

Book Reviews

Richard L. Engstrom, Editor

Philip G. Roeder. *Where Nation States Come From: Institutional Change in the Age of Nationalism.* Princeton, NJ: Princeton University Press, 2007. 430 pp. (\$70.00 cloth; \$29.95 paper.)

Inspired by what has happened in Eastern Europe and the former Soviet Union, Philip Roeder asks the question, which appears as the suggestive title of this book: Where do Nation States Come From? The author continues his research on the issue of democratization and transition processes in post communist countries pursued in his previous books *Post Communism and the Theory of Democracy*, *Red Sunset: The Failure of Soviet Politics*, and others.

In our days the world numbers 192 states. Over the last two centuries, their number swelled and some estimate that there may be up to eight hundred more nation-state projects underway and seven to eight thousand potential projects. Roeder endeavors to find the reasons why some nation-state projects achieve sovereign independence while hundreds of others do not. He argues that the answers to the question of where nation-states come from can provide guidance for the design of stable political orders in culturally diverse societies, and that “the source of new nation-states has been a crisis of “stateness”—a crisis in which residents contest, even seek to create, new independent states—and that this crisis typically results from the design of their institutions” (p. 5).

In the first two paragraphs of the book the author specifies the investigation terminology, analyzes the politics of periphery and center, and clarifies the relationship between nationalism and nation-states from the view point of primordialists and constructivists. Roeder develops the concept of the segmented state and introduces a key subunit that has its own political identity, boundaries, and institutions, grievances or dreams of nationhood—“segment-state,” for the subunit, whether a colony or a federal subject. A segment-state further divides its territory and population among separate jurisdictions and gives the population that purportedly is indigenous to each jurisdiction a distinct political status. Segment-states are not simply territorial jurisdictions within a federal state; they also contain judicially separate communities of peoples who supposedly have a special claim to that jurisdiction as a homeland (p. 12). The research strategy of this book includes the segmental institutions thesis, which links institutions to bargaining within the segment-states and between segment-state and common-state leaders.

The descriptive data about the institutional variations in segmented states from the beginning of the twentieth century are sometimes tedious, but this is an analytic payoff, and represents the building blocks of the answer provided by the segmental institutions thesis to the question “where do nation-states come from?”

Part Two of the book is dedicated to the problems of forging political-identity hegemony, which consists in both the relative predominance of a national identity within “the people” and the relative empowerment of a cohort of politicians associated with that project within “the homeland.” This part introduces the foundation of a larger theory of national identity formation to set the stage for addressing the following question: Which nation-state projects and which proponents are likely to emerge hegemonic prior to independence? Here the author examines political hegemony in segment-states, arguing that segment-states create a nurturing environment for nationalist machines to appear before independence, and describes different political machines that develop in most of the segment-states of the USSR and the Russian Federation. It is also pronounced that segment-state nationalist machines play a powerful role in the question of national identity that is an alternative to an existing common-state.

The author explores variation among segment-states and elaborates on the primary constraints that increase or decrease the likelihood that political-identity hegemony will emerge within a segment-state. The traditional explanations of the rise of nationalism typically focus on cultural and economic constraints, but in the segmental institutions thesis these become important only in the context of politicians empowered by segmental institutions that use the cultural and economic opportunities created by segmental institutions to privilege their nation-state projects and their own role within these projects.

In this part of the book, the author analyzes cases from Eurasia, juxtaposed to the Russian Empire, the USSR, and the post-Soviet Russian Federation and seeks to identify variation not only among these, but within each of them. This close-up examination of cases permits to trace more carefully the processes that led from cause to effect and illustrates how political-identity hegemony is actually constructed within segment-states. As the author specifies, the method of this second part is exploratory and the purpose is to generate hypotheses to be tested through statistical models, in Part Four of the book, which employ a large number of cases from around the world (p. 85).

In Part Three the author offers an explanation for why a few nation-state projects succeed, when the most proponents of nation-state projects fail, considering the reasons why some but not other segment-states achieve sovereign independence. In this chapter the discussion shifts from the

horizontal bargaining on the periphery to the vertical or center-periphery bargaining between the proponents of nation-state projects and the leaders of existing sovereign states. The chapter summarizes the major claims of the segmental institutions thesis concerning the consequences of segmental institutions for the vertical bargaining between leaders of a central government and various claimants with nation-state projects. The chapter analyses the Perestroika nation-state crisis of 1988-1991, the so-called “parade of sovereignties” that began on June 12, 1990, with Russia’s declaration of sovereignty and the collapse of the USSR.

Segment-state leaders concluded that a common-state was not only unnecessary but possibly dangerous, and the common-state leader was unable to forestall the segment-states withdrawal. The nation-state crises in segmented states may instead end in a loss of segment-state decision rights and in centralization: the cases of nation-state crisis of the early USSR (1922) and a nation-state crisis within the Russian Federation during the transition to and in the first years of independence (1990-2004). Besides, this part examines the escalation of both claims and actions that characterizes nation-state crises and increases the likelihood of failure of an existing common-state and independence for segment-states. The discussion also explores the tendency for the agenda to focus on allocation of decision rights between common-state and segment-state governments; it focuses on the leverage that segment-state leaders have over common-state leaders as a consequence of segmental institutions and the declining leverage of the common-state leaders that can appear when the common-state government is weakened by devolution of power.

Part Four introduces hypotheses that are derived from the propositions in previous chapters, and also includes the results of statistical tests of these hypotheses that explain which of the projects succeed in becoming sovereign nation-states. The chapter begins with a systematic overview of the patterns of nation-state crises in Central Eurasia between 1987 and 1997. Yet this data inevitably raise the question if these findings “travel”—whether these patterns apply to other decades of the age of nationalism and other parts of the world. To broaden the analysis, the discussion in this chapter draws on two additional sets of comparisons. The first is a temporal comparison of these post-Soviet cases with pre-Soviet cases in the same Eurasian region. The second includes global comparison involving all major ethnic groups in the independent countries of the world throughout the later half of the 20th century. The chapter reviews nation-state projects that succeed in creating nation-state crises, and analyses not only nation-state projects that get heard, but also nation-state projects that fail to get heard—not only the “dogs that didn’t bark,” but also the “dogs that were never born” (p. 278).

In the previous chapters, in search for answers to the question of where nation-states come from, the segmental institutions thesis stresses that it is

not ethnic groups and their mobilization that become nation-states; the segment-states are more likely to secede and become new, independent nation-states: “no segment-state, no nation-state.” Out of the 177 sovereign states that joined the international community between 1901 and 2000, more than 86 percent had been segment-states immediately prior to independence. Almost 80 percent of these segment-states that gained independence was simply elevated to independence without significant reconfiguration, and the remaining 20 percent was either combined into unions or adjoined to existing independent states (p. 290).

Chapter Ten, titled “Which Segment-States Become Nation-States,” begins with hypotheses derived from the bargaining model and reexamines the Eurasian cases with a systematic comparison of the pattern of outcomes for Soviet and post-Soviet segment-states, including successful independence, de facto and thwarted secessions, and continued inclusion in a common-state. The comparative analysis, including statistical methods, between the segment-states of the late Soviet period and the segment-states of the Russian Empire seventy years earlier permits Roeder to begin narrowing the focus to those conditions that have been most important in explaining the successful elevation of segment-states to independent nation-states. Thus, other comparisons go one step further to include all segment-states which existed around the world between 1900 and 1990, and, of course, such a broad comparison justifies less careful attention to details.

The final chapter, “Nation-States and International System,” discusses the relationship of the segmental institutions thesis over the sources of change in the international system, and contends that new nation-states have been fashioned not by the invisible hand of the international system, but by a very real hand of politicians within segmented states.

This book is relevant to a remarkable range of debates in comparative politics and international relations. It is of vital importance to specialists who work on nationalism, democratization, nation and state-building in various parts of the world.

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William E. Hudson. *The Libertarian Illusion: Ideology, Public Policy, and the Assault on the Public Good.* Washington, DC: CQ Press, 2008. xii, 244 pp. (\$34.95 paper.)

Say the word “libertarian” to many people and they will think of the Libertarian Party. Though this minor party has elected a few local politicians

in the western states, it has garnered a reputation for eccentric candidates and odd behavior at its nominating conventions. As a small fringe party, it has had little influence on national politics. However, if one moves away from this party and considers the political philosophy of libertarianism, it is clear that this ideological stance has come to have a substantial political impact on this country over the last several decades.

At its core, libertarianism seeks to maximize individual freedom. It favors market-based solutions to most of society's problems and it envisions a very minimal government whose sole task is to protect individual rights to life, liberty, and property. Hard-core libertarians believe that people should be free to take drugs and visit prostitutes, and that even fire protection and education should be privatized.

Though the Republican Party continues to publicly embrace "conservatism" as its guiding philosophy, libertarianism has played an increasingly larger role in defining its approach to many policy issues. Deregulation, slashing taxes, and privatizing Social Security are all ideas that have been pushed hardest by the libertarian wing of the party. During the Republican reign in Washington, the libertarian Cato Institute became one of the most listened to think tanks on a wide variety of issues.

And while few Republicans officials have openly identified themselves as libertarians, many are closet exponents of this minimal-state approach. Many of the appointments of the Bush administration to the federal bureaucracy and the judiciary had libertarian leanings and this has clearly had an effect on public policy during the last eight years.

Given this growing influence, it is somewhat surprising that there have been few books that have taken a critical look at libertarianism. Nearly all of the books published about this political philosophy in the last few decades, such as Charles Murray's *What It Means to Be a Libertarian*, have been written by proponents. There have been some spirited defenses of liberalism—such as Paul Krugman's *The Conscience of a Liberal*—but few of these analyses have taken on the theories and values underlying libertarianism in a sustained way. For this reason, William Hudson's *The Libertarian Illusion* is a particularly valuable contribution to the literature on this topic.

As the title implies, Hudson is a critic of libertarianism. The primary way that Hudson develops his critique is by comparing libertarianism to what he sees as its polar opposite: communitarianism. Communitarianism contrasts with libertarianism in several key ways. First, instead of seeing people as isolated autonomous individuals, communitarianism sees them as "socially embedded persons" who are products of their communities and who have obligations to those communities. In addition, this philosophy views government as a positive institution, a way for people to come together to pursue "collective moral outcomes" and the common good.

Moreover, when markets produce undesirable outcomes—such as pollution, unsafe workplaces, or excessive economic inequality—the public is then seen as justified in regulating those markets, even if this involves imposing limitations on property rights.

Hudson spends the first chapter summarizing these basic differences between libertarianism and communitarianism. The rest of the book is devoted to exploring those differences as they apply to specific policy issues, including taxation, health care, deregulation, and Social Security. The issue of health care, for instance, is an ideal avenue for a discussion of the limits of market-based policy approaches. Libertarians assume that the main problem is that people use too much medical care because they often don't have to pay for it themselves. Their solution is a move to a more "consumer driven" health care system where people rely primarily on their own health savings accounts. Hudson argues that this approach would do little to reduce costs and would only exacerbate the class differences in access to medical care. He advocates a more collectivist approach: a universal, single-payer plan based on the Medicare model. This approach would not increase the costs of medical care—only how it is paid for—and would ensure equal access for all.

Hudson's choice to focus primarily on policy issues is an interesting one. Some readers might have preferred a more consistently philosophical critique, where policy decisions were used only as brief illustrations of larger ideological positions. But the focus on policy does have some advantages. Primarily, this makes the book more useful and appealing to students and the general public. Most people have a more avid interest in contemporary political issues than they do in prolonged theoretical analyses of ideological distinctions. This policy focus, along with the Hudson's accessible style of writing, make this book particularly appropriate for undergraduate courses.

Another strength of the book is that while Hudson does not pretend to be neutral—he definitely believes that communitarianism is the way to go—he tries to be scrupulously fair. He is careful not to caricature the positions of libertarians. And he goes out of his way to agree with them that liberty is a critical value in any democracy. His point is that it is a mistake to elevate liberty above all other political values and that we need to balance freedom with other political priorities, including equality, solidarity, community, and security.

Likewise, Hudson acknowledges that market economies have many significant advantages. But he argues that libertarians are prone to market-worship. They have a rigid insistence that the solution to virtually every policy problem is to cut back on government and promote market forces. In

his view, this is a disastrously oversimplified approach and one that cannot successfully cope with the complex problems we now face as a society.

The timing of this book is good. The financial system crisis that we are currently suffering through has made more Americans realize that markets are not perfect and that collectivist policies involving government regulation and market intervention are often necessary to promote the public good. For this reason, Hudson's book may find a much more sympathetic audience than it might have five or ten years ago.

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Michael J. Gerhardt. *The Power of Precedent.* New York: Oxford University Press, 2008. 352 pp. (\$45.00 cloth.)

In *The Power of Precedent*, Michael Gerhardt explores the role precedent plays in constitutional decision making. He argues that precedents exert more force than commonly acknowledged, mainly due to the "golden rule of precedent." According to Gerhardt, "justices must be prepared to treat others' precedents as they would like their own to be treated or risk their preferred precedents with the same kind of disdain they show others'" (pp. 3-4). Gerhardt defines precedent quite broadly as "any past constitutional opinions, decisions, or events which the Supreme Court or nonjudicial authorities invest with normative authority" (p. 3). Although Gerhardt approaches his analysis of precedent as a legal scholar, his goal is to synthesize social science research with conventional legal analysis. The book is a valuable contribution to the debate regarding the role of precedent.

In Chapter 1, Gerhardt discusses the patterns of Supreme Court precedent, examining the number and rate of overrulings, pointing out that the Court has only reversed itself four times with no changes in composition. He also examines individual justices' attitudes about precedent by providing many examples of the reasons given by justices for respecting or overruling precedent, an unexplored area which may prove to be of interest to political scientists.

In Chapter 2, Gerhardt discusses the most prominent theories of precedent proposed by legal scholars and social scientists. Specifically, he compares and contrasts the weak view of precedent and the strong view of precedent. According to the weak view of precedent, the Court owes little or no deference to precedents. The strong view of precedent, on the other hand, perceives precedents as the principal standard in constitutional law, constraining justices' decision making. Gerhardt argues that there are problems

with the empirical analyses of attitudinalists and rational choice theorists, most importantly that precedent is not the only source the Court has in order to decide a case: “the justices are usually required to coordinate sources in deciding cases” (p. 73). In making judgments about these different sources, Gerhardt admits that justices’ ideological preferences may come into play, but he argues that these preferences are not the same as partisan policy preferences, in fact, they can be principled approaches to deciding cases. “They can be dismissed as unprincipled only if they fail to be grounded in coherent constructions of legal materials and to comport with normatively superior principles” (pp. 73-74). Unfortunately, Gerhardt does not provide how we can ascertain the difference.

Gerhardt proposes his theory of a moderate view of precedent in Chapter 3. This moderate view of precedent leads most justices to carefully pick and choose which particular precedents to challenge because of the golden rule of precedent. Thus, “while justices express respect for precedent in the abstract, the actual process of deciding cases has enough play in the joints to make it difficult, if not impossible, to predict which particular precedents the justices will agree to weaken, if not overrule” (p. 79). Gerhardt then proceeds to describe eight factors “that demonstrate how and why some, but not other, precedents generate path dependency” (p. 94). One interesting factor he discusses is how the Court frames its judgments. Gerhardt explains that the Court generally frames its judgments as rules or standards. Since rules constrain more than standards, the more absolute the rule handed down by the Supreme Court, the more strongly the case imposes path dependency.

In Chapter 4, Gerhardt analyzes the precedent created by nonjudicial authorities. Presidential signing statements, floor votes, and rule making in the House and Senate are a few examples. Gerhardt argues that the Supreme Court is actually not as supreme as we give it credit for since “most nonjudicial precedents endure or elude judicial review. . . .” (p. 111).

Gerhardt provides a fascinating discussion of the multiple functions of precedent in Chapter 5. Precedent should not be viewed merely as a way to constrain, but precedent facilitates constitutional dialogues, shapes national identity, and educates people about the Constitution. In addition, the Court’s precedents are important to understand because they frame its choices of which constitutional cases *not* to hear. “[R]efusals to hear cases may provide insights into justices’ priorities,” he states (p. 154). There are patterns to discover in the Court’s cert decisions, when it grants cert (sending a signal that it wishes to reconsider or clarify certain precedents) and when it denies cert.

Finally, in Chapter 6 Gerhardt discusses super precedents, “constitutional decisions in which public institutions have heavily invested, repeatedly relied, and consistently approved over significant periods of time”

(p. 178). There are three kinds of super precedents: (1) foundational institutional practices, such as *Marbury v. Madison*, establishing judicial review; (2) foundational doctrine, such as incorporating most of the Bill of Rights against the states through the Due Process clause of the 14th Amendment; and (3) foundational decisions. Gerhardt admits there is no simple test to identify super precedents, but he provides a few examples. He argues that *Miranda* is a super precedent, although it has less force because the Court has recognized so many exceptions. He argues that *Roe v. Wade* is not, because there has been persistent public opposition against it.

In sum, *The Power of Precedent* offers a wealth of valuable insights regarding the complex role precedent plays in constitutional law. The next step is to flesh out the theory and to provide rigorous empirical testing of hypotheses derived from it.

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Robert J. Spitzer. *Saving the Constitution from Lawyers: How Legal Training and Law Reviews Distort Constitutional Meaning.* New York: Cambridge University Press, 2008. 186 pp. (\$85.00 cloth; \$27.99 paper.)

In his new book, *Saving the Constitution from Lawyers*, political scientist Robert Spitzer takes lawyer-bashing to a new level: he makes not just lawyers but also legal training and law students responsible for “wayward constitutional theorizing” pervading legal scholarship and the courts (p. 4). While the book will undoubtedly be popular among academics and the reading public who agree with the author that legal scholarship has degenerated into partisan advocacy and helped to ruin our judicial system, the book is replete with the same problems that the author suggests undermines modern legal thinking and advocacy (p. 4). The book is fun to read and provocative, but it is a good idea gone awry: it lacks the rigor and dispassion that ought to be the hallmarks of good scholarship in law and other academic disciplines. The problem is not that lawyers and legal scholars should be immune to reasoned or principled attack but rather that the anecdotal, caricature of lawyering and judging that the book targets is not a reasoned, principled critique.

Spitzer’s book consists of five chapters bounded by a brief introduction and conclusion. The first two chapters describe the damage that Spitzer believes law reviews have produced in constitutional law. In the first chapter, the author uses a variety of examples to illustrate the basic “argument of [his] book: American legal values and principles function effectively and

properly when lawyers practice law—when they are applying their training and trade within the bounds of the civil and criminal justice systems or in similar, traditional legal activities. But when individuals with those principles and training engage in academic scholarly analysis regarding the Constitution, the results may well be inimical to an accurate understanding of constitutional meaning” (p. 13). Drawing on social science research, Spitzer argues that legal scholarship has no standards and thus fails far too often to emulate scientific inquiry that requires the empirical testing of hypotheses and dispassionate assessment of competing theories. The second chapter is a critique of law reviews. Spitzer faults most legal scholarship for being published in journals that are edited not by peers but rather law students who are not experts in the subjects about which they are approving and editing articles. He notes that “disciplines in which peer review is the norm are not insulated against defective or wrong-headed writing, but the peer review firewall is at least a functioning barrier; no comparable barrier exists in the world of law reviews” (p. 55).

Each of the next three chapters examines “cases of . . . constitutional theories that, by virtue of their law journal provenance, acquired a degree of legitimacy and respect as “scholarly” constitutional doctrine that is . . . unwarranted” (p. 7). The subjects of these chapters are the line-item veto, the unilateral theory of the executive (the notion that all executive power should be under the control of the President), and the Second Amendment. In the chapter on the latter, Spitzer, a recognized expert in the field, derides the D.C. Circuit opinion in *Parker v. District of Columbia* for relying in part on poorly reasoned law review articles to uphold an individual right to own firearms. Although the decision was affirmed by the Supreme Court after the book’s publication in *Heller v. District of Columbia*, the majority and dissenting opinions in *Heller* relied in part on law reviews, a fact that presumably reinforces the thesis of Spitzer’s book.

Spitzer concludes the book with two reform proposals. He suggests that law schools should train law students in the methodologies of other academic disciplines and urges “turn[ing] editorial and review control over to competent faculty and impose peer review” (p. 180).

The central problem with the book is, however, the author’s failure to follow the rules of inference that he urges legal scholars to follow. For example, Spitzer challenges law reviews for not adhering to the standards of scientific inquiry, but he neglects to compare the relative qualities of articles appearing in student-edited and peer reviews. While he acknowledges law reviews publish “excellent” articles (and does not hesitate to cite to them when he likes what they say), he has no empirical data on the relative numbers or percentages of “excellent” articles as opposed to bad ones in law reviews.

Moreover, Spitzer is prone—like the professors whom he derides—to overstate the significance of his constitutional arguments. In his case studies, he treats his legal arguments as if they were knock-out punches, but they are not. He discounts the fact that his case studies are examples of hard cases in constitutional law—those in which no single source, or set of sources, points to an obvious answer. In hard cases, there is no knock-out punch; the challenge is to demonstrate why one argument is superior to another and to assess the relative strengths and weaknesses of various constitutional arguments.

Perhaps most importantly, Spitzer fails to establish a connection between the absence of standards for legal scholarship and deficient constitutional theorizing running amuck in our courts and classrooms. It's not just that he fails to establish that "lawyers are poorly equipped to engage in the scholarly world as it pertains to constitutional scholarship" or that deficient constitutional reasoning is a rampant problem in the legal system (p. 31). He fails as well as to show that law review articles are the "but-for" cause of the "distorted" constitutional meaning he claims has become rampant on the Supreme Court and elsewhere. The fact that constitutional actors cite law review articles might reflect their influence, but hardly establishes they are primarily responsible for the poor state of constitutional decision making. Moreover, Spitzer offers no systematic analysis on whether legal scholarship follows—rather than leads—constitutional decision in our society. Indeed, he suggests that legal scholarship supporting the unilateral theory of the executive followed the Reagan administration's lead. (He also ignores the fact that the Supreme Court has rejected the theory.) Nor does he consider (much less address) the possibility that legal scholars and judges have different incentives, customs, practices, and constraints.

Nevertheless, I find myself agreeing (based on a non-scientific survey) that many law review publications are not good and not just because they are partisan, advocacy pieces. (Spitzer fails further to distinguish bad and advocacy scholarship). Spitzer deserves credit for calling attention to possible difficulties with law review publishing standards, but his book largely leaves it to others (and maybe himself) to demonstrate systematically the real nature and implications of this problem.

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Christina Wolbrecht, Karen Beckwith, and Lisa Baldez, eds. *Political Women and American Democracy*. New York: Cambridge University Press, 2008. 272 pp. (\$75.00 cloth; \$24.99 paper.)

Christina Wolbrecht, Karen Beckwith, and Lisa Baldez have compiled a volume that provides an extensive review of the past thirty years of political science in the area of politics and women in the American context. The goal of this text according to the editors is to present a “critical synthesis” of extant research, discuss its implications and suggest avenues for additional scholarship. In sum, Wolbrecht and her colleagues present a single volume that purports to identify much of what we need to know.

The chapters of this text are organized thematically (gender as a category of political analysis, representation by gender and parties, women as candidates, women as officeholders, etc.). Each author gives a thorough treatment of the relevant literature from seminal works to most recent contributions while also noting the progression of women in the real political world. The importance of this collaboration is that in addition to providing strong reviews of previous research the authors here critique the pieces, provide chronologic context and discuss the impact of the research findings on the broader political science literature. The authors then conclude their chapters (and in some of the chapters these are major sections) with recommendations for future research directions. These elements of the chapters are especially positive as they synthesize what each author thinks are the “known unknowns.” I found these sections compelling because not only did they suggest directions from the author’s points of view but they stimulated the reader to think about other potential research questions.

For example, Dolan begins her chapter on women as political candidates by noting that “Each election year brings more women candidates than the year before” but that currently, “women . . . represent between 15 and 25 percent of the offices at any level of government” (p. 110). Those who have followed the literature from the early days of eligibility pool and self selection discussions will appreciate her coverage of the recent literature on candidate emergence, political ambition, stereotypes, campaigns, and how women are presented in the media. The chapter achieves two important tasks. First, it synthesizes a body of related literatures and draws those relationships between discrete pieces of research. Second, the chapter prompts readers to wonder about the integrative research questions that these separate literatures imply; are media treatments of women candidates based on gender alone or are lesbian and transgendered candidates for office treated differently? Is sexual orientation part of the emergence/ambition/stereotype equation?

Another intriguing chapter in this collection is Lisa Baldez's effort to examine women from a comparative perspective. Baldez fires the shot that many interested in women's status in political democracies and the position of the United States in promoting democratization have pondered—the fact that the “greatest democracy in the world” ranks 68th in elected female representation; that we don't guarantee women equal rights at the constitutional level; and that we refuse to codify women's rights as human rights. This chapter discusses the literature that has placed the U.S. in cross national perspective and the findings about women in the areas of public opinion, public policy, media attention, election rules, and grassroots activism. This chapter more than the others has a highly contemporary feel as it also discusses the impact of such approaches to public administration as “gender mainstreaming” and “gender budgeting” governmental efforts to eliminate or reduce inequality (p. 177). This chapter illuminates the significant cross national similarities and differences that have yet to be fully studied and poses the theoretical question about why the American version of democracy does not support women to the same degree that other countries have been able to achieve. Following the editors framework the comparative chapter discusses the unasked questions and successfully stimulates consideration of the range of alternatives from the institutional to the behavioral. Why are Latin American presidential systems moving women to the executive level? What are the implications of economic globalization and the spread of micro-credit for women's political participation in the developing world? The comparative research questions identified here demonstrate the exciting work being done and the openness of the field to new research. The Baldez chapter segues smoothly into Beckwith's final wrap up which discusses the fundamental question of what democracy means for women in the United States and around the world. This discussion will never cease to intrigue and these authors have done an outstanding job laying out the arguments.

Clearly this is a valuable text at least in part because it addresses the needs of a diverse set of individuals working at various levels in academia. Undergraduate students will encounter a reader friendly basic introduction to the fundamentals with much of the data emphatic analysis stripped out and replaced with cogent interpretation and discussion. Graduate students will find a thematic overview of the many facets of the study of women in politics and comprehensive introductions to the primary literature. Researchers will find that it provides comprehensive overviews of what they already know, but challenges them to think about future directions. This book is clearly a useful reference tool especially for those looking to cross pollinate into the broader body of research on women as political actors. Faculty that are designing or updating syllabi for courses on women in politics will find this text useful because of its thematic organization and wide net approach to

the literature. Likewise faculty in other American government themed courses can refer to the literature reviewed here as they add sections focusing on women. These broadly comprehensive chapters on the body of political science research about women's political behavior, activism, treatment by media actors, transformational economic presence during processes of globalization and the impacts of these behaviors, institutions and movements make for a strong evaluation of the literature and the major themes in the research effort to date.

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Simon Topping. *Lincoln's Lost Legacy: The Republican Party and the African American Vote, 1928-1952.* Gainesville and other places: University Press of Florida, 2008. xi, 290 pp. (\$65.00 cloth.)

Topping's is the first book to synthesize the extensive secondary literature and some of the most important primary sources on the Republican party's attempts to win black votes from Hoover to Eisenhower. GOP efforts were inconsistent, but by no means entirely cynical or opportunistic. For every Herbert Hoover, who tried to create a white southern Republican party by building on the anti-Catholic reaction against Al Smith in 1928 with the nomination of a southern racist to the Supreme Court in 1930, there was a painfully sincere Wendell Wilkie, the Republicans' 1940 presidential candidate, who was truly committed to a racially egalitarian platform (pp. 14-19, 70, 76). Unburdened by a southern congressional contingent dogmatically committed to white supremacy, as the Democrats were, Republicans were free to back anti-lynching, anti-poll tax, and other anti-discrimination laws in much larger proportions than Democrats did (pp. 55, 96, 121, 150, 181). Until the Democrats lost so badly in the 1946 congressional elections that the Truman Administration had no choice but to try to construct a winning coalition by emphasizing rights for African-Americans and labor union members and subsidies for farmers, the national Democratic party did not offer an explicit civil rights program (pp. 118, 122, 136). The pre-1948 Democratic straitjacket left Republicans the opportunity to counter the New Deal economic appeal to African-Americans with targeted programs, familiar black Republican political leaders, and the old loyalties to the party of emancipation and Reconstruction (pp. 57, 72-74, 110-12, 206).

Relying entirely on elite qualitative sources, especially the papers of the NAACP and of Wendell Wilkie, British Lecturer in American Studies Topping provides neither theoretical nor statistical underpinnings for his work.

Despite the fact that NAACP official Henry Lee Moon published *Balance of Power: The Negro Vote* in 1944 and that innumerable formal theorists have speculated about how political parties bargain for marginal political groups since, Topping engages in no explicit theorizing, mathematical or otherwise. While one might imagine that estimates of African-American voting for the two major political parties would be the basis for assessing the efficacy of various Republican and Democratic strategies and for explaining the reasons why blacks made the choices they did, Topping is content to cite others' often poorly-grounded estimates (pp. 13-14, 45, 82, 117, 136-37, 171, 190-91) or to make vague guesses about black voting behavior without explicit justification (pp. 2, 4, 28, 30, 75, 104-05, 201, 203-05, 208).

Without a consistent set of estimates of national and regional or state black turnout and partisan choice, without much examination of how Republican policy and factionalism in other fields affected its stance on civil rights (but see pp. 147, 163), and without a properly sympathetic view of the Republican party's historic support of African-American rights (pp. 2, 34, 197), Topping can give only incomplete answers to his two principal questions: "Why did the Republican Party lose the African American vote, and how did it attempt to regain it?" (p. 8). Nor can he satisfactorily answer the deeper question that runs through the book: **Why** did the party fail to connect with black voters?

Topping's well-written and interesting narratives of Republican maneuvering in each of the presidential campaigns and often, in the consideration of anti-lynching, anti-poll tax, and Fair Employment Practices Commission bills in Congress, show a party dazed by being definitively out of power after 1932 and confused as to how to respond. As Topping puts it, "leaderless, split, and groping for issues, the party staggered from one election catastrophe to the next" (p. 29). Alf Landon, the Republican presidential candidate in 1936, and Thomas E. Dewey, the 1944 and 1948 nominee, had good records on civil rights issues during their terms as governors (pp. 41, 114-16)—certainly much better than Franklin Delano Roosevelt's as president (pp. 40, 46, 70)—but they were indecisive about stressing those issues, even though neither had realistic hopes of carrying any southern states (pp. 41, 100-03, 131-32). Wilkie, a political amateur with no pre-nomination record to build upon, was more forthright on racial issues, but blacks had less reason to trust his promises (pp. 69-71). Dwight D. Eisenhower spoke reassuringly to African-American leaders in private (p. 182), but in public seemed even more conservative on racial issues than Sen. Robert Taft, his chief opponent for the 1952 Republican nomination (pp. 169-70). Eisenhower's cracking of the Solid South further increased northern black skepticism (p. 183). Once the Eisenhower Administration took office, however, it did fashion pro-black policies, most importantly by appointing Earl Warren

and William J. Brennan Jr. to the Supreme Court, and by all accounts, it did win back a significant percentage of the black vote, in response (p. 205).

The likely increase in the black percentage for Eisenhower from 1952 to 1956 suggests that a political party had to win office *before* it could make significant inroads into the African-American vote. Prior to the New Deal, Democrats had controlled the presidency for only 16 of the 68 years since the Civil War, giving them little chance at the national level to deliver policies to attract blacks, even if they had wanted to. From 1932 to 1952, Republicans were similarly powerless to bid for the black (or any other) vote by delivering on favorable policies. Rather than *more* willing to switch sides than other voters, as the balance of power strategy suggested they should be (p. 121), blacks appear to have been *less* malleable. And African-Americans were apparently more likely to vote on the basis of economic issues, not “values” issues, than many lower-income whites are today, for according to Topping, blacks switched to the Democratic party because of New Deal economic programs and in spite of the fact that Republicans offered more liberal civil rights positions (pp. 46, 206).

Even if Republicans had controlled the presidency and congressional majorities, they would have been unable to bid for black votes by passing civil rights laws because of the necessity to muster two-thirds of the Senate to break the innumerable and inevitable southern filibusters (pp. 51-53, 108, 141, 145, 156, 192). In a sense, all of Topping’s meticulous research into and careful narratives about Republican strategies and struggles over civil rights and the black vote are irrelevant, because the filibuster reduced the outcomes of those struggles to empty words, and without tangible successes, Republicans could never win substantially more African-American votes. Race relations in America, as I have argued more extensively elsewhere, has been shaped primarily by institutions and institutional rules. Topping’s thorough study proves it again.

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Christine Mahoney. *Brussels versus the Beltway: Advocacy in the United States and the European Union.* Washington DC: Georgetown University Press, 2008. vii, 260 pp. (\$29.95 paper.)

The notion that there are “cultural” differences between the United States and Europe is so widespread that it has become almost a given in most social and political discourse that includes a comparison between the two. In particular, the naturally combative or conflict ridden character of

America is contrasted with a more consensus driven European Union (EU). These general system level stereotypes are often assumed to pertain to the underlying political practices within each political system, as well as, to the system as a whole. It is this general assumption in many analyses of lobbying and advocacy in the USA and the EU that Christine Mahoney questions in her book *Brussels versus the Beltway: Advocacy in the United States and the European Union*. Unlike much of the previous research and writing comparing advocacy in the two systems, Mahoney approaches the question with a clear view to empirical testing and quantitative as well as qualitative hypothesis testing. Her conclusions exemplify both the utility of comparing the EU and the US, as well as the need to carefully examine the institutional differences that continue to separate the two.

Because of the paucity of previous large scale empirical research comparing the two systems Mahoney is forced to collect a new and unique data set that includes interviews with 149 advocates working on 47 issues on both sides of the Atlantic. Even the method of data selection and its application are unique and set Mahoney's work apart from existing literature that tends to be case based, anecdotal, non-random and as a result largely un-generalizable. As described in Chapter 3, instead of focusing on specific policy areas or types of advocacy actors, Mahoney begins the data selection through a random selection drawn from the universe of advocates within each political system. The policy areas, procedures, and other related case characteristics researched were then a function of the current activities of the advocates that agreed to participate in the research project. As a result these too became in a sense a random selection (although no information is provided on the success rate of interview requests or possible inherent biases in the data that might result). The range of advocate types and policy topics is auspiciously broad and provides a good deal of empirical weight and generalizability to Mahoney's conclusions.

The analysis itself is centered on a general theoretical framework that assumes that there are three core aspects of an advocacy effort that will impact the type of advocacy activities pursued as well as their likely success. These are the broad institutional environment created by the political system itself, the characteristics of the issue/policy that is the subject of the advocacy (for or against) and the nature of the interest group doing the advocating. Within each of these broad variables there are a number of attributes that are incorporated into the analysis. Some of the most critical include the number, breadth, accessibility and electoral accountability of the key political actors within the political system, the saliency, breadth, and type of policy that is the subject of the advocacy action, and the type, resources and characteristics of the advocates or interest groups themselves. Although each of these variables is discussed in Chapter 2, the number of potential varia-

tions and possible combinations make this critical discussion difficult to follow at times. While the content of the discussion is quite good, its organization and presentation would be significantly helped by the use of summary tables and/or figures to assist the reader in following the discussion.

In Chapters 4 through 10 Mahoney moves the research forward by combining quantitative and qualitative analyses of the possible variations that exist in the lobbying positions (Chapter 4), argumentation strategies (Chapter 5), lobbying targets (Chapter 6) and tactics (Chapters 7 and 8), as well as strategies (chapter 9) and outcomes (Chapter 10). The general organization of each chapter is clear, beginning with a concise discussion of the topic, a qualitative summary of the variations that exist between the USA and the EU and some quantitative analysis of the similarities and differences between the two. The discussion is always centered on the three core variables that shape her theoretical framework: the institutional environment, issue characteristics and interest group or advocate characteristics. Within this general and impressively consistent framework Mahoney allows for variations in emphasis and specific topics based on the individual subject being discussed. Here again, however, there are times when the number of variables being discussed and the individual hypotheses being tested or analyzed become numerous and potentially confusing. Greater use of summary tables and/or figures would provide greater clarity, especially for readers who are less familiar with interest group or advocacy literature more generally. To her credit, Mahoney does end each chapter with a succinct and admirably clear summary of the results of the empirical analysis as well as some discussion of how these conclusions challenge or support existing assumptions about lobbying on both sides of the Atlantic.

In the end, Mahoney is able to demonstrate that superficial generalizations that conclude that cultural differences between the EU and US political cultures (consensus versus conflict) are inadequate and often incorrect in their conclusions about the nature of advocacy in the respective political systems. Indeed, by building a general theoretical framework based on specific variables related to the character of the political institutions, issues and interest groups involved she demonstrates that the differences in advocacy activities in the US and EU are not simply a function of culture. Indeed, to a great extent she demonstrates that the two are far more similar than most of the literature would suggest and that those differences that do exist are more complex than a simple cultural explanation implies. Indeed, much of the differences Mahoney does find can most effectively be explained by the differences that exist between the two political systems in terms of the electoral accountability of the political actors and the character of the legislative process. As Mahoney examines in her conclusions, these differences have perhaps surprising implications for the democratic character of policy

process of the system as a whole, but they do not suggest that the differences themselves are a function of culture—political or social.

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Clifford Winston and Ginés de Rus, eds. *Aviation Infrastructure Performance: A Study in Comparative Economy*. Washington, DC: Brookings Institution Press, 2008. vi, 237 pp. (\$59.96 cloth; \$24.95 paper.)

Since 11 September 2001 the aviation industry has received a great deal of attention from and been the source of considerable frustration for the flying public. Safety concerns have given way to anger over long lines and inconvenient security check-in procedures. Airlines have filed for bankruptcy, sought mergers, and announced reductions in scheduled flights. The rising costs of jet fuel and other operating expenses have led to the reduction of in-flight amenities and increased airfares. The plight of commercial aviation is not a happy one. Amid the turmoil, Winston and de Rus do not offer a comprehensive solution for all that ails the aviation industry; however, they do present a comprehensive look at aviation infrastructure and provide some thoughtful suggestions for its improvement.

The book is an edited collection of papers from a 2006 conference held at the Rafael Del Pino Foundation of Madrid, Spain. In their short introduction, Winston and de Rus relate the topic of aviation infrastructure to globalization and state that the purpose of the book “is to assess how different regions around the world make investments and operate airports and air traffic control systems to address congestion and delays, carrier competition, and air travel safety” (p. 2). The three sections of the book provide a global overview of differing approaches to the operation of aviation infrastructure. Section one deals with the United States and Continental Europe. Section two covers Australia, New Zealand, The United Kingdom, and Canada. The third section examines China and the developing world. The editors’ synthesis and concluding remarks are brief but very helpful to the reader.

There are approximately 49,000 airports throughout the world with nearly 40 percent of them located in the United States. Scheduled passenger service is offered at 663 U.S. airports (Winston and de Rus, p. 1). The overwhelming majority of the aviation infrastructure in the United States is provided by the public sector. Airports generally are owned and operated by local governments or public airport authorities with varying levels of support from the federal government. Air traffic control and airport security are

provided by the federal government. Morrison and Winston, in their contribution, posit that many of the aviation problems the United States currently faces are the result of an over reliance on the public sector and its inability to adopt policies and innovations that would bring about needed improvements. They believe that privatization is the only solution and argue that “privatized airports and air traffic control would have the potential to improve service to travelers and reduce the cost of carrier operations while maintaining the nation’s outstanding record of air travel safety in the face of ever greater volume of traffic” (p. 9).

The privatization of aviation infrastructure is a constant theme throughout the book. Morrison and Winston offer several examples of how privatization of the U.S. aviation infrastructure would improve upon the current system. They suggest that the federal government embark on a series of experiments in privatization. Gillen and Miemeier examine the Continental European aviation system with a special emphasis on economic regulation and the allocation of take off and landing slots at airports. Slots give airlines “the right to take off or land at an airport during a specific period of time” (p. 36). They also prefer more privatized market oriented solutions as a means for improving the allocation of scarce resources in and the overall operations of the European aviation system.

Given the disproportionate share of aviation activities that occur in the United States and Continental Europe, it is not surprising that one third of the book is concerned with these two entities. The remainder of the book is concerned with aviation policy reforms in the rest of world. The smaller scale and scope of aviation activities in areas outside the United States and Europe may make experimentation with privatization and reform easier. For example, the entire nation of Australia has only five “moderately large” airports and New Zealand has one large airport (Forsyth, p. 68). The difference in size of aviation systems among nations leads one to question whether or not reforms that work in smaller systems will work in larger systems such as those operating in the United States and Europe.

Generally, the findings from the world outside the United States and Europe suggest that experiments with privatization and regulatory reform offer some promise for improving the overall operation of aviation systems. The studies indicate widespread interest in privatization and regulatory reform as a means of improving aviation services because of the near universal acceptance of the idea that modern aviation services are necessary for economic growth and development to occur. The various studies also reveal that the nature of reform should be closely related to the desired outcome. Certain types of privatization and reform will lead to increased investment and capacity in the aviation sector. Others will help to improve efficiency but do little to encourage investment.

The book has a strong bias for more privatization and regulatory reform in the area of aviation infrastructure. The editors, however, conclude that the evidence presented from various experiments with privatization “is [not] sufficiently positive to make a strong case for more extensive and widespread privatization of aviation infrastructure” (Winston and de Rus, p. 223). Instead, they offer a more qualified endorsement of further experimentation and privatization and note the large cost involved with public operation or regulation of aviation infrastructure.

As one might expect from a publication by the Brookings Institution, the book covers an important public policy topic, is filled with first rate scholarship, and is well written. Those interested in a detailed descriptive and prescriptive work on aviation infrastructure performance will find the book valuable. For those who are drawn to the book by its subtitle, “A Study in Comparative Political Economy,” the book may be less valuable. The vocabulary of aviation infrastructure and economics may be a bit tedious for the casual reader of works on comparative political economy. Overall, the book represents a valuable contribution to the literature on an increasingly important area of public policy concern.

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Thomas L. Brunell. *Redistricting and Representation: Why Competitive Elections are Bad for America.* New York: Routledge. ix, 145 pp. (\$23.95 paper.)

In *Redistricting and Representation*, Thomas Brunell puts forth the argument that the American political system would be better off if more elections—primarily congressional elections—were uncompetitive. In brief, winning voters are happier with the government than losing voters, and happy voters have higher levels of efficacy and generally imbue the government with more legitimacy than unhappy voters. Thus, Brunell reasons, policymakers should purposefully gerrymander as many uncompetitive districts as geographically possible. On the surface, this argument has a certain intuitive appeal—who, after all, does not want to improve the government’s legitimacy? However, a closer look reveals some flaws with the argument. Specifically, uncompetitive redistricting may not produce high legitimacy despite Brunell’s claims, and even if it does, it could come at a significant cost.

The strength of this book lies in the attention it brings to an important but understudied issue: the role losing voters play in our political system.

Losing voters receive so little attention because politicians are likely more responsive to their voters' preferences than those of their opponents. However, losing voters represent a non-trivial proportion of the population, and likely play some role in keeping incumbents responsive to voter preferences. The lack of systematic analysis of losing voters means we don't know the nature or extent of this role. By turning our attention to these voters, Brunell begins to address this gap in our knowledge. In this way, *Redistricting and Representation* is related to Fenno's idea that representation occurs differently for different voters, depending on how strongly they supported the incumbent. Brunell's work also has common cause with Tracy Sulkin's recent work detailing the effect losing congressional candidates have on the legislative behavior of the winner.

These strong points notwithstanding, the book's specific argument regarding the positive effects of uncompetitive elections is significantly flawed. The first issue presents itself in Chapter 3, the empirical cornerstone of the book. Looking at individual-level data, the chapter examines the relationship between two independent variables—whether the person voted for the eventual race winner and the overall margin of victory—and various measures of voter satisfaction. Relationships are consistently significant when the satisfaction measure focuses on how the voter feels about his or her own member of Congress. However, this is unsurprising and borders on tautological. The more interesting portion of the analysis is that which focuses on broader indicators of satisfaction—approval of Congress and overall voter efficacy. The book's argument hinges on there being significant differences between winning and losing voters on these measures, but the substantive differences between the groups are small (Tables 3.3-3.5), and in multivariate analysis the relationships are not consistently significant (Table 3.7). This indicates that while uncompetitive races may improve aggregate voter satisfaction with voters' own representatives, they are much less likely to improve overall attitudes toward the government.

Thus, it is unclear what effect uncompetitive elections would have on efficacy and legitimacy. Voters are already dissatisfied with the amount of competition in elections—as Brunell ably points out—so it is likely that making elections less competitive would make voters less satisfied with politics. Brunell claims this is not so, analogizing politics to football: fans are happiest when their team wins, and wins big (p. 11). However, this analogy falls apart if it is taken a step further: if every football game was a blowout, fans would likely be turned off to the sport and there would eventually be fewer of them. Aggregate viewership, ticket sales, and revenue would almost certainly decline. Similarly, a lack of competition in elections is likely to leave American politics with fewer “fans,” and the results could be just as devastating to the “sport” of politics.

Secondly, the book preemptively addresses a number of critiques to its core argument (primarily in chapter 6), with the broad message that elections could be made uncompetitive at little cost to the political system. However, some of these arguments do not hold up to scrutiny. First, Chapter 6 argues that increasing the number of uncompetitive elections would not impact members' legislative behavior. Figure 6.5 plots DW-NOMINATE scores against margin of victory; visual inspection reveals a significant party effect but little if any electoral effect. However, this bivariate analysis belies a large literature on this very relationship which brings multivariate analysis to bear and which the chapter largely ignores. The most recent and sophisticated work on this topic indicates that vulnerable members of Congress do indeed vote differently on the floor than safe members.

A second preemptive argument is that elections are decided randomly and inherently lack meaning, so they can be removed from the political landscape without significantly changing voters' signals to politicians. While it is true that there is a stochastic element in the outcome of many close races, chance is far from the only deciding factor. Short-term political forces play a very important role in the outcome of many races, including (and perhaps especially) close ones. Moreover, the aggregation of these forces produces national partisan tides which play many important roles in our political system, including enabling collective rewards and punishments of political parties. Creating a host of uncompetitive districts denies the public their ability to express a national will through the aggregation of many close election contests. If, in 1992, all or most House districts were drawn to produce a safe incumbent, the public's desire to turn Congress rightward two years later would have been thwarted. The same is true with the 2002 redistricting and Democrats winning in 2006.

Finally, a running theme throughout the book is that general election competition is not a necessary component of American democracy, because competition (and thus accountability) also happens at the primary election level. This seems to me a specious argument. The book's core message is that competition is bad for democracy; thus the book undercuts its own major premise by assuring readers that if we eliminated competition in the general election, it would live on in a different arena. Indeed, if the primary locus of political competition were to shift from the general election to the primary, the (supposedly negative) effects of political competition would shift as well. Instead of general election losers displaying low levels of satisfaction, we would instead witness primary election losers display them.

In the end, I believe that the goal of focusing attention on losing voters is a worthy one, but the particular way in which this book does it is misguided. Rather than arguing that we should minimize the number of losing voters, it would be more useful to take a direct look at the effect of being a

political losing voter. The question of voter satisfaction (Chapter 3) is a good start, but the book could have gone further in this direction, for instance by discussing to what degree the preferences of losing voters influence Congress members' legislative behavior.

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Alasdair Roberts. *The Collapse of Fortress Bush: The Crisis of Authority in American Government.* New York: New York University Press, 2008. xi, 261 pp. (\$29.95 cloth.)

Academics and journalists have quickly formed a consensus interpretation of George W. Bush's "failed presidency." According to this view, the terrorist attacks on 9/11/01 triggered dramatic changes in domestic and foreign policies designed to combat terrorism at home and abroad. The resulting policy departures so aggressively expanded executive power that Congress, the courts, the media and the domestic public pushed back to prevent reemergence of an "imperial presidency." Foreign governments, no less appalled by the specter of a runaway elephant in the global system that used power unilaterally and preemptively (indeed, preventively), refused to follow America's lead. But Alasdair Roberts offers a very different interpretation. Rather than a break with the past, Roberts emphasizes the continuity with pre-Bush developments, identifying policies that were discussed and formulated under Clinton. In addition, Roberts stresses the effects of severe limits placed on executive action in the post-Watergate era. These constraints thwarted Bush's efforts in one policy realm after another, a reflection of Americans' "antistatist ethos" (p. 7) but also a result of the neo-liberal reliance on markets that was firmly established during the Reagan administration. In essence, the post-millennial presidency confronts far greater limits on action than either the president or commentators have appreciated.

Far from pursuing a single-minded expansion of executive power, Roberts' narrative notes how the Bush administration was boldly assertive only in areas where it received support and "retreated when confronted with stiff resistance" (p. 18). In the area of homeland security, fewer limits were placed on the civil liberties of citizens than in previous national security crises. Instead, aliens, not citizens, bore the brunt of expanded government efforts to guarantee domestic security through surveillance and detention (Chapter Two). Despite major efforts launched by the Bush administration to reorganize government in order to deal with future crises—principally

creation of the Department of Homeland Security—Hurricane Katrina revealed the administration's failures. FEMA's ineffectiveness illustrated the inability to build new avenues of federal-state cooperation. In truth, however, the barriers to effectiveness might well exceed the capacity of any administration (Chapter Three). Despite its exhortations about terrorist threats, the administration made no appreciable progress in guaranteeing the security of critical infrastructure systems at risk: the nation's chemical facilities, the electric grid or ports. Roberts provides valuable detail of failures in each of these areas (Chapter Four).

These many failures at home contribute to another important development: the militarization of policy. As Roberts argues, U.S. policy is a "combination of debility [at home] and power [abroad]" (p. 23). The result is a "neo-militarism" evident in the repeated resort to military solutions for a wide range of problems outside the traditional purview of the military including natural disasters (Katrina), illegal immigration, and democracy-building abroad. The military gained greater autonomy from political control with the creation of an all-volunteer force and the shift to high-tech weaponry. Both meant that combat casualties would generate less political response. Legitimacy of the military also grew in the wake of Vietnam as public trust was restored and stood in particular contrast to the decline in trust in most other U.S. institutions. As a result of enhanced autonomy and legitimacy, using the military to carry out government action has become a favorite instrument (Chapter Five).

Much like the failed government response to Katrina, the administration's fumbled efforts to rebuild Iraq demonstrated a chronic inability to achieve bold goals; success "was thwarted by the inability to develop the administrative capabilities required for their accomplishment" (p. 133). As failures mounted, "fortress Bush" collapsed. The exterior image of a unified administration evaporated with the appearance of rampant leaks and an avalanche of kiss-and-tell accounts where insiders cashed in on their access to power. The vaunted secrecy maintained during the Bush administration's first years in office disappeared as "the tell-all industry became the powerful engine of transparency" (p. 139). Roberts also details numerous intra-governmental battles with important bureaucratic constituencies, particularly the CIA and the Department of State, where leaks to the media were followed by whistleblower accounts. Even dissent in the military was widely publicized (Chapter Six).

What Roberts has produced is an early revisionist interpretation of the Bush presidency, perhaps the first in print. Rather than a restoration of the "imperial presidency," an old narrative drawn from the Vietnam era, Roberts paints the portrait of a "dysfunctional presidency" (168) beset by crises, flailing in the effort to construct responses through deeply flawed policy-

making processes and ineffective or non-existent links to the permanent bureaucracy. At heart, Roberts offers a neo-Neustadtian view of the chief executive where “the president often lacks the power to command” (p. 174). Bush appointees failed to negotiate necessary understandings with other centers of power (particularly the permanent bureaucracy), and were collectively unwilling to challenge established conventions. In this way, he argues, neither President Bush nor the commentators using an outdated narrative correctly appreciated the degree to which a 21st century president is more severely constrained than ever. Bush’s ineffectiveness was not inevitable but he recognizes the degree to which executives will be hemmed in by societal and institutional changes. In a fashion befitting Neustadt, Roberts’ final chapter reviews nine of these influences: a more complex institutional context with more elaborate procedures and more deeply entrenched cultures; “a web of statutes, watchdog agencies and nongovernmental constituencies that did not exist before Nixon’s election in 1968” (p. 165); less budgetary flexibility as a result of growing shares of the national budget devoted to mandatory expenditures; neo-liberal suspicion about government intervention into the market (likely to recede after the financial system failures of 2008); more complex and growing markets opposed to regulation; declining public trust in government; declining trust among elites toward each other; eroded norms of commitment and loyalty among bureaucrats toward administrations; and digitization of communications which allows “instantaneous dissemination of leaked information” (p. 167). Following Roberts’ logic, success will be enormously difficult for Bush’s successor to achieve, something likely to be lost amidst heightened expectations.

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Bartels, Larry M. *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton: Princeton University Press, New York: Russell Sage Foundation, 2008. 328 pp. (\$29.95 cloth.)

In this meticulously researched book, Larry Bartels disabuses the reader of any notions he or she may have about an ever-expanding democracy in the United States. Government policy makers and politicians are generally nonresponsive to the polity. When these actors do respond, they respond to those with the most money. Bartels destroys our illusions about equal access to power through participation, government checking the tyranny of corporate titans, and class and race as mutually exclusive variables. He demonstrates that the rich have more influence and that govern-

ment, particularly when under Republican control, protects the rich more vigorously than the poor or the working class. The three main points are: (1) The Republican Party has not captured working class and poor citizens by a focus on cultural issues; (2) Republicans represent the interests of the wealthy; and (3) Neither party is particularly interested in the welfare of the poor.

Unequal Democracy makes a dent in the work by Frank (2004), who argues that we are distracted by “cultural” issues promoted by politicians who need to get our minds off economic, educational, and infrastructure issues—you know, the stuff that matters. While Bartels’s work does not definitively erase the power of Frank’s argument about cultural issues, it does offer other explanations for the string of Republican victories at the presidential level. Republicans are careful to make economic gains during an election year, while Democrats raise the standard of living for the working and poor earlier in their administrations. Most people only look at their current year’s situation when deciding how to vote.

The historical comparison to the Gilded Age with current economic conditions is striking, and it leads naturally to the other points made in the book about the effects of the accumulation of wealth on the stagnation of wages and the poor’s ever-dwindling political influence. A group has influence when politicians enact policies that serve the group’s interests. There is no doubt that with the exception of the latest increase in the federal minimum wage, Congress is not serving the interests of the poor. We need to acknowledge that some experts argue that minimum wage policies hurt the poor, but let us be optimistic. Perhaps the increase in minimum wage will result in some small gains for the working poor.

There are small bones to pick with Bartels’s methods. The first one is why he chooses abortion and the role of women as key cultural issues. Clearly all survey analyses are constrained by the questions in the National Election Study. However, if we are to be fair toward Frank and others, the culture wars are not so specific -- we cannot render them to just the abortion issue and women’s roles in society. Issues of gay marriage rights, immigration, and separation of church and state might also be included.

The table on “Issue Preferences and Presidential Votes by Income Class, 1984-2004,” indicates that “government spending” and “defense spending” are more important among White voters than abortion or women’s roles in society. He neglects to add that “government jobs” and “aid to Blacks” also make the list, and both can be confounded with government spending. Bartels mentions the good work preceding his analysis from Edsall and Edsall (1991), who introduced to scholars the power of racial coding, which should make us all be more careful with specifying variables in survey data. We cannot know if respondents’ answers

on the government spending question mask concerns about welfare and aid to Blacks (p. 84).

In Chapter 4, Bartels does an excellent job of supporting his three hypotheses that explain voter behavior in any election year—myopia, current earnings, and how much money is spent by the winning candidate. The latter hypothesis is my second small bone to pick. Bartels words this theory by positing that there is a relationship between the size of the campaign budget and the victories of the Republican Party. He then tackles the *effects* of such spending. While he addresses the incumbency advantage and the enhanced ability of the rich to contribute to campaign spending, Bartels fails to explore the nature of the media advertising that makes having a large budget essential to victory. In other words, you need more than money. You need a message, substantive or not, that resonates with voters. This section might have been clearer (pp. 120-124).

My third and final small bone is with the analysis in Chapter 5 on questions regarding support for overall egalitarian values. Nearly half agree with the statement “The country would be better off if we worried less about how equal people are” (p. 131). While the first question on equality of opportunity elicited a large percentage of egalitarian responses, he does not discuss why the last question, on its own, stands out in that nearly half of the respondents would rather not think about inequality. Respondents are prioritizing equality of opportunity rather than outcomes. If you think that providing equal opportunity is important, but you do not think we should worry about it, what does that mean? It likely means that many Whites feel that there is plenty of opportunity for equality, but only enterprising and worthy individuals can take advantage of those opportunities. In a study by Ratina, Form, and Pease, there is evidence that wealthier White citizens attribute economic success to individual characteristics (Rytina, Form, and Pease 1970, p. 713). Other scholars, including Jennifer Hochschild, have found similar patterns (Hochschild, 1986).

Minor bones removed and neatly piled on the side of the plate, Bartels leaves us with a very satisfying read. Chapter 8 is a historically grounded and nuanced account of both sides of the minimum wage debate. The book is data heavy, but many readers may find the numerous tables and charts informative and fun. Bartels does not let the data get in the way of his story, which ends with a moving portrait of real-world outcomes of unequal democracy. He touches the third rail, at least in some circles, of the Hurricane Katrina tragedy. Bartels courageously and pointedly challenges the conflation of race and class in governmental response to the crisis of citizens in the ninth ward of New Orleans, a crucial case study that illustrates his theory that “government doesn’t care about poor people.”¹ This book is a fine contribution to the literature on political participation, parties, and

processes. It effectively argues that economic inequality has profound and near catastrophic effects on the ability of poor citizens to participate in politics, government is more responsive to the affluent, and even poor people often do not understand their economic interests. This is the bad news. The good news is that Bartels still believes parties matter, even if the reasons that they matter do not include greater representation of citizens.

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NOTE

¹In a NBC network relief telethon for victims of Hurricane Katrina, aired on September 2, 2005, rap artist Kanye West broke from the script to announce, “George Bush doesn’t care about Black people.” The comment raised the ire of many in government and the media. I amended it, for the reader’s amusement, for this review.

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Evan Gerstmann. *Same-Sex Marriage and the Constitution*, 2nd ed. New York: Cambridge University Press, 2008. xiv, 231 pp. (\$70.00 cloth; \$23.99 paper.)

In *Same-Sex Marriage and the Constitution* (2nd ed.), Evan Gerstmann expounds on his first edition written in 2003, given the changes in the law based on recent court decisions affecting homosexuals. In *Lawrence v. Texas*, 539 U.S. 558 (2003), the U.S. Supreme Court invalidated Texas’ homosexual sodomy law. In *Goodridge v. Department of Public Health*, 798 N.E. 941 (2003), the Massachusetts Supreme Court, relying on the just-decided *Lawrence* case, invalidated the state’s law that forbade same-sex marriage. As Gerstmann points out, nearly 6,000 same-sex couples got married in the first year after the decision (p. 5).

For Gerstmann, same-sex marriage is one of the “most important constitutional issues today” (p. 3). This is a very bold claim given that just 2.8 percent of American males age 18 or older and 1.4 percent of females age 18 or older identify themselves as homosexual, gay, lesbian, or bisexual [*Amicus Curiae* in support of petitioners *Lawrence and Garner v. State of Texas*, No. 02-102 (U.S. March 26, 2003) p. 16]. Further on, Gerstmann follows this claim with “no right is more important to basic human happiness than the right to marry the person one loves” (p. 7). Again, a very strong assertion. The casual reader might infer then that the brashness of the author would color any future analysis of this issue, turning the book into a polemic tome that will alienate some readers. Gerstmann himself recognizes this when he states that he has “no claim to objectivity” but that he will do his best to attempt “fairness” (p. 11). I must say, he meets that standard—he is a passionate advocate for legal recognition of same-sex marriage and he presents his argument honestly and even-handedly. While some readers will disagree with his premises, they will come away with a better understanding of the underlying components of the arguments and why they are made.

Gerstmann’s primary jumping off point is the question of what is the government’s interest in banning same-sex marriage (p. 21)—that is, the constitutional balancing of individual interest against government action. He actually takes it a step further because his discussion centers on two people of the same sex who love each other and want to marry. This allows him to posit the arguments against same-sex marriage and knock them down logically and forcefully. For example, the arguments of definition—marriage as always been defined as between a man and a woman, tradition—marriage has always been between a man and a woman throughout time, and religion—marriage is between a man and a woman because that is what religion says it must be, are countered by Gerstmann as “irrational”: definitions and traditions change over time and not all religions agree with that same-sex marriages violate religious tenets. Furthermore, the idea that same-sex marriage is for the purpose of protecting reproduction is also dismissed because states already allow opposite-sex couples who cannot reproduce (through injury, disability, or incapability) to marry. These reasons, alone or in combination, according to Gerstmann, stand in the way of the logical extension of the right to marry to same-sex couples.

Two additional topics that have been used to justify bans against same-sex marriage also receive the similar treatment as the reasons above although Gerstmann’s reasoning regarding why they should be discounted appear somewhat weak given the points of his arguments above. These topics are polygamy and incest (pp. 105-111). Regarding polygamy, one of his conclusions that polygamy will continue to be banned is that “there certainly seems to be a difference between a right to marry *whomever* you want and

marrying *however many* people you want” (p. 110, emphasis in original). He says doing so (allowing polygamy) would “alter the legal structure of *every* couple’s marriage” (p. 110, emphasis in original), and this is why dual-partner same-sex marriages will not have the same effect on traditional heterosexual marriage. As for incest, he claims that it is a “strong taboo” that triggers revulsion in people (p. 112), but that there could still be limited categories of incestuous marriage. Furthermore, Gerstmann claims that just because something is “shocking or repulsive” does not mean it does not merit Constitutional protection (p. 114).

As the title implies, the main focus of the book is the constitutionality of same-sex marriage. To constitutional law scholars, this means applying legal tests regarding discrimination against individuals as derived through case law. These tests set out the standard of scrutiny by which the Court will determine the constitutionality of the state or federal statute. The weakest of these tests is the “reasonable standard test” that has been applied to same-sex couples—as long as the state offers what the court believes to be a reasonable link between the aim of the statute and need for disparate treatment, the law is presumed valid. Gerstmann argues that because marriage is a fundamental right—and points to specific language in numerous court cases where the courts have made that exact claim—same-sex marriage should be viewed under the “strict scrutiny test” that holds that all laws aimed at disparate treatment are inherently invalid absent the most narrowly tailored law by the state. Despite the courts’ language regarding marriage as a fundamental right, no court has explicitly held that it is. In fact, Gerstmann spends some time discussing this right relative to the most recent case dealing with marriage, *Loving v. Virginia* 388 U.S. 1 (1967), and comes to the conclusion that the U.S. Supreme Court has not yet held (formally) that marriage is a fundamental right.

Gerstmann then sets out to do just that—argue that marriage *is* a fundamental right. The key for doing so is that once the Court makes that legal determination, it must apply strict scrutiny to the statute in question. To do so, however, Gerstmann must first identify the standards that should be used to label something a “fundamental right”: one, whether the decision squares with precedent; two, whether it is inherently connected to other rights; three, whether government exercises monopoly power over it; and four, whether it runs afoul of the political question doctrine (p. 145). By framing the debate in such a manner it should come as no surprise that Gerstmann is able to construct his support based on those parameters.

Gerstmann ends with the discussion of the role of the courts in countering majority opinion and offers several examples that indicate that consistency has not always been the courts’ strategy when it comes to protecting individual rights, or conversely, determining what rights will be infringed

upon. By returning to a strong level of consistency, or paying attention “to the role of clear, well-developed legal principles in protecting legal equality” (p. 212), he argues, courts will make the rational decision that same-sex couples will be afforded the same fundamental right to marry as opposite-sex couples.

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Stephen Skowronek. *Presidential Leadership in Political Time: Reprise and Reappraisal*. Lawrence: University Press of Kansas, 2008. xiii, 192 pp. (\$34.95 cloth; \$16.95 paper.)

Frankly, I was expecting a book with the subtitle *Reprise and Reappraisal* to be nothing more than an update of the author’s past seminal work on the presidency. That past work already stands as an outstanding theoretical contribution to the history of the presidency. But surprisingly, this new short work is much more than a simple restatement of the author’s prior work on political time. It provides challenging new ideas and extremely well written, fascinating essays that help us to better understand the amazing sweep of presidential history.

In his past work, the author laid out a convincing idea of how presidents operate in political rather than secular time. The common method used by many scholars is to examine presidents in particular historical eras, such as an examination of the various Jeffersonian presidents or the Jacksonian presidents. But Skowronek argued that presidents who share a similar period in political time might have much more in common than those who serve in a particular historical era.

For example, Skowronek argued that certain presidents arrive in office at a time in which the old political order has fallen into disrepute. This provides these presidents with the unique opportunity to construct a new political order. These transformative leaders, such as Jefferson, Jackson, Lincoln, and Franklin Roosevelt, are often identified as the great presidents. Next in line of political time are presidents who have the more difficult task of maintaining an existing order. These presidents, such as Polk, Teddy Roosevelt, and Kennedy have to hold together a fracturing political coalition, while also moving forward with their policy agendas. These presidents, even if they have policy successes, usually find that their political coalitions become frayed over time. As the political philosophy of the coalition falls into even further disrepute, presidents of disjuncture have the thankless task of trying to hold together a party that is fractured beyond repair. When they inevitably

fail the door is open for a new transformative period and a new reconstructive period. Also governing during each distinct political time are presidents who do not share the same political affiliation as the reconstructive president. These presidents must amalgamate a third way approach, combining their ideals from their own party's philosophy, without abandoning the ruling paradigm of the relevant reconstructive president. Hence, neither Eisenhower nor Nixon had the luxury of entirely turning their backs on the precepts of the New Deal, nor could Clinton ignore the logic of the Reagan Revolution.

In the present work, Skowronek further develops his thesis with a series of interesting essays that nicely demonstrate how the politics of such seemingly different presidents as George W. Bush and Lyndon Johnson have much in common. Skowronek argues that this is not merely a historical curiosity, but rather has to do with similarities in the types of leadership challenges that both men confronted. Both were presidents who were governing during a period in which they were maintaining a political coalition (FDR's and Reagan's, respectively) that was becoming increasingly fractured over time. Another essay comparing similarities between Polk and Kennedy is of equal interest.

The most interesting essay of all, however, is the last. It re-examines the imperial presidency thesis. It is interesting that maintaining presidents have each had their own war: Polk's war, Johnson's war and Bush's war. Furthermore, one maintaining president who fought the impulse to go to war, John Adams, saw his re-election prospects destroyed rather than have an Adams' war that was not winnable. Skowronek writes (p. 166), "This episodic impulse to overreach and rush headlong into the quagmire does not constitute a particularly admirable, efficient, or dependable mechanism for recalibrating constitutional power. . . . But for a polity that has been playing fast and loose with its constitutional design for 225 years, there may be little left to help balance things out save these contingencies of political time."

Since I read this book during the 2008 presidential election, it was impossible not to think about what Skowronek's thesis tells us about our next president. Had McCain been elected there is a strong possibility that he would have been a president of disjuncture, governing during difficult and hardly propitious times, tied to a political philosophy that has fallen into disrepute. The election of Barack Obama, however, provides an opportunity to construct a new era of political time that can replace the fractured Reagan coalition. Whether President Obama does so or not will depend a great deal on his unique political skills. But the potential for a reconstructive presidency is there. But if so, then this begs the question, was George W. Bush really a maintaining president? If so, did he so vastly overreach that he

discredited an entire era of political leadership? Could he then have been both a president of maintenance that turned into one of disjuncture?

These are interesting possibilities, for in his past work Skowronek discussed the probability that the concept of political time may be less relevant in the future. Even here he discusses how new technologies and election processes may ultimately dilute the logic of political time. But his solid reasoning suggests that while the logic of political time may be altered, it will continue to be relevant. We will see new presidents who are similar to their predecessors in political time because they face similar political incentives and constraints.

The only major criticism that I have of the present volume is that I wish the author would combine his discussion of political time with the election theory of realignments. By combining these two approaches it will be possible to show why realignments occur periodically in American history and how presidents are the engines that drive these realignments. In other words, I believe that the ground upon which Skowronek has planted his theory is so fertile that it provides the basis for further development. I hope then, that this is not the last book Skowronek writes on this topic.

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Dorothy F. Olshfski and Robert B. Cunningham. *Agendas and Decisions: How State Government Executives and Middle Managers Make and Administer Policy.* Albany: State University of New York Press, 2008. 157 pp. (\$50.00 cloth.)

This volume, evidently long in preparation, rests at the intersection of at least four major avenues of research and analysis in the field of public policy and administration. The first and perhaps most neglected avenue is state government administration and management. The second is the leadership and management of style of a governor, in this case, Lamar Alexander (Tennessee 1979-1987). A third more well-beaten pathway is the pattern of policy implementation, particularly the interactions between and among top-level political executives and middle managers in this instance charged with policy implementation within Tennessee state administrative agencies.

The last but by no means least traveled avenue leading into the intellectual intersection of this volume is that of administrative theory. Theory in several if not most respects is a prominent thrust of *Agendas and Decisions*. According to the authors (p. 115), the book “. . . seeks to inform both theory and practice about how executives and middle managers in a devolved,

state-level management system make and administer policy. Theory knowledge (knowing what) and practice knowledge (knowing how) are found in the authors' descriptions and analyses."

The theoretical avenue followed by the authors is a four-stage "Decision Process Model," specified in Chapter 2 textually and in a diagram showing the formal components of the model. The four stages are Problem Identification (Ch. 3), Alternatives and Decision Making (Ch. 4), Implementation (Ch. 5), and Evaluation (Ch. 6). Antecedent to but not overlooked is the avenue of agenda setting. In the avenues-intersection metaphor agenda setting is best described as the red-yellow-green- traffic lights controlling entry into the complex traffic exchange of problem identification.

The above sequential listing of chapters framing and containing the content(s) of the model's components might imply a linear and regularized process by which (a) the authors describe and (b) how the Alexander administration managed Tennessee state government. In both respects the implications are wrong. The authors are explicit in noting that (p. 4), "in the upper reaches of Alexander's governance system, neither hierarchy and the rational decision model nor incrementalism dominated." Not only the text supporting the model but the warp and woof of the chapter contents are filled with qualitative stories. Indeed, "Stories are the primary data source for this study" (p. 5).

The story quotation represents the tip of an iceberg which caps an array of methodological and epistemological issues. These include tradeoffs between quantitative and qualitative data collection and use, positivism versus postmodernism in fact/value analysis, constructivism versus deconstructivism in drawing inferences, and objectivism versus subjectivism regarding administrative reality. Clues to what is submerged come from two quotations: "we theorize about practice through stories" (p. 4) and "The fall of the positivist monolith has brought forth a range of ideas and approaches for studying organizations" (p. 5).

The foregoing is sufficient to convey the focus, framework, and features of this spare, thoughtful, reflective, and analytical volume. The reader comes away with several messages, two of which are most prominent. One is how the Alexander administration largely succeeded in installing across Tennessee state government a "management system [that] stands as a prototype of devolved, strong cabinet governance at the state level" (p. 115). A second message is how the authors gained access and assembled the data (stories) on which the book is based. They were staff members who helped design and manage the Tennessee Government Executives Institute starting in 1983. That Institute trained middle and upper level state managers and also enabled the authors to collect the stories that constitute the data for the book.

A few concluding comments merit mention. First, with respect to timeliness, the volume might have served as a classic case study two decades ago illustrating what Sabato in his book on governors (1983) termed, *Goodbye to Good-Time Charlie*. The Tennessee case reveals that a governor can “win” both politically and administratively in governing a state. A second observation is more content and presentation focused. Of necessity the numerous stories that populate the pages are short vignettes. Unfortunately, some come across more like snippets than cases involving thick description of decision-making, implementation, or evaluation. The reader sometimes wonders what more lies behind or below each “case.” An associated problem is the difficulty of keeping track of the cabinet heads in whose departments the cases (stories) are located. An organizational chart with the names of major department heads would have served a useful orienting purpose. This tool and perhaps others would have eased the difficulty confronting a monograph resting heavily on organization theory but at the same time drawing on data (cases) extracted from at least a dozen different organizations.

This book merits commendations and attention from multiple standpoints, most of which have been identified above. It is an exceptional single-state case study providing insight about a strategic approach to administering a state’s policies and programs. It reflects a coherent, concerted, and conscious effort to place a meaningful theoretical template on public administration and policy processes.

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