasian preferences, the "best form" of weak foundationalism would be one (p. 10) that "somehow combined a thin conception of rationality and a substantive philosophical anthropology regarding how humans respond to sudden and epochal cultural transformations . . . and how they accommodate difference and newness in the form of cultural hybridization."

The volume opens with two prominent statements defending globalization, normatively construed, against hegemonic unilateralism: first, Habermas's condemnation of the invasion of Iraq, and next, the plea issued by both Habermas and Jacques Derrida on behalf of a common European foreign policy. Congruent with one of Habermas's central concerns, the second part of the volume addresses the contemporary need for a "global public sphere." Critiquing the nationalist bias in traditional conceptions of the public sphere, Nancy Fraser seeks to open these construals in the direction of "transnationality," while James Bohman—concerned about the democratic deficit in many transnational institutions—aims to increase the "democratic influence over globalization" (p. 50) through the cultivation of a "multiperspectival" public sphere. Relying largely on Kantian teachings, Maria Pia Lara proposes to move global interactions in the direction of a "globalizing justice" and "cosmopolitan solidarity" (p. 72), while Peter Uwe Hohendahl explores the model of a "transnational university" by asking (p. 89) what happens to such a model "when the public sphere has openly transcended national borders and reflects global concerns?"

The third part of the volume deals with issues of memory and forgetting. Lifting up the "Eurocentric" background, Clay Steinman charges the Frankfurt School with (limited) complicity in "whiteness theory" and the European legacies of slavery and colonialism. Turning to the history of the United States, Thomas McCarthy finds that the country has not yet faced up to its past and adequately developed a "politics of the memory of slavery" (p. 138). Complicating matters somewhat, Andreas Huyssen counsels that memory politics has to be seconded in some contexts by a politics of "forgetting."

The concluding part assembles various "globalizing visions" or trajectories. Focusing on the theory of science, Eduardo Mendieta (p. 189) advocates the transformation of critical theory into "a geopolitical critique of the political economy of knowledge(s)"; in turn, F. Scott Scribner promotes the enrichment of critical theory through the resources of television and global media technology. With an animus against Theodor Adorno, Carsten Strathausen proposes to replace traditional aesthetic theory with "media aesthetics" (p. 231), while Silvia L. López ponders the positive repercussions of Adorno's aesthetics in Brazil.

The volume is clearly impressive and I heartily applaud its intent. My reservations concern mainly two kinds of deficit: first, a deficit of theoretical openness, and secondly, a shortfall of democratic globalism. As indicated, Pensky defends a "messy theoretical pluralism" against the "outdated ideology of absolute theoretical mastery"; likewise, Bohman endorses the notion of a "multiperspectival public sphere." Yet the dominant perspective in the volume is Habermasian or Frankfurtian in a narrow sense, with little hospitality shown to other perspectives. What Pensky has to say (p. 8) about "Heidegger's fundamental ontology," "Foucauldian social philosophy," "British cultural studies," and "Derridean deconstruction" does not convey much sensitivity for theoretical pluralism. Can the Frankfurt School still claim a monopoly or "mastery" of critical thought? More serious is the shortfall of genuine globalism. Steinman speaks of the lingering Eurocentrism of critical theory. Even though absolving the Frankfurt School of blatant bias, Pensky acknowledges (p. 11) that the School has "remained tied to a small set of cognitive commitments underlying the European experience" and hence inherited "the paradoxical legacy of a parochial theory of the West that defended its own parochialism with global claims about the agency of its object."

In light of this situation, the volume's title becomes problematical: Does "globalizing critical theory" mean globalizing the Frankfurt perspective, thus theoretically colonizing the world? With the exception of Mendieta and López, the essays are all written from the center, not the periphery. One misses critical voices from Asia, India, the Muslim world, and Africa. As Pensky admits (p. 12): "Critical globalization theory would need to learn to travel." Indeed, a lot a traveling still seems required to assist in the emergence of a viable global public sphere.

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