

## Book Reviews

Richard L. Engstrom, Editor

**Angie Maxwell and Todd Shields, eds.** *Unlocking V.O. Key Jr.: Southern Politics for the Twenty-First Century*. Fayetteville: University of Arkansas Press, 2011. xxxii, 231 pp. (\$29.95 paper).

This impressive collection of essays aims to be a companion volume to V.O. Key's classic *Southern Politics in State and Nation*. There are a number of intriguing directions one could go with such a volume. One could try to fill in the gaps in Key's original work, addressing topics and issues that he neglected. One could update the original, extrapolating the trends identified by Key to the 21st Century. One could address the broader significance of Key's work not just for area specialists but for political scientists in general, highlighting what *Southern Politics* says about political science both in its own time and our own. One could address the current controversies in the study of Southern politics stemming from Key's work. Or one could try to strike out in all of these directions at once . . . and that is what Angie Maxwell and Todd Shields try to do in this brief but rich volume. The result is a book that definitely accomplishes a great deal but also leaves the reader wanting more out of each author and each chapter. Given how important these multiple goals are and how much the authors have to say in pursuit of them, this project could have easily generated three or four books instead of one.

The book originated from a conference sponsored by the Dianne Blair Center of Southern Politics and Society and the Winthrop Rockefeller Institute at the University of Arkansas. The work opens with a substantive Foreword by the Conference's Keynoter, Byron Shafer, coauthor of *The End of Southern Exceptionalism*. Shafer begins the book with an interesting brief detour into the *other* major projects of V.O. Key's career *besides* Southern Politics, dealing with public opinion, pressure groups, and political parties. Although Shafer is not the first to draw parallels between *Southern Politics* and Key's other work, this discussion really deserved a full-blown chapter later in the book, perhaps in the second section of the book which is devoted to broader political issues addressed in *Southern Politics*.

The first section of essays in the book deals with the areas of neglect and omission in *Southern Politics*, and the contributors in this section take pains to focus on evidence and sources that would have clearly been available to Key and his collaborators (and, in more than one instance, cite specific evidence that Key himself had consulted or even commissioned certain

sources!) In other words, they avoid the academic sin of confronting an author with material that he could not have known about or that was unavailable to him. Historian Charles Reagan Wilson addresses the relative absence of religion in Key's South, ably refuting the fallacy that the infusion of religion into Southern politics is a recent phenomenon brought about by the Christian Right in the 1970s and 1980s. Wilson points out how crucial religious activism on the Prohibition issue was in bringing about the first major apostasy from "Solid South" Democratic voting in 1928 . . . and earlier on, had (at least for a time) even raised the spectre of a genuinely biracial Christian temperance movement in the South (Wilson, p. 15). In separate essays Pearl Ford Dowe and Kari Frederickson take issue with Key's implicit characterization of blacks as largely passive victims in the struggle for their political rights; tremendous amounts of research by Ralph Bunche a few years earlier for Myrdal's *An American Dilemma*, and Alexander Heard's own interviews with black activists for the Southern Politics project itself, refute the idea that blacks were too apathetic or intimidated to work collectively for enfranchisement. The authors suggest that Key's rather blank portrayal of black political engagement may have been due to Key's near-exclusive focus on conventional, legal, and individual forms of political participation; both black protests and pressure group activity, and the violent backlash of whites in response to black activism, escape Key's scrutiny. Frederickson's chapter documents several widely reported incidents of white violence in response to black assertion of constitutional rights right after World War II; these incidents surely were lurking in the background of Key's interviews but are only characterized vaguely as "intimidation."

The second group of essays deals with Key as political scientist, providing a window through which to view the discipline as practiced in Key's day and now. Margaret Reid's chapter provides a nice transition from the first section to the next, using a critical theory perspective to link Key's specific substantive omissions to the blinders narrowing the vision of 20th Century mainstream political science when it came to gender and race. Ronald Keith Gaddie and Justin Wert review Key's clever use of quantitative methodology, using often less-than-ideal data, to shed light on issues such as the impact of the poll tax; but they also characterize *Southern Politics* as a shining example of "problem-driven" research that employs mixed methods, resting on fundamental normative principles, to inform our understanding of urgent social problems. Harold Stanley presents fascinating data from an informal survey of teachers of Southern Politics courses, along with data from academic search engines, to demonstrate the book's lasting (but somewhat distorted) legacy. Stanley observes Key's penchant for making dramatic sweeping generalizations (such as the famous "the politics of the South revolves around the position of the Negro") followed by qualifica-

tions, “yet it is far from the truth to paint a picture of southern politics as being chiefly concerned with the maintenance of the supremacy of white over black” (Stanley, p. 109). Predictably, the generalization has been universally cited; the qualification, not so much. Stanley goes on to note that one of Key’s most powerful statements—that the politics of the South is not adequate to deal with its social problems—has been strangely neglected by Key’s followers, and makes an intriguing suggestion as to how Pew data on the quality of state governance could be used to explore that hypothesis today (Stanley, p. 108). I hope that someone—maybe Stanley himself—takes that bait! I also wish that Stanley had included not only data on how many instructors use *Southern Politics*, but in what manner they use it and how they integrate it into their courses. When regaling my students with stories of E.H. Crump, Theodore Bilbo, and the Talmadges, the looks on my students’ faces occasionally make me wonder along with them “Why do we have to learn about this stuff that seems like it’s from another planet entirely?”

The final section, which addresses contemporary issues in the study of Southern Politics actually produces more data relevant to this last issue, that is, the contemporary relevance of *Southern Politics* in such a drastically altered region. Susan MacManus’ chapter provides up-to-the-minute data on the trends Key wondered about in his final chapter—in-migration, generational change, and racial change, plus trends he did not foresee (except in Texas), such as immigration and the growth of Hispanic and Asian populations. My only quibble with the presentation of this fascinating material is the small size of the graphs: given the extreme outlier nature of Florida and Texas on many of these measures, it is really difficult in such small charts to make out trends in the bottom seven or eight states. (Perhaps a companion website could provide the raw data or larger graphs!)

The “main attraction” for many readers of this book, of course, will be the face-to-face confrontation between the historian Dan Carter and the political scientists Byron Shafer and Richard Johnston concerning the roles of race and class in the Republican realignment since Key’s time. Shafer and Johnston nicely summarize the main arguments of their 2006 book, with updated data that includes the Obama election. They also introduce religion into their analysis, a topic largely neglected in the 2006 book; I look forward to their future work on the relationship between religious affiliation (evangelical vs. mainline) and class. But, for the most part, Carter and Shafer and Johnston mainly restate their primary arguments: Carter’s insistence that class and race cannot be quantitatively disentangled given the subtlety of contemporary racial cues and attitudes, vs. Shafer and Johnston’s insistence that this disentanglement can and must be done to get an accurate explanation of the current Republican hegemony. In an apparent concession to

Carter, however, I do note that Shafer and Johnston cite some historians whose work was not cited in their 2006 book!

Wayne Parent closes with a pedagogical essay that rarely is found in an academic book of this kind: he not only summarizes the authors' contributions, but makes very helpful suggestions as to how the essays could be used for undergraduate and graduate teaching. Parent's chapter draws attention to the main virtue of this book: the editors and authors somehow have managed to produce a collection of essays that is interesting, relevant, and accessible to undergraduates, graduate students, and professors. To say this, and to repeat that I wish that every essay were longer, is the highest praise I can give to an academic book. V.O. Key, Diane Blair, and Winthrop Rockefeller would all be proud and pleased with what their legacies, editors Maxwell and Shields, and these twelve scholars have combined to create.

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**Douglas L. Kriner.** *After the Rubicon: Congress, Presidents, and the Politics of Waging War*. Chicago: The University of Chicago Press, 2010. xi, 322 pp. (\$90.00 cloth, \$30.00 paper).

Constitutional scholars have long argued about the role of Congress in the use of military force. While the Constitution is clear on giving to Congress the power to declare war, what is less apparent is the legislative branch's power to involve itself in waging a conflict. Its ability to have an effective role in influencing presidents' decision-making once a conflict has ensued is largely misunderstood as well. With the United States being involved in greater numbers of military conflicts, both major and minor, in the 20th and 21st centuries, this is a question that is of even greater importance.

Douglas L. Kriner's book *After the Rubicon* attempts to remedy an oversight in the existing literature about Congress's role in going to war and influencing the duration and outcome of military conflicts. While books abound on the president's role in military conflicts, the role of Congress draws less attention. Kriner states "prior scholarship tells us surprising little about how interactions between the executive and legislative branches continue to shape the conduct of military ventures once they are launched" (p. 2).

To study this interaction, Kriner offers four hypotheses around which the book revolves. The first three hypotheses all involve partisan control of Congress. The first hypothesis states when the president's party controls Congress that the President will be more likely to initiate military engagements. In this same vein, the second hypothesis asserts that presidents are

more likely to engage in larger-scale actions when their party controls the Congress. Kriner also posits that when the president has the luxury of his party controlling Congress that it yields military conflicts of longer duration, like the wars in Vietnam in the 1960s or Afghanistan and Iraq in the early 21st century. The final hypothesis states that public congressional support, or opposition, to a president's military policies will have a direct impact on either the duration or brevity of a conflict.

To answer these ambitious hypotheses, Kriner uses robust statistical models to test a large variety of variables. Overall, Kriner finds support for his contentions about partisan conflict having influence upon a president's willingness to commit military force and the duration of that force. Contrary to the conventional wisdom that Congress's influence ends once it has approved either a declaration of war or authorization to use force, Kriner produces statistical evidence that members of Congress continue to exert influence over a president in the midst of military conflict. While the statistical models focus on military conflicts, both declared wars and smaller military actions, from 1877 to the present, Kriner primarily focuses on the post-World War II era. He finds support for his hypotheses that partisanship has much to do with a president's willingness to engage military forces and for how long. The statistical models also lend support to the argument that presidents will avoid the use of large-scale ground forces when their party has smaller majorities or during eras of divided government. Presidents also generally seek short-term actions in the face of divided government. In contrast, presidents during periods of unified government are more likely to engage in major conflicts like Korea, Vietnam, or the War on Terror.

Kriner also finds evidence that Congress continues to influence decision-making about wars once they have begun. Through a variety of formal and informal means, Congress's public support or opposition to a conflict is one of the most direct influences on a president's decisions during the midst of military conflict. Far from the old adage of "politics stopping at the water's edge," Kriner presents empirical evidence of how Congress can influence the course of conflict. While Kriner discusses the Vietnam era and the War Powers Resolution, he uses the presence and eventual withdrawal of the U.S. Marines from Beirut during the Reagan Administration as a dedicated case study. Using archival data from the Reagan Library, Kriner presents a compelling case of how Congress had a large impact upon President Reagan deciding to withdraw the Marines in early 1984. While the Marines' presence in Beirut initially drew almost no criticism from members of Congress, the pressure on the Administration after the suicide bombing in October 1983 finally played a large role in Reagan's decision to withdraw.

While this book presents some interesting insights, one of the shortcomings is the relative lack of case studies outside of the Beirut example.

Throughout the book, Kriner's statistical models are quite impressive, but the work would be even stronger if more case studies were incorporated to show how these various hypotheses apply. Perhaps one of the largest surprises is that the final chapter of the book is an addition to include some discussion on the wars in Afghanistan and Iraq. In fairness, given that we still fighting the war in Afghanistan and slowly withdrawing from Iraq, we do not know how the conflicts will ultimately conclude. Nor did Kriner have access to archival data given we are still in the midst of these conflicts.

Incorporating the War on Terror into the previous chapters, rather than coming at the end of the book, would have made for a more lucid argument. One of Kriner's insights is that President Bush was relatively immune from congressional opposition in Iraq after 2006 because of his lame-duck status. Even when he faced formidable opposition, Bush's ability to order a surge in 2007 illustrates that even with opposition that presidents still wield the sword of state.

While current conflicts can be more difficult to tackle, there are a number of other military conflicts from which to choose. Kriner presents an impressive list of military conflicts dating back to 1945, many of which would have made interesting case studies where these various hypotheses could have been applied. With that said though, this book is an interesting study that expands our knowledge about the dynamic interaction between the executive and legislative branches in the use of military force, the conduct and duration of conflicts, and the end of conflicts. Scholars of these two branches, as well as constitutional scholars, will find insights into the politics of war making offered by Kriner's research.

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**Monica W. Varsanyi, ed.** *Taking Local Control: Immigration Policy Activism in U.S. Cities and States*. Stanford, CA: Stanford University Press, 2010. 320 pp. (\$65.00 cloth, \$24.95 paper).

Scholarship on the political, economic, and social impacts of contemporary immigration patterns is increasingly theoretically informed and empirically grounded. Monica Varsanyi's edited collection is an important contribution to this vibrant literature. While immigration studies often focus on the political dynamics constraining coherent national policies or the international context in which migration flows occur, local communities are on the front line in dealing with these impacts. Yet there are few analyses of subnational immigration policy making in the United States so one of the

most compelling contemporary political arenas is the least understood. This collection addresses that gap in our understanding and sketches a timely scholarly research agenda.

As Wayne Cornelius points out in his thoughtful Preface, a better understanding of subnational immigration policy making is needed because this policy activism affects national immigration policies both in terms of their impacts and effects as well as their substantive orientation. The active role of the federal judiciary in mediating conflicting national, state, and local policies also shapes policymaking at multiple levels. And, of course, many of the policies and programs directly influencing the well-being of immigrants and their U.S. born children are funded and/or carried out at the local level. As a result, there is tremendous state and local variation in subnational immigration politics and policies, enmeshed in a complex web of value conflicts, public opinion shifts, historical settlement patterns, electoral ambitions, fear, and community activism.

A stellar, interdisciplinary set of authors begins to unpack these patterns, case by case, looking at states and communities across the U.S. Arguing that the historic distinction between immigration policy—constitutionally the purview of the national government—and immigrant policies regarding individuals is now blurred, the contributors address how and why these boundaries have become so porous and with what consequences. While the book's title suggests a subnational focus, a multi-level approach is implicit in each chapter. The first four chapters set out the emerging "immigration federalism" in the U.S., charting how and when immigration policy responsibilities became stalemated at the national level and activated at the local level. Many states and communities justify their growing activism by the apparent policy void at the national level, but these authors make clear that federal policymakers set the stage for increased local immigration enforcement responsibilities over 10 years ago. With 9/11, these multi-level efforts became more formalized and centralized, with many immigration enforcement responsibilities moved to the new Department of Homeland Security along with the Department of Justice. National policies criminalized undocumented immigration by making repeated efforts at entry liable for felony prosecution and undocumented entry status grounds for deportation. This generated a substantial demand for broader enforcement efforts, one in which state and local law enforcement professionals increasingly become engaged in the national "politics of control" over immigration flows. Cristina M. Rodriguez, Muzaffer Chishti, and Kimberley Nortman trace the evolution of this growing involvement, particularly through 287(g) agreements, while Michele Waslin analyzes the tensions created by these agreements in local police departments and immigrant communities. Monica Varsanyi's analysis of efforts to regulate undocumented day laborers and

Michal Danielson's case study of these efforts in Vista, California introduce a local policing initiative undertaken independent of these federal programs.

While there is tremendous variation in local cooperation with federal programs, there is even more variation in state and local immigrant policy initiatives. As Varsanyi points out in her Introduction, a large proportion of immigrants live in states with relatively progressive, integrative statutes in place even though media coverage emphasizes anti-immigrant activities and legislation. Some of the most intriguing contributions are those that try to sort out the conditions under which localities are more or less likely to adopt restrictive or integrative legislation. Pablo A. Mitnick and Jessica Halpern-Finnerty, for example, note that inclusionary policies appear more likely to be adopted when framed in universalistic, rather than immigrant-specific, terms. S. Karthick Ramakrishnan and Tom Wong go further, analyzing all (176) local immigrant-related ordinances—integrative and restrictive—proposed as of July 2007 to test alternative explanations for policy adoption. While recent changes in the size of the immigrant population are important, their multivariate analysis reveals different dynamics in communities proposing integrative and restrictive ordinances. City size and partisan composition are the most significant factors predicting proposal and passage of both types of ordinances, with party composition able to predict the type of ordinances passed. They argue that scholarly attention needs to be redirected to the politicization of demographic change at multiple levels of government, rather than demographic change alone.

Several fine case studies round out the presentation. Owen J. Furuseth and Heather A. Smith's analysis of Charlotte, North Carolina is especially useful for insights into local immigration politics in new immigrant destinations in the South. Charlotte follows a trajectory evident in several other case studies: an initial welcoming emphasis on integrating newcomers, then, as the scale and speed of new immigration accelerates, the mobilization of anti-immigrant sentiment. Furuseth and Smith's observation that anti-immigrant groups in Charlotte were able to reconstruct the "Old South" legacy of racism and nativism to characterize a new "Latino menace" highlights the distinctive nature of local immigration politics in Southern cities.

Although it is commonplace to note that edited books lack coherence, this is an instance where contributions from scholars across disciplines and cases from diverse communities actually suit the topic. Undergraduates, in my experience, respond enthusiastically to the mix of contextual detail and analytic thinking evident in each chapter. For scholarly analysis to progress, the remarkable, often contradictory, range of subnational initiatives needs to be mapped out and empirical evidence on how and why these initiatives are adopted needs to be documented. Varsanyi and colleagues admirably provide this important stimulus for future analysis. Reading this volume under-



scores the need for multi-level, multi-method research strategies: as Ramakrishnan and Wong note in their conclusion, their large N analysis offers a unique understanding of the patterns of local immigration policy adoption but requires systematic analysis of the contextual detail offered in case studies to understand how and why these decisions were made. This is an important and engaging research agenda for future scholars to consider.

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**Richard L. Pacelle, Jr, Brett W. Curry, and Bryan W. Marshall.** *Decision Making by the Modern Supreme Court*. New York: Cambridge University Press, 2011. 265 pp. (\$85.00 cloth, \$27.99 paper).

This work focuses on institutional judicial decision-making at the Supreme Court level in both civil liberties and economic cases during the 1953-2000 terms. Since the book includes meticulous case analysis in addition to sophisticated empirical modeling, it is applicable to a broad audience in law and social science. The most important contribution is the development of an integrative model of Supreme Court decision-making from both the individual decision-making literature and institutional-level models. The purpose of the integrative model is to offer insight into how strategic, legal, and political conditions shape the Court's decisions. This integrative model better explains the reality of Supreme Court decision making than does a narrow focus on attitudes or legal considerations. To accomplish this goal, the authors construct a theoretical and empirical evaluation of the primary factors explaining the collective decisions of the Court. The book studies the impact of a variety of factors that have affected the Court during the second half of the Twentieth Century. There is support that the Court behaves strategically in an expansive separation-of-powers system. Their evidence proposes that the Court anticipates the political constraints imposed by the president and majorities in Congress.

Following a superb analysis of the three models of Supreme Court decision-making (legal, attitudinal, and strategic models), the authors acknowledge that although each model contributes to our understanding of the Court, individually alone each model is lacking. They argue that judicial behavior is a function of substantive preferences and structural considerations. According to conventional wisdom, the authors argue that constitutional cases provide fewer constraints than statutory cases and that economic cases are less salient to the modern Court than civil liberties and civil rights cases. They find that the overriding factor in the Court's decision-making is

when it is confronted with highly salient cases and few institutional constraints. The two most significant factors were the attitudinal variable (the ideology of the Court as measured by the median justice) and issue evolution (the difficulty of the case on the docket relative to other cases in that issue area.)

They found that in constitutional economic cases that precedent played an important role in decision-making. However, the authors noted that judicial activism may be creeping back into economic cases. In most of the economic issues, the sincere preferences are subjected to the structural considerations. This buttresses their overall viewpoint: decision-making is the result of multiple forces that are intertwined and operate differently under different conditions.

Although the authors do not directly test whether the Court pays attention to Congress in statutory cases, the book's results provide support for the thesis that the Court responds to the current Congress. The role of precedent is more important in these cases and may provide either cover or some indirect support for the attitudinal model variables. They contend that by following precedent in these cases, the Court provides for the opportunity to participate in issues that are more salient to the justices. They found that legal factors, especially precedent, served an important macro role in structuring decision-making.

In the final analysis, the authors found that no single explanation or model is practical in comprehending all decision-making. When viewed holistically, Supreme Court decision-making is a complex process that is a function of the opportunities for and the constraints on the Court. The results suggest that whereas individual justices have a great deal of autonomy and discretion, the Court does not have complete independence. Although the ideology of the Court is the most salient factor, it does not function to the exclusion of other factors. One major finding is that constitutional and statutory cases are subject to different factors. When attitudinal and issue evolution variables are consistently present, the impact of precedent and the House of Representatives is positive and statistically significant in the statutory cases. However, in the constitutional cases and in civil rights and liberties, the president has a significant impact on decision-making. Finally, the authors argue that decision-making is a function of issue salience. More salient issues are more closely watched by the Court and the president. In contrast, decision-making by the Court in less salient issue areas tend to be more responsive to Congress and to precedent. The book maintains that precedent, attitudes, issue evolution, the president, and Congress all influence decision making but they are conditional segments of the Court's environment. The results also suggest that the legal model ought to receive

more attention. For instance, the Court considers precedent and seeks to foster consistency in the law but the application of the law is unclear.

In conclusion, the authors support the concept of the “golden rule” of precedent quite persuasively. Justices tend to support precedent so that their decisions will have the respect of their colleagues. They find that the attitudinal model is far too simplistic because its decision making focuses strictly on exogenous factors. Although agreeing that the Supreme Court is an independent branch of government, the authors believe it is an institution that is influenced by other institutional actors competing for power. At the institutional level, Court decision making is a product of both exogenous and endogenous considerations. These considerations include some loyalty to legal precedent, a group desire to protect the Court’s legitimacy, and some deference for coordinate branches of government. In the last chapter, the authors analyze in a couple of pages the notion that moderate justices, including the median justice, may be more concerned than more ideologically extreme justices with protecting the institutional prestige of the Court in making decisions. This fits well with the authors’ view of the Court as similar to a cybernetic system. When the ideological balance tips too far to the right or to the left, one of the justices in the center usually shifts to counteract it.

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**Graeme Boushey.** *Policy Diffusion Dynamics in America*. New York: Cambridge University Press, 2010. xvi, 221 pp. (\$85.00 hardcover, \$54.40 electronic).

In *Policy Diffusion Dynamics in America* Boushey breaks new ground in a sub-field that has progressed in fits and starts since the early-1990s. Boushey has a fairly ambitious agenda for the book. The main thrust of his endeavor is to move away from the single-issue studies that predominate the policy diffusion literature and develop a more general theory of policy diffusion that recognizes policy types, but is also able to systematically identify characteristics of policies that are likely to diffuse widely and rapidly versus those that will diffuse more incrementally in time and space. In fact, although most traditional theories of policy diffusion focus on the notion that policies diffuse in a fairly incremental pattern, Boushey argues and then demonstrates that while there are policies that diffuse incrementally, there are those that diffuse rapidly. His theoretical framework can account for both diffusion patterns while existing frameworks do not.

In Chapters 1 and 2 Boushey asks us to view the diffusion of policy in a similar manner to how epidemiologists view the spread of contagions, but he also wants to incorporate an information processing perspective outlined by Jones and Baumgartner's (2005) agenda setting framework. Boushey refers to his combined framework of the contagion of innovation and information processing as the dynamics of diffusion. From epidemiology Boushey presents the concepts of agents, hosts, and carriers or vectors, all of which interact within a broader environment. The environment includes systematic forces, such as public opinion, elections, and dramatic events, but is especially centered on issue salience and the national mood. The hosts are the states, with their particular characteristics, while the agent is the innovation or policy idea itself. Carriers, or vectors, are interest groups or other advocates who frame the problem and push for particular solutions before government actors. From an information processing perspective, here focused on state government decision-makers, Boushey focuses on the limited time, resources and attention that government institutions can bring to bear on perceived problems. Given the limited resources of state legislators, they assess problems based on how they see the salience of the issue, issue complexity, and issue importance.

Chapter 2 takes the first step in empirically examining Boushey's theoretical argument. In his first empirical tests he simply demonstrates that when actual policy diffusion patterns are compared to a simulated incremental diffusion process, few policies actually diffuse in the incremental S-shaped pattern that we commonly expect. Some policies do fit a normal distribution pattern, but most do not. So much like Jones and Baumgartner (2005), Boushey concludes that although incrementalism can describe some policy diffusion, there are many policies that do not fit this pattern, especially those policies that undergo positive feedback cycles.

In Chapter 3 Boushey explains the differences in incremental versus non-incremental policy diffusion by focusing on the agent, or the characteristics of the policy itself. Here his framework recognizes that there are important differences across issue areas or so-called types of policy. Specifically, policies vary in their salience, cost, complexity, and fragility. Boushey then goes on to demonstrate that his list of innovative policies can be divided into three groups that vary on these characteristics: morality, regulatory, and governance policies. Morality policies, for example, tend to be salient, inexpensive, and non-complex, and therefore tend to diffuse in a rapid, non-incremental pattern.

But even though the characteristics of the policy issue itself can influence the rate of diffusion across the states, (state) host characteristics and carriers have important roles to play. Chapter 4 highlights the ways in which state characteristics shape the adoption of the three policy types. For

example, states that have a lower information processing capacity (those with citizen legislatures) are less likely to adopt innovative regulatory policies in a timely manner, if at all. And perhaps not surprisingly, state ideology has a significant influence on the adoption of morality policies.

In his examination of the role of carriers or vectors (interest groups and advocates) in Chapter 5, Boushey moves away from his quantitative analysis and relies on mini-case studies of the roles played by interest groups in policy diffusion dynamics. This brief subset of cases supports Boushey's contention that interest groups serve to frame policies in particular ways and serve as vectors to spread the policy innovation across the American states. Although this chapter is illustrative, it also makes it clear that this element of the theory has the greatest need for future empirical research.

Perhaps the largest drawbacks of the book stem from some of its innovations. The incorporation of the Jones and Baumgartner (2005) agenda setting framework, while a plus, is also a drawback. Boushey does over rely on public policy and interest group work at the national level and this apparently leads him to overlook a considerable amount of relevant state politics and policy literature from the 1990s and 2000s. Boushey's sometimes narrow view does not just mean that he fails to cite some important literature, it also means that his selection of policy issue cases for his dataset is limited. Of course his sample of policy issues, which builds on Walker's original dataset, can only be a sample and not encapsulate the full population. And any sample of policy issues could be limited or even biased by data availability; I would simply prefer a bit more transparency on this issue. Some readers might also quibble with the classification of all child related policies and crime policies as morality policies rather than regulatory policies, but this is an empirical question that can be examined by future research.

In short, the book makes a significant contribution to policy diffusion theory but also the empirical study of policy diffusion. Even with its limitations this is an easy recommendation to make for graduate courses in public policy and American politics. The theory is broad enough to also be incorporated into comparative politics courses. Advanced state politics and public policy undergraduates would also benefit from reading it, even though it might be a difficult read for them. Certainly scholars of state politics should consider this volume for their own libraries since it will very likely shape much of the next several years of research on the topic.

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## Reference

Jones, Bryan D., and Frank R. Baumgartner. 2005. *The Politics of Attention: How Government Prioritizes Problems*. Chicago: University of Chicago.

**Joyce A. Baugh.** *The Detroit School Busing Case: Milliken v. Bradley and the Controversy over Desegregation.* Lawrence: University Press of Kansas, 2011. 234 pp. (\$34.95 cloth, \$17.95 paper).

When the topic of school desegregation is mentioned, most people focus on the South and on *Brown v. Board of Education*. The controversy over school desegregation in the North often takes a backseat to what occurred in the South. With the publication of *The Detroit School Busing Case: Milliken v. Bradley and the Controversy over Desegregation*, Baugh reminds us of the controversy that arose in the North over school desegregation. Baugh provides a clear and complete history of *Milliken v. Bradley* and examines various events and court cases surrounding it as well as the political atmosphere at the time. She traces *Milliken v. Bradley* from its inception in Detroit, through the district court, the Court of Appeals, and the Supreme Court. The implementation of *Milliken* and the aftermath are also discussed. Baugh accomplishes all of this using easy to understand language, and she carefully explains concepts that may be unfamiliar to the reader. For that reason, the possible audience for this book is not limited to judicial scholars but extends to the general public and students. In fact, the book would be a great supplement in an undergraduate course that covers school desegregation.

The book is divided into seven chapters. After using the first chapter to discuss *Plessy v. Ferguson* and the cases leading up to its demise in *Brown v. Board of Education*, Baugh then turns her focus to Detroit and its school system, which was at the center of *Milliken v. Bradley*.

Chapter 2 focuses on the political, social, and economic environment in Detroit in the mid to late 1960s, which is the time period leading up to *Milliken*. Baugh points out that civil rights activists were concerned about de jure segregation in the South, but after the passage of the Civil Rights Act of 1964 and the Voting Rights Act of 1965, the activists began to address the de facto segregation occurring in the North. Although the segregation was not legally mandated, many of the major causes of the segregation were legally sanctioned. Baugh goes on to discuss segregation in employment and in housing. The actions of private individuals and the government are examined. Baugh later makes the connection between the segregation in employment, in housing, and the segregation in the public school system.

In Chapter 3, Baugh delves into school segregation in Detroit and in other northern communities. Various pieces of legislation and court cases that addressed school segregation are discussed. After providing a brief discussion of school segregation in the North from the 1830s through the early 20th century, Baugh then turns to Detroit and discusses the history of that school system in detail. The educational system in Detroit is described as

going from segregation to integration and then back to segregation. The major financial crisis within the Detroit's school system is briefly mentioned as well.

Chapter 4 examines the state of the Detroit school system in 1965 until *Milliken* was filed in 1970. Baugh discusses the controversy within the community concerning integration versus community control of black schools. She addresses the actions taken by political officials, including the school board and the state legislature. Chapter 4 culminates with the filing of *Milliken v. Bradley* that charged that the racially discriminatory policies and practices of the school board and the state created segregation in the Detroit public schools.

With Chapter 5, Baugh delves into the specifics of the lawsuit. She provides a detailed account of the entire trial and describes the testimony of key witnesses for the plaintiffs and the defendants. She then turns to the Judge's ruling and provides the substance of the ruling as well as reactions to it. Baugh then follows the case as it is appealed to the 6th Circuit Court of Appeals and discusses the actions of the that court. Chapter 5 ends with the defendants filing a writ of certiorari to the United States Supreme Court in 1973.

Chapter 6 provides a detailed account of *Milliken* once it reaches the United States Supreme Court. Baugh discusses the briefs filed by the petitioners and the respondents as well as the amicus briefs. Before turning to the Court's opinion, she discusses the justices' records in previous school desegregation decisions. Baugh then delves into the Court's majority opinion, the concurrence, and the various dissents.

Chapter 7 concerns the aftermath of *Milliken*: reactions to the *Milliken* decision, the return of the case to the Supreme Court, and the implementation of the district court judge's order. Baugh explores the reactions of many different people to this decision—from the white suburbanites to the school authorities and elected officials and then members of the black community. The remedy is also examined in detail. In the plainest terms, the ultimate decision in *Milliken* was to desegregate the Detroit school system. How to do that, however, became another source of controversy and led to another trip to the Supreme Court. Baugh discusses that development in detail and then turns to the implementation of the busing order. She points out the difficulty in determining the exact impact of *Milliken*, which had a major effect of hindering efforts to desegregate urban schools in the North. The aftermath of *Milliken* is also examined with Baugh addressing the retreat from efforts to desegregate schools in the North to the Supreme Court's resegregation decisions in the 1990s. In her conclusion, Baugh notes that urban education is still in crisis and that conditions in the Detroit schools still need improvement. She closes with an important and thought provoking question: "is school integration still an important value in American society?" (p. 209).

Baugh does an excellent job of setting the foundation for the *Milliken* decision. She discusses the Detroit school system and the legal landscape in

regards to school desegregation as it existed both before and after *Milliken*. She does not focus on the legal aspects of the decision alone, but examines the social, economic, and political aspects as well. She demonstrates that legal decisions do not occur in a vacuum. Through the book, one can see why there was so much controversy over school desegregation and how that issue affected, for various reasons, so many people. One can also see the difficulties ingrained in a case of this magnitude from the legal perspective.

Baugh compiles a clear and concise history of the *Milliken* decision and all of the surrounding social and political issues that accompanied it. At the same time, she does an excellent job of explaining a complicated court case and the various decisions at the various court levels. This book would be valuable to anyone interested in learning more about school desegregation in the North including members of the public and undergraduate students.

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**Jason Brennan.** *The Ethics of Voting*. Princeton, NJ: Princeton University Press, 2011. 222 pp. (\$29.95 cloth, \$18.95 paper).

Empirical and normative political scholars—as well as citizens more generally—agree that voting is a basic duty of an individual living in a democracy. Jason Brennan challenges this most basic of American assumptions, making the controversial argument that there is not a duty and further, some individuals should explicitly *not* vote. And, vote buying, selling and trading are “morally acceptable.” The principal reasoning for his position has to do with the “common good” but this is not a book advocating particular public policy. Rather, it is a treatise questioning our understanding and basic assumptions about the franchise and especially to question what he calls “the folk theory of voting ethics.”

All in all, Brennan has written a thought-provoking, tightly sealed argument. There are a number of important points, justifying both deference to those who know more about politics (e.g., taking cues when voting), strategic voting, and the acceptability of vote buying and selling. I think that this book should cause even the most ardent of voters to question his or her own motives in voting. An especially impressive thing about this book is how it anticipates many of the arguments against the core thesis and answers them. However, in my opinion, the amorphous concept of the “common good” still presents a difficulty for this work, especially if one takes the idea of the common good to a logical conclusion.



Technically, Brennan is correct; his theory does not depend on what the common good is (p. 115), but I think it is important nonetheless. While he covers various philosophical conceptions of the common good and explains these conceptions are consistent with his thesis, when philosophy ends and practice takes over, there is a potential problem. My guess is that Westboro Baptist Church's Rev. Fred Phelps likely believes he is promoting the common good by picketing funerals of soldiers lost in our conflicts over the last decade, but there are many who disagree. Yet despite Phelps' reasoning, even the courts have recognized his freedom to do so. Perhaps Phelps is behaving in a way he believes is consistent with the common good, rather than his own narrow self-interest. Brennan may argue that Phelps should not engage in "collectively harmful" activities, yet "voters can be said to have voted well, despite having voted for what turned out to be harmful policies, only if they have a sufficient epistemic justification for their votes" (p. 70). It is hard to speculate about Phelps' state of mind, but he might believe he is advancing the interests of society because of his concerns for the hereafter.

Further, Brennan does devote a chapter to what he calls "wrongful voting." For example, expressive voting is wrong in his view because "On my view, [voters] should vote instrumentally or not vote at all. Or, more precisely, they may have whatever motive they please when the vote, but they are required to vote in ways that they justifiably believe will promote the common good" (p. 85). But the open question is who decides what is justified in reality?

And of course, we do know what the United States and other countries have justified restricting the franchise on the grounds of the common good. According to historian Alexander Keyssar, throughout our history, policy-makers have justified disenfranchisement of African Americans, women, the poor and those not meeting various property requirements (among others). One example from his *The Right to Vote: The Contested History of Democracy in the United States* (2000), is about justifying limiting franchise among African Americans, "At the New York convention in 1821, for example, a delegate opposed to black suffrage rather temperately had described blacks as 'a peculiar people, incapable, in my judgment of exercising that privilege with any sort of discretion, prudence, or independence'" (Keyssar, p. 55). Even today, there are reports of state legislators wanting to limit the franchise of college students under a justification that the students do not vote correctly.

Least the reader believe that Brennan is advocating that some individuals lose the franchise, he takes great care to note that he expects the bad voters to abstain, but it is their *choice*. "My position is not that the good voters should rule by right, or that the bad voters are by right forbidden from ruling. Rather, bad voters should exercise their equal right to rule in the way

that is most advantageous to themselves and others: by abstaining from politics” (p. 95). Brennan likens voting to going to another community where he might not know the restaurants and in going with a group, others *do* know the restaurants. While in a democracy, he should have equal say where the group eats, he defers simply because he does not know which restaurants are better. However, in my view, Brennan’s theory, taken to logical extremes, in order to promote the common good, society should *ensure* that only knowledgeable people vote. If voting badly is a negative externality (p. 5), then the government should step in to make the cost of bad voting equal with societal costs (e.g., a poll tax on “bad” or “wrongful” voters) or prevent them from voting at all (command and control regulations).

While Brennan does spend a chapter discussing some empirical realities that scholars have discovered about voters (e.g., the disgraceful lack of political knowledge), he also challenges political scientists to learn more. My read of his argument is, if empirical political scientists want to cling to the long-held assumption that voting is necessary and good, then we must provide evidence to support the good. In examining empirical realities, Brennan takes a pessimistic view of work suggesting that uninformed voters “vote correctly” noting that for such work, voting correctly only means voting in a manner consistent with one’s preferences. Brennan argues that “suppose a German voter in 1932 voted for the Nazis because he hoped they would exterminate Jews. This voter would count as voting correctly according to Lau and Redlawsk’s definition of ‘correct voting.’ However, this voter counts as a bad voter on my theory of voting ethics” (p. 167). Certainly citizens should be exceedingly wary of parties advocating extreme positions clearly harmful to fellow citizens, but not all situations are as clear cut as the Nazi example.

Does an individual vote matter to the outcome or to saving democracy (providing good governance or legitimacy)? Brennan presents a variety of arguments that show that indeed, any particular individual vote does not save democracy and certainly does not affect the outcome of the election. In fact, many voters engage in “wrongful voting” wherein “bad voters consume psychological goods at our collective expense” (p. 75). Individuals should probably help out in a soup kitchen rather than vote. Voting is a cheap way out of fulfilling one’s beneficent duty to society. This sort of “cheap altruism” does not promote public welfare. Perhaps, but many people have died for voters to have their say; personally I cling to my access to the franchise. As an empirical political scientist, I will continue to cling to the franchise of other citizens as well.

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**Kenneth N. Hansen and Tracey A. Skopek, eds.** *The New Politics of Indian Gaming: The Rise of Reservation Interest Groups*. Reno and Las Vegas: University of Nevada Press, 2011. x, 228 pp. (\$49.95 cloth).

With the passage of the 1988 Indian Gaming Regulatory Act (IGRA), the politics of Indian gaming changed dramatically. This book provides an extensive, in-depth analysis of that change, and describes in lucid detail how tribes adapted to their new-found relationship to state governments by engaging in a wide variety of lobbying and interest group activities. The authors make a compelling argument that tribes, forced to respond to the “sovereignty dilemma between tribal and state governments” (Hansen and Skopek, p. 3) that was imposed on them by IGRA, have adopted a sophisticated mix of strategies to affect state-level decision-making. Traditionally, tribes turned to federal courts for redress, but IGRA forced them to deal directly with state governments for approval of gaming compacts, arrived at through a process of “good faith” negotiation. The Act “delegated extraordinary authority to states” (Rand, p.164), and the negotiations are between “unequal parties” (Skopek and Hansen, p. 210). Desperate to exercise some control over state deliberations over gaming compacts, tribes have engaged in venue shopping, adopted both inside and outside strategies, and used their new-found gaming wealth to enhance their ability to participate in state-level politics. In effect, tribes simply started doing what their opponents were doing and engaged in the raw competition of influence politics.

In effect, tribes have learned that they must out-bully the bullies. They have competed in state political venues against opponents such as Las Vegas-sponsored lobbying in several states, card parlors in California, racetrack owners in New Mexico, Minnesota, and New York, and the interest groups funded by slime-meister Jack Abramoff. If this book were to be summarized in two words, it would be “money talks.” Tribes have learned they can increase the probability of success in gaining state approval of gaming compacts if they engage in “revenue-sharing” (i.e., buying off state and local opposition with a cut of the profits), making targeted campaign contributions, donating to interest groups that support their position, and hiring expensive lobbyists. In gaming politics, it appears that a lot of alleged moral opposition to gambling can be tempered by the judicious allocation of financial resources.

The tribes’ record of success is mixed, and the book describes some failures, in New York and New Mexico for example. But undoubtedly they would have suffered more defeats if they had not engaged heartily in the political process. Gaming revenue has increased tribes’ self-reliance, and their ability to provide social welfare benefits for tribal members, especially

at a time of inadequate and often declining social services from local, state, and federal sources.

What is not made clear in this book, perhaps by design, is the impact that gaming has had on tribal sovereignty. Chapter 3, by Joy Clay, Diane Wilde, and Rodney Stanley, argue that gaming has been a “catalyst for self-government” for Arizona tribes. However, other authors in the volume, including Steven Light and Kathryn Rand, make the argument that tribes have been forced to trade their sovereignty for approval of the coveted Class III compacts. The editors end the volume with this most important question: “Is gambling the path to self-governance and nation-building for Indigenous peoples, or in fact the erosion of it?” (Skopek and Hansen, p. 215). This book does not answer that question definitively (no one at this stage really can), but anyone attempting an answer it should read this book.

The scholarship in this book is of high quality, and the book covers many different aspects of the politics of Indian gaming, with a primary focus on campaign contributions and lobbying. However, it does not discuss the role of Indian voting in the political process. My only real criticism of the book is that it is almost four years out of date. It appears that the chapters were written primarily in 2006 and 2007, with a few references to 2008. There is a growing literature on Indian political participation, and political developments in this area are fast-moving and changes develop rapidly. It is unfortunate the editors did not ask the contributors to update their chapters prior to publication. Other than that, this is a well-written book and should be read by anyone with an interest in the welfare of American Indians and their quest to both protect their identity as separate peoples, yet also make a living in a world dominated by another culture.

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**Tom Glaze, with Ernie Dumas.** *Waiting for the Cemetery Vote: The Fight to Stop Election Fraud in Arkansas*. Fayetteville: University of Arkansas Press, 2011. xiii, 236 pp. (\$19.95 paper).

Tom Glaze’s *Waiting for the Cemetery Vote* is in the genre of political writing that features first-hand accounts of local political machine corruption, of which Lincoln Steffens’ *The Shame of the Cities* is perhaps the best known. Glaze’s story takes place in 1960s-1970s Arkansas, a state that has contributed a raft of characters to the violent and colorful political history of the U.S.—from Redemption-era disfranchisers Senator W.S. Hanna and Governor Simon Hughes, who in the 1880s armed a militia to keep blacks

and Republicans away from the ballot box in Conway County; to Sid McMath, a decorated WWII hero and leader of the "G.I. Revolt" of 1946 that overturned the corrupt McLaughlin machine at Hot Springs, winning every seat in Garland County; to the better known political figures Orval Faubus, Dale Bumpers, David Pryor and Bill Clinton. Glaze presents an animated account of modern-day reform efforts to root out pernicious election rigging in Arkansas, including a citizens' clean elections movement, of which he, as attorney to the local women who organized the grassroots campaign, was a leader.

Glaze describes in detail the methods used to routinely steal elections in Arkansas, and draws on his own activism from that period to provide hard numbers on ballot signatures forged and ballot boxes stuffed across a set of state and local elections. The principle forms of corruption involved poll tax receipt fraud perpetrated by "big farm owners and other estimable men of the community" (p. 41), and full-scale conspiracies among election officials and party workers who engaged the systematic abuse of the absentee ballot, ". . . the source of the most flagrant corruption," says Glaze (p. 84). He recounts the prolonged legislative effort in the state to outlaw the poll tax and bring greater transparency and ballot security to election administration. Important to the drama of the events are the concurrent efforts of the "Snoop Sisters," a band of local women out to outwit the vote thieves and bring their tyranny to an end, and a handful of courageous newsmen and county prosecutors who took on the powers-that-be to expose the soft underbelly of partisan corruption undermining local Arkansans' trust in government.

Thankfully missing in Glaze's tone is the moral sanctimoniousness of the anti-machine zealots and reformers of an earlier era. Glaze, who recently retired after serving 22 years as an associate justice of the Arkansas Supreme Court, is a wise and mellowed man, forgiving of human perfidy and imperfection. It is clear that he's maintained his passion for "honest and efficient" elections, and believes strongly that one fraudulent vote is one fraudulent vote too many. But he also writes with the humor and grace of a man who understands why the petty vote thieves of rural Arkansas did what they did and got away with it for so long. Yes, they usually personally benefited from the fraud—in jobs retained and future favors re-paid. At least some of them, however, also believed that what they were doing was in the public interest and best for their communities.

Glaze's description of the corruption permitted by the poll tax system points to an overlooked achievement of the Voting Rights Act (VRA). The VRA was the most important enfranchising reform of the 20th century, and much has been written about it from this perspective. As J. Morgan Kousser has pointed out, however, the VRA is also the most important anti-fraud electoral reform of the last century. The poll tax wasn't just anti-democratic.

Glaze describes another of its corrupting features—the easy abuse of the poll tax receipt. Elites could and did pay poll taxes for their workers or otherwise illegally obtain large batches of poll tax receipts from corrupt officials. They then used these receipts in conjunction with purloined absentee ballots to manufacture votes for their favored candidates.

Glaze does not pretend to be an historian or social scientist. As such, his brief historical account of the origins of fraud in the state is derivative of scholarly treatments (which he humbly acknowledges). And, I think caution is warranted in adopting Glaze's assessment of suspicious indicators of fraud. For example, Glaze says, "If you want to steal an election, the absentee box is the place to begin, and if you want to calculate the likelihood of fraud in a country, first figure the percentage of its total vote that is cast in absentia." (p. 39). But, legitimately cast absentee ballots are also the target of voter suppression shenanigans.

As a thoughtful memoirist, Glaze can only raise important questions for scholars. His story suggests a need for a more nuanced and historically-grounded understanding of how our thinking about what constitutes corruption changes, and for deeper insight into how tolerance for corruption becomes embedded in a culture.

In the end, what can scholars learn from Glaze?

I would highlight several points of inquiry his book stimulates for me. Glaze presents a sympathetic portrait of Winthrop Rockefeller, who Glaze believes genuinely cared about electoral corruption beyond what rooting it out would do for his own electoral fortunes. (Rockefeller, a wealthy liberal Republican, served two terms as mostly Democratic Arkansas' governor in the 1960s.) This raises a question about the role wealthy elites have played as activists and philanthropists in a variety of "public-regarding" "good government" movements. However offensive their older sense of noblesse oblige might be to a full-throated democracy, it sometimes functioned to improve the democratic process. What a contrast to today, when anyone would be hard pressed to identify a single leading industrialist or financier playing a similarly public-regarding role in any pro-democracy political reform movement.

Glaze's memoir also suggests that the obsession with polling place photo ID requirements is misguided and inefficient because it targets voters. The likely perpetrators of election fraud crime still are the people who have the motive—the maintenance of political power—and the means—the ability to engage in conspiracies to buy votes, stuff ballot boxes and manipulate the count—to rig elections.

Finally, Glaze's story shines a bright light on the need for more study of absentee balloting and its vulnerability to abuse. As states liberalize their absentee ballot laws in response to citizen demands for greater convenience,

we need more systematic analysis assessing the degree to which voting in absentia is still vulnerable to the kind of fraud once endemic in Arkansas and other states.

*Waiting for the Cemetery Vote* is a valuable book and a fun read, suitable as a primary document for analysis and discussion in undergraduate or graduate courses on American electoral politics or political corruption.

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**Ian McAllister.** *The Australian Voter: 50 Years of Change*. Sydney, Australia: UNSW Press, 2011. xv, 301 pp. (A\$49.95 paperback).

Australia's parliamentary democracy has sometimes been called a *Washminster* system because it has elements of the systems of government operating in both Washington and Westminster. Less derivative is Australia's electoral system—Australian jurisdictions first issued official ballot papers for voting in secret—what is still sometimes referred to in the U.S. as the “Australian ballot,” enfranchised all males early and soon afterwards enfranchised women (and allowed women to stand as candidates) and have experimented with different ballot structures, different counts, full or optional preferential voting, proportional representation, multi-member districts and diverse redistricting requirements. Australia has compulsory voter enrolment for citizens aged 18 or over, and compulsory turnout, and the major parties run candidates in every district.

Does all of that make Australians behave differently when we vote? Does it make the parties less relevant for Australians? (Or more?) Ian McAllister, Distinguished Professor of Political Science at the Australian National University in Canberra, argues that the parties are more important to the way Australians navigate from one election to the next, although he says that is changing. He covers that ground, and much more, in *The Australian Voter: 50 Years of Change*.

This book's name is no accident. Campbell, Converse, Miller, and Stokes' classic study, *The American Voter*, showed that voters formed an ongoing identification with a party and understood the political world through that orientation. That book's phenomenal impact prompted a myriad of follow-up works and also led to the development of a series of similar survey vehicles around the world, including in Australia. McAllister's book reports on almost 50 years of data from the Australian Election Surveys (AES), interpreted through the intellectual framework which emerged from *The American Voter*. In addition, McAllister puts the Australian material

into context internationally, drawing on results from related studies which form the Comparative Study of Electoral Systems (CSES). McAllister knows both collections well: he has run the AES since 1987, ran the CSES from 2004 to 2009 and now chairs the CSES Planning Committee.

So what does this book tell us? McAllister argues that in structuring Australia's electoral system the parties have also structured political behaviour. In particular McAllister argues that compulsory voting and a full preferential ballot have constrained voters' choices in ways that benefit the parties. At the same time, he does show that voters have found ways to express quite sophisticated intentions—for example, supporting a major party to govern in the lower house ballot and simultaneously imposing constraints on it by voting for a minor party in the upper house ballot.

Compulsory voting has been popular with voters throughout the period of the study. Roughly three in four voters support it—a majority of them supporting it strongly—and roughly 85 percent of voters say they would turn out even if it was not compulsory. Of those who do not support compulsory voting, only about one in three objects strongly. McAllister calls this support a puzzle, given the disregard of authority often shown by Australians, but my take is different. There is a strong utilitarian streak in Australia, and in a parliamentary system elections are about choosing governments as much as representatives, so my guess is that Australians see electing a government as an important task in which everyone should have a say.

The second chapter examines the public's changing views of, and identification with, the parties. McAllister says that by international standards party loyalty is high in Australia, though loyalty is weakening, and our children are less likely to adopt a party simply by following our example. For me, the interest is at the edges of this analysis. Roughly 15 percent of the survey population do not identify with a party, but that group is not immutable—it declined to a tiny 3 percent when the popular Hawke-Keating Labor government was in power. Now that the Greens have won their first federal seats and about 15 percent of the vote, have they converted these independents or won their support away from the major parties? And if, as the surveys indicate, the strength of party attachment is weakening, is that true also of those who become converted to newer parties?

Interestingly, McAllister shows that although voters are not as “rusted-on” as they were—those who have always voted for one party have declined from roughly 70 percent of all voters to about 50 percent—party identification does still determine our votes. The correlation between the two has been consistently high, never dropping below 0.80 and with a mean of 0.83.

By comparison with voters in other countries, Australians' level of satisfaction with democracy remains very high, although political knowledge is no higher. Chapters three and four look at respondents' views of politics



and of campaigns. Compulsory voting makes elections both easier and harder for the parties: they do not need to mobilise voters, but increasing their share of voters' support involves winning the support of people who rejected them last time. And in a system where the lower house majority leader forms the government, election campaigns focus on party and government, much more than on local representatives. McAllister shows that only about 35 to 40 percent of respondents declared "a good deal of interest" in elections but that proportion doubled when respondents were asked whether they cared a good deal about which party won the election. There were two peaks in these graphs—1993 when Labor retained government despite a pre-election sense of doom, and 2007 when Labor regained government after eleven years in opposition.

There are five more chapters, all of them interesting and useful. They look at the impact of the social bases of politics (inherited characteristics, religion and class), and of voters' own values and beliefs, and the increasing importance of perceptions of party leaders.

McAllister comes from a British intellectual tradition though he has lived in Australia for a long time and knows our system well. I disagree with him on a few matters of opinion—he sees the rules for filling in ballot papers as complex and I see them as straightforward. Given the overwhelming acceptance of compulsory voting he interprets the informal rate as evidence of confusion, needing a remedy; given the sophistication of split ticket voting I see the informal rate as small and in part a legitimate protest vote. Then too, he occasionally draws conclusions that seem bigger than the data support. But these are quibbles, not disqualifications. McAllister's strength is his intimate understanding of both the Australian Election Survey and the Comparative Study of Electoral Systems.

*The Australian Voter* won't set the world on fire like *The American Voter* did: it updates and refines, rather than reinterprets, our understanding of how Australian voters behave. But it has generated a lot of interest here, simply because it does bring us up to date. U.S. readers may be interested to see how the parties use Australia's electoral institutions to achieve partisan advantage, and how voters use the same electoral institutions to send complex messages of their own.

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**Ronald M. Peters, Jr., and Cindy Simon Rosenthal.** *Speaker Nancy Pelosi and the New American Politics*. New York: Oxford University Press, 2010. ix, 336 pp. (\$29.95 cloth).

Fifty-two individuals have become Speaker of the U.S. House of Representatives. Only one has been a woman. That fact not only indicates the Congress' relative backwardness compared with the Nordic national assemblies and the U.S. states in promoting women to leadership positions, but also the symbolic if not the substantive significance of Nancy Pelosi's election as Minority Leader in 2003 and Speaker four years later. Unquestionably, Pelosi was one of the most effective, most activist, and most progressive—and most polarizing—House speakers in U.S. history.

In this timely book, Ronald Peters, a leading scholar of the speakership, and Cindy Rosenthal, a leading authority on women's political leadership, provide descriptive analyses of Pelosi's political career, rise to power, and leadership style set in the context of "the new American politics." The authors note that Pelosi's rise to power and effectiveness stemmed from her uncommonly adept organizational ability combined with a prodigious ability to raise funds for the Democratic Party, as well as her appeal as a well-connected westerner and non-centrist activist. Despite her reputation as a San Francisco liberal, moreover, her conception of leadership was "operational rather than ideological" (p. 52). These personal characteristics and the political strategies Pelosi advocated, of abandoning her predecessor's centrist strategies for a more distinctive and aggressive partisan approach, these authors insist, were particularly suited to the changed social and political context of the 1990s and 2000s, the key elements of which were the increased political partisanship, organized fund-raising and big money, new communications technologies and the increased participation of women, Afro-Americans and Hispanics.

The central foci of this well-researched book are the descriptive analyses of Pelosi's leadership strategies as speaker in the last two years of the Bush presidency and the first year of the Obama administration, when her strategic position was significantly different, as well as a valuable chapter on Pelosi's electoral and messaging strategies. During the final six years of the Bush presidency, we see Pelosi—as minority and majority leader—concentrating power over committee assignments in her hands for the first time since "Uncle Joe" Cannon, flexing her muscles over the election of committee chairs, and replicating Republicans' imposition of term limits on committee chairs. Still, as the authors emphasize, reflecting her penchant for organization, interpersonal politics, and pragmatism, and in order to accommodate the greater diversity within the House Democratic Caucus, Pelosi also reached out to party centrists and conservatives through networking,

committee assignments, and by writing PAYGO into House rules. Her approach to floor agenda-setting combined both control and compromise: in common with recent Republican and Democratic speakers, she made heavy use of restrictive special rules and fewer conferences with the Senate to ensure passage of legislation supported by majority Democrats, but also allowed House Democrats more slack than her Republican predecessors in how they voted on motions to recommit. She also denied President Bush fast-track approval of the Colombia Free Trade Agreement.

As the 2008 elections and then the global financial crisis loomed, however, Pelosi was obliged to improvise her leadership. After unified party government returned in 2009, she adapted successfully to the new so-called “postpartisans” fielding policymaking initiatives from Obama’s White House, insisted on “governing from the center,” and adopted a somewhat more relaxed approach to leading her party, ending term limits on chairs, developing more relaxed relationships with committee leaders, and allowing the Republican minority further leeway on motions to recommit. The upshot was an exercise in effective party government, in which Pelosi played a very important part.

Few scholars will find much to challenge in the account the authors’ rich descriptive analyses provides. More problematic are some important theoretical problems that are glossed over or fudged. One problem concerns causality, and the relative primacy of structure and agency in analyzing congressional leadership. The authors are well aware of the theoretical debate, but do not engage with this debate in any depth. The authors use the formulation that their book will “seek to understand Nancy Pelosi, not just as a Speaker of the House, or even as its first female Speaker, but also as a reflection of a new era in American” (p. 4). In the subsequent analysis, however, they then do not explain persuasively how much of what happened under Pelosi’s leadership might be explained by her agency/effectiveness, and how much by the context of “the New American Politics.” At some points, Pelosi is given credit for the supposed positive effects of her internal House strategy in helping Democrats win in 2006 and 2008, but these election results might more appropriately be seen primarily as a referendum on President Bush and his party’s implosion on Iraq and a variety of other issues.

A second related problem is the importance of Pelosi’s gender, which as the authors note the former speaker has consistently played down. In many respects, Pelosi’s gender is this work’s *raison d’être*. Admittedly, the pulled punch at the beginning, that “each element of the New American Politics has a gendered dimension that we believe warrants investigation” (p. 15), hardly kindles much expectation. Still, notwithstanding their declared aim *not* to understand Pelosi as the House’s “first female Speaker,” the authors nevertheless raise readers’ expectations by including a considerable

amount of discussion on the significance of Pelosi's gender and the increased role of women in U.S. politics, and by noting how existing literature demonstrates that women leaders bring different skill-sets and policy concerns to the role compared with male leaders. Granted, the changed context provided Pelosi with the opportunities that would likely have been denied previous generations of women. However, as the authors finally concede in a heavily qualified conclusion, the extent to which her gender explains her style and aided her success is marginal, and begs the question whether a male westerner with her skill set and background would have exhibited a similar style and achieved the same effectiveness.

Much more persuasive is the book's final chapter, which seeks to place Pelosi's speakership style and achievements in the context of her predecessors. Although the authors demur from using it as a hook for launching into an analysis of the relative weight Pelosi's leadership should give to agency and structure, respectively, the authors make the telling observation that, notwithstanding Pelosi's disdain for Gingrich and Hastert, their speakerships most closely resembles hers. Unfortunately, they then go on to suggest that Pelosi's skills might prove better adapted to the environment of "the New American Politics than those of her predecessors Wright, Foley, Gingrich, and Hastert." In late 2009, and within the contemporary hyperpolarized congressional context, this judgment was always going to be premature at best; at worst, wide of the mark.

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**Paolo Spadoni.** *Failed Sanctions: Why the U.S. Embargo against Cuba Could Never Work.* Gainesville: University Press of Florida, 2010. 224 pp. (\$34.95 hardback).

This well-researched book provides an impressive argument as to why the United States economic embargo on Cuba has proven a failed policy. Transnationalism from "above" and "below" is identified as an exogenous factor helping to undermine the embargo. The author correctly declares that, in the beginning, the embargo was hampered by the fact that Cuba mostly traded with Soviet-bloc nations. It was not until the collapse of the Soviet Union that the true impact of the embargo could be accurately assessed. Acknowledging that United States-Cuba policy was mostly internally driven by active Cuban-American hard-liners, the book argues that the rationale behind the embargo of denying hard currency revenues to the island has faltered.

One main reason that the United States embargo has failed is the fact that it was unilateral, meaning that Cuba is able to freely trade with other nations. Even after the enactment of the Torricelli and Helms-Burton laws, which are mostly aimed at preventing other nations from trading with Cuba, the embargo has not been able to deny the Castro regime of its much needed hard currency. Transnational corporations continue to do business with and invest in Cuba, especially in the tourism and telecommunications sectors. A second reason for the wavering embargo is the exorbitant amount of money that Cuba receives through remittances. The author estimates that by 2002, "hard currency revenues to the Cuban government from family remittances (\$586 million) were greater than its profit from tourism activities (\$354 million), sugar (\$176), and nickel (\$84 million) exports" (p. 154).

The strong points of the book rests on two factors; namely, on the thoroughness of the research and the precision of the argument. The author is meticulous with data. Whether evaluating the breadth and scope of economic sanctions (p. 5), generally, or very exact figures pertaining to select companies (p. 162), the gathering and interpreting of information is excellent. The supporting data presented throughout the book are relevant and revealing. This takes me to the second point. Once the dots are in place, the author does a magnificent job connecting them. There is no overreaching in his suppositions; he goes where the data take him. A case in point is when he concludes that even though "Cuban authorities argue that foreign investment in Cuba is consolidating, available figures on the number of international economic associations (mostly joint ventures) show disappointing results" (p. 95). Another case in point is when he astutely explains that the Cuban government may be overstating the impact of the Helms-Burton law so that it can blame the embargo for the underperformance of its economy. The inferences made in the book are accurate and insightful. From an analytical vantage point, the book provides a valuable contribution to our understanding of the United States embargo on Cuba.

Unfortunately, the book also has some drawbacks. The main shortcoming stems from the fact that the book provides a study of only one case, therefore not really providing comparative analysis of embargos. It informs us about the United States embargo on Cuba, but falls short of being generalizable. Does transnationalism undermine both multilateral and unilateral sanctions? Does it undermine one type of sanction (multilateral or unilateral) more than the other? Does it undermine all unilateral sanctions? This research cannot enlighten our overall understanding of this phenomenon because of this limitation.

Another downside of the book is that it does not adequately touch upon the role of other states in helping undermine the embargo. Cuba has established significant trade flows with countries such as Venezuela and China

that surely contribute to the weakening of the United States embargo on the island. The author would be hard-pressed to attribute these trade relationships to transnationalism, especially in the case of Venezuela. The author briefly writes about Venezuela's relationship with Cuba, stating that "booming revenues under bilateral agreements with the government of Hugo Chavez have now converted Venezuela into Cuba's new economic lifeline and engine of growth" (p. 171). On the following page he continues describing the importance of this relationship, writing that "exports of professional services, primarily medical ones under special deals with Caracas, generated \$2.9 billion or about 39 percent of Cuba's total hard currency revenues in 2005. These figures emphasize not only the significance of Venezuela's role in the Cuban economy, but also allude to the fact that this relationship may be as much political as economic. Venezuela appears to be subsidizing the Cuban economy with foreign aid, similarly to how the Soviet Union had done prior to the 1990s. On its face, it appears to go beyond transnationalism and merits greater attention.

Overall, this study provides an exceptional and nuanced account of why the United States embargo on Cuba has proven to be a failed policy. The book offers a comprehensive and clear-cut analysis of how transnationalism in the form of trade, tourism and foreign investment has offset the United States effort to isolate the island nation. The book is essential reading for anyone interested in United States-Cuban relations, as it may well be the most definitive recent examination of the United States embargo on Cuba. What the book may not do is provide a better general understanding of how states effectively sanction one another; as such a goal can only be realized through broader comparative approaches.

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**Zoltan L. Hajnal and Taeku Lee.** *Why Americans Don't Join the Party*. Princeton, NJ: Princeton University Press, 2011. 330 pp. (\$75.00 cloth, \$27.95 paper).

Why do American citizens choose to identify themselves as non-partisans? A broad range of scholars have attempted to address the question of the growing number of citizens who either identify themselves as independents or who simply elect not to specify their partisan identification. Some would argue that non-partisanship, like party identification, is rooted in one's childhood and early adolescence (e.g., Campbell, Converse, Miller, and Stokes, *The American Voter*, 1960). Others maintain that non-partisans

are simply those citizens whose political points of view rest dead in the middle of partisan identifiers and thus do not clearly identify with the positions of either party (Downs, *An Economic Theory of Democracy*, 1957; Carsey and Layman, 2006); or that partisan non-identifiers are ill-informed of politics and do not really care enough to become engaged (Campbell et al. 1960; Layman Carsey and Horowitz 2006).

In *Why Americans Don't Join the Party*, Zoltan L. Hajnal and Taeku Lee take a different tact, focusing on how racial and ethnic definition explain in part differences in partisan-identification. The authors argue that information, identity, and ideology are the key ingredients to political party identification. Further, they maintain that "racial and ethnic definition and immigrant experience" explain why partisanship or non-partisanship vary among whites, African-Americans, and immigrant groups (p. 6). Immigrants and racial minorities, who do not fit comfortably within the profiles of the Democratic and Republican parties, will opt to stay neutral. The authors seek to understand "not only how Americans who fit neatly into the partisan structure choose an affiliation, but also how other Americans whose interests, ideologies, and identifies fit less well choose to align or not align with one of the two major parties (p. 7).

They focus on whites, African-Americans, and two immigrant-based groups—Asians and Latinos. Each of these groups, the authors maintain, respond to information, identity, and ideology in a distinct ways. The book relies on empirical analysis to support the argument that because the two main political parties in the United States fall short in recognizing and raising the political awareness of racial minority or ethnic citizens; or reaching out to better understand the convictions or attachments of racial and ethnic groups, citizens within those groups make the rational choice to not identify with either party.

Throughout the book Hajnal and Lee discuss two foundational approaches to the study of partisan identification and political participation: 1) *The American Voter's* argument that partisan identification is a product of social identification and socialization; and 2) the Downsian rational learning approach to political identification. However, as they rely on these approaches they also argue both must be adapted to account for the growing presence of immigrant and racial minorities in this country, and the increasing presence of citizens who claim they are political independents.

As the authors point out, popular approaches to partisan identification and political participation assume a lengthy socialization process that depends on longstanding knowledge and attachment to the American political system. This attachment brings with it information and knowledge of political parties and politics. Assumptions of lengthy engagement with political parties or the American political system fall short for some members of

immigrant groups. Foreign-born citizens and succeeding generations of Latino and Asian-American citizens have fewer opportunities to learn about and experience core values of American political parties. The authors argue that the lack of information does not necessarily apply to African-Americans and white citizens, but that some members of each group “stand apart” from the rest of America, living in extremely poor or isolated communities (pp. 81-82.) As is the case with immigrants, this isolation makes identifying with a political party an act of uncertainty.

The book devotes empirical chapters to each of the subgroups. In this review, I will briefly outline these arguments, beginning with the analysis of African-American citizens, then moving to immigrant groups, and finally, whites. It is not possible to engage a comprehensive discussion of each subgroup in a single review.

African-Americans in modern American politics are strongly identified with the Democratic Party. However, as the authors note, this Democratic self-identification has not always been the case; and also, recently there has been some evidence that African-Americans are slowly moving away from an exclusive relationship with the Democratic Party. Hajnal and Lee offer three expectations of African-American behavior. They argue a “liked-fate heuristic”—a perception the group members’ collective fates are somehow tied together—binds African-Americans to the Democratic Party. Also, they posit that perceived legitimacy of “mainstream politics” as a venue for the pursuit of collective interests, and rational evaluations of the two parties’ efforts on behalf of these collective interests, define African-American partisanship or lack of partisanship.

The authors use data from the National Black Election Study (1984-88), the National Black Politics Study (1993), and National Black Election Study (1996) to test a series of hypotheses. They conclude African-American partisan choices do not fit into a liberal-to-conservative continuum. Instead, race remains a primary driver in partisan self-identification. They maintain that “How African-Americans view American society and the degree to which they think their own well-being is tied to the fate of the larger black community underlies much of black partisan politics” (p. 143.)

The book identifies a different process of political identification for immigrants, arguing that for immigrant citizens there is a multi-stage process of identifying as a Republican, Democrat, Independent, or non-identifier. In the first stage, Latinos and Asians must decide whether to identify themselves in terms of America’s two-party system. Immigrants decide whether they have a sufficient understanding of American politics to comprehend partisan politics and to participate in the political process. The second stage, entered if immigrants feel they do have enough understanding, is to choose to be a Democrat, Republican, or Independent.



The study focusing on immigrant groups relies in large part on the 2006 Latino National Survey and the 2008 National Asian American Survey. Immigrants who indicate through answers to survey questions a lack of understanding or comfort with American partisan politics are more likely to say they have no party preference, or to offer a non-response or “not sure” answer to questions of partisan identification. Those who respond in a manner that reflects greater understanding of the American partisan system, are more likely to identify with one of the major parties or as an independent. As they express more trust in American institutions, they are more likely to identify with a political party.

White non-partisans, on the other hand, are generally characterized by a sense of ambivalence about politics or extremism—either conservative or liberal. In this instance, the authors challenge the conventional wisdom that independents are situated somewhere in between liberals and conservatives on the ideological spectrum. For citizens who have a mix of liberal and conservative views across issue areas, the ideal partisan placement is self-identification as an independent. Other white citizens may have more extreme views than either party. In instances, citizens with extreme views may compromise and identify with the party having views closest to their own. However, there also is a probability that those individuals will think of themselves as independents or non-partisans.

The authors use GSS and ANES data and find that white citizens identify as independents for a variety of reasons. These findings bring into question whether the traditional view of independents as ideological moderates is representative of all Americans or whether there are multiple dimensions of political independence. The authors conclude that the results perhaps cast a different light on assumptions that independents are somehow apolitical or apathetic.

*Why Americans Don't Join the Party* presents a compelling case that American citizens who are non-partisans or independents represent portions of the American population who do not fit very well—or do not believe they fit very well—into our two-party system. The core argument of the book is that there are multiple dimensions of partisanship. Partisan choices of racial and ethnic groups in America hinge on group ideological beliefs, group identities, and the information environments that determine how partisan cues are viewed or acknowledged as “relevant political considerations” (p. 276). When neither of the two major parties pays attention to the ideological profiles and identities of immigrants, racial minorities, and whites, there will be some citizens within those ethnic or racial groups who opt to remain politically neutral.

This book is a good resource for students of political behavior and worthy of inclusion in courses focusing on partisanship and political

participation. It makes a worthy contribution to our understanding of how citizens identify with political parties. Such an understanding is important to studies of participation, and also to candidates and political elites seeking a better understanding of the political identifications of racial minority and immigrant citizens.

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**Clark, Tom S.** *The Limits of Judicial Independence*. New York: Cambridge University Press, 2011. 336 pp. (\$90.00 cloth, \$29.99 paper).

The Supreme Court's exercise of judicial review power has long sparked criticisms that the Court is a counter-majoritarian institution that overturns actions of democratically elected actors without regards to the public's preferences. In this important study, Tom Clark challenges these criticisms by demonstrating that the Supreme Court responds to public opinion and exercises self-restraint when using judicial review and interpreting statutes. Clark argues that because the Supreme Court relies upon institutional legitimacy to exercise power it is responsive to public opinion, albeit indirectly, and will opt to follow the majority's will to protect its legitimacy. Using a multitude of methods, Clark empirically demonstrates that the Supreme Court is indirectly responsive to public opinion. Specifically, Clark argues that when Congress introduces "court-curbing" legislation, legislation that attempts to remove or lessen judicial power, it is signaling that the Supreme Court has lost political support. The Court then responds to this signal of declining support by opting not to exercise judicial review or by interpreting statutes more in-line with Congress' preferences than it would otherwise. In this sense, the Court is not responding to court-curbing legislation out of a fear that Congress will harm it; rather, it is responding to the legislation out of a fear that it will lose public support. The

introduction of court-curbing legislation is, therefore, an indirect manner in which the public exercises control over Supreme Court decisions.

The most tenuous claim in Clark's book is that the Court is responding to court-curbing legislation as a signal of public opinion; however, he does an exceptional job in systematically building a theoretical foundation and providing empirical support for this claim. First, Clark clearly demonstrates using both qualitative and quantitative methods that court-curbing bills are more a means of political posturing than a credible legislative threat against the Court. Citing interviews with legislative staff and examining the history of court-curbing legislation, he demonstrates the introduction of such bills is largely symbolic and intended to respond to a member's constituency. He then provides further quantitative evidence by showing that court-curbing bills are introduced in response to decreases in public support for the court, not in response to Court behavior. Next, Clark shows that as the amount of court-curbing legislation increases, it exercises self-restraint. Then Clark systematically demonstrates that when the Court responds to these bills it is doing so not out of fear of Congress but rather out of fear of losing political support. Using formal modeling, Clark lays out a series of testable empirical implications of the Court responding to court curbing as a signal of public opinion; he then tests those implications. Generally, Clark argues that as court-curbing activity increases the Supreme Court will exercise greater self-restraint, especially when 1) the activity is a credible signal of public opinion (namely when Congress and the bill introducers are ideologically proximate), and 2) the Court perceives itself as lacking public support.

The strongest evidence that Clark provides in support of his theory that court-curbing serves as a signal of public opinion rather than a reaction to Congress is the empirical evidence that the Court does not respond out of fear that Congress will actually pass the bill. Clark shows that the Court responds most strongly to curbing attempts made by ideological allies in Congress and far less strongly to those made by opponents (p. 182). When allies engage in court curbing it is less likely to be because they disagree with the Court's decisions and therefore actually want to reign in its power, thus it is a more credible signal of public opinion than congressional action. Contrastingly, when opponents introduce court-curbing bills it is more likely because they dislike the decisions and it is also more likely they are seeking to actually reduce the Court's power, thus it is a less credible signal of public opinion. Therefore, the increased responsiveness of the Court to the allies provides strong evidence that it is in fact reacting to public opinion rather than congressional dissatisfaction. Likewise, this finding that the Court's responsiveness to court-curbing bills is unaffected by the seriousness of the legislative attempt further supports Clark's theory (p. 180). He examines the effect of the number of bills receiving committing hearings or being reported

out of committee, both of which are indications that Congress is actually considering the bill, and finds that the Court is not responsive to fears that the bill will be enacted; rather, it is responsive to the fact that the bill was introduced. The fact that the Court is only reacting to the number of bill introduced not the viability of those bills is strong evidence that the Court is not actually reacting to Congress.

Though Clark makes a strong case and provides compelling empirical support that the Court is not exercising self-restraint out of fear that Congress will enact legislation, he still does not fully substantiate the claim that the Court is responding to court-curbing as a signal of public opinion. In addition to the anecdotal evidence (p. 79), since it is impossible to get direct evidence of how the Court interprets court-curbing, the book would benefit from further indirect evidence along the vein of that mentioned in the preceding paragraph. The argument would be stronger if the author was able to look more in-depth at the nature of the court-curbing legislation and tease out characteristics that would provide a more credible signal of public opinion to the Court and examine whether that had an increased effect on self-restraint.

The other potential weakness of this book lies in the testing of Clark's second condition on the effect of court-curbing legislation: the Court's own perception of public opinion. Clark postulates that the Court's self perception (pessimistic or optimistic) of public opinion affects its use of judicial review and statutory interpretation and conditions the Court's responsiveness to court-curbing; however, he measures optimism using GSS survey data on public support for the Court. This is problematic because GSS is not a measure of the Court's perception but is an actual measure of public opinion. Thus, the measure is demonstrating that public opinion has a separate and independent effect on judicial behavior. This begs the question of through what channel is public opinion having this separate effect, and what precisely is the Court responding to when it responds to court-curbing that is different than public opinion? What is not accurately addressed is how the Court's view of public opinion is shaping its behavior, unless of course the Court's perception is perfect, which Clark does not argue.

None of these issues harms nor undermines the vast contributions of this book to the discipline; rather, they serve as excellent jumping off points for future research. This book presents a very well developed theory of how and under what circumstances public opinion constrains the Supreme Court, which should serve as a framework for understanding the relationship between the Supreme Court and public opinion. The study then provides extremely compelling evidence that the Supreme Court responds to congressional court-curbing legislation and not out of fear of its enactment. Then, it makes a strong and empirically supported argument that court-curbing legis-

lation is serving one signal of public support for the Court and thus limiting judicial behavior. The remaining questions that beg to be answered are what other mechanisms serve as signals of public opinion to the Court and what are their relative effects. Answers to these questions will only further our understanding of how the Court is tempered by the public.

I believe that Clark's book will become a staple for judicial process graduate classes around the nation. Not only does it provide an excellent synthesis of the countermajoritarian dilemma and shed novel and important insight into the relationship between the public and the Supreme Court, but it is an exemplary example of the use of multiple methods and how they strengthen a study. Clark's use of formal modeling to derive his hypotheses and then his test using a mix of large-n analysis, case studies, and interviews should serve as a model for future research.

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**Susanne Sreedhar.** *Hobbes on Resistance: Defying the Leviathan*. Cambridge: Cambridge University Press, 2010. vii, 183 pp. (\$85.00 cloth).

Generations of readers have struggled with—or, in a great many cases, have studiously ignored—the large, obvious and embarrassing contradiction that seems to lie at the heart of Hobbes's political thought. How is it possible that Hobbes could argue for the absolute and apparently unlimited authority of the sovereign and, at the same time, insist on the inalienable right of subjects to disobey the sovereign in cases of self-defense? Prof. Sreedhar tackles the issue head-on. She proposes that Hobbes, suitably and faithfully reinterpreted, does not in fact contradict himself. In defending this position, she offers two principal arguments, one pertaining to the nature of the subject's right to self-defense, the other pertaining to the nature of sovereignty and obligation under the Hobbesian social contract. I myself believe each of her arguments to be incorrect, but I could well be wrong.

Sreedhar claims that, internal to the social contract, the right to self-defense is best understood in the light of what she calls "reasonable expectations." It is common to think that Hobbes presupposes a universal principle according to which it is conceptually and/or psychologically impossible to undertake an obligation not to resist death (p. 31), hence that the right to self-defense cannot be contracted away. Now Sreedhar shows that this standard account cannot be correct, since Hobbes also acknowledges what we all know, namely, that for certain people in certain circumstances death is not necessarily viewed as the greatest evil. In his own words (from *De*

*Cive*), sometimes “there are commands that I would rather be killed than perform.” But Sreedhar also claims that, contrary to appearances, Hobbes’s account of the right to self-defense actually accommodates this fact perfectly well. For “[e]ven if people can refrain from avoiding death and even do so on rare occasions, it could still be unreasonable to *expect* them to, or to hold them accountable for not doing so” (p. 37). According to such an account, avoiding death is a “powerful human urge.” To be sure, this urge can be overcome—we can, for example, knowingly risk our lives in pursuit of some other value—but it is “unreasonable” to expect us to do so. All things being equal, we should assume that people will indeed defend themselves when their lives are threatened, and will do so even when the threats are coming from the sovereign.

From this, Sreedhar attributes to Hobbes the following argument: if man has a strong natural urge to defend himself, then it is not unreasonable for him to do so; and if it is not unreasonable for him to defend himself, then when he does defend himself he does it justly and with right (p. 37). It’s not easy for me to understand how such an argument could work. If, for example, I have a very strong natural urge to kill you, this hardly means that it’s not unreasonable for me to do so and it certainly doesn’t mean that, ergo, I kill you justly and with right.

Sreedhar uses the argument to claim further that the Hobbesian contract cannot in fact ask the contractors to give up their right of self-defense. If it is unreasonable for me to expect that other contractors will actually not defend themselves when their lives are threatened by the sovereign—unreasonable because of the powerful natural impulse to avoid death—then it is unreasonable to ask me to agree to a contract that presupposes the very opposite. I cannot be party to a contractual provision that I know in advance is unlikely to be respected by the other contractors. Any such provision would be null and void. The problem with such an interpretation, I think, is that if one powerful natural impulse makes one kind of contractual provision unreasonable, hence untenable, then why is this not equally and identically true for a host of other powerful natural impulses, including all of those impulses—e.g., the impulse to kill my enemy or to steal my neighbor’s property—that make the state of nature a war of each against all and that require the creation of the social contract in the first place? Why, in other words, doesn’t the powerful urge/reasonable expectation theory invalidate virtually all important provisions of the Hobbesian covenant?

Of course, Hobbes argues that contractual provisions are valid and enforceable only where there is a sovereign that can compel obedience. In the absence of such a sovereign, the reasonable expectation of widespread non-compliance would render the contract null-and-void in all of its particulars. It is the active presence of the sovereign that makes it reasonable to

expect that contractors will comply. But why, then, doesn't the authoritative coercive power of the sovereign apply as much to expectations rooted in the powerful natural impulse to avoid death? This is not to suggest that there's no difference between (say) the powerful natural urge to steal and the powerful natural urge to avoid death. But it is to doubt that the reasonable expectations account is helpful in understanding exactly how they are different.

One might respond—and Sreedhar does respond—by suggesting that resistance in the interest of self-defense is distinctive because it “in no way detracts from the sovereign's power” (p. 63). If someone is hauled away to the gallows kicking and screaming, that's not much different from being hauled away willingly. But this seems to me wrong. It appears to ignore, among other things, the possibility of successful resistance. One would guess that successfully avoiding punishment—running away or escaping or otherwise going underground—might well be a blow to the sovereign's prestige, might constitute public evidence of the sovereign's weakness, and might provide an opportunity or an encouragement to continue a life of crime.

Sreedhar's second principal argument is that there is, in fact, no necessary contradiction between a Hobbesian right of self-defense, as she conceives it, and Hobbes's insistence on the absolute authority of the sovereign to be obeyed. The argument relies heavily and explicitly on the analysis of the concept of authority proposed in a rather different context by Joseph Raz. In thinking about our moral engagement with the world, Raz distinguishes first-order and second-order reasons. Whereas a first-order reason is a reason for doing something, a second-order reason is a reason for acting or not acting on a first-order reason. My desire for a piece of candy provides a first-order reason for eating the candy, and my desire to avoid having a cavity in my tooth provides a first-order reason for not eating the candy. Often I will simply weigh my various first-order reasons and then act on the basis of whichever seems stronger. But I may also have an “exclusionary second-order reason” not to act on that set of first-order reasons at all. According to Raz, authority—such as the authority of the state—is best understood precisely in terms of exclusionary second-order reasons. Authoritative directives provide second-order reasons for suspending or preempting an action that would be based on some set of first-order reasons. When I accept an exclusionary reason, I put aside my personal predilections—they are “excluded”—and adopt the authoritative directive of someone or something else.

Sreedhar claims that this model helps explain Hobbes's account of sovereignty and the right of self-defense. Specifically, the Hobbesian social contract provides the sovereign with Razian authority—the sovereign's demands count as exclusionary reasons—whereas the right of self-defense is a

“nonexcludable first-order reason” (p. 114). Thus, the covenant is “not . . . a surrender of judgment.” Rather, it is “an agreement to treat the sovereign’s commands as Razian authoritative directives,” and that agreement most emphatically does not apply in cases, such as avoiding death, where reasons for action “cannot be excluded on the basis of a further second-order reason.”

This seems to me problematic in two ways. To begin with, it’s hard to see how the account does anything other than change the terminology of the question at hand. The apparent conflict between an absolute right of self-defense and the absolute authority of the sovereign is now transformed into a conflict between a nonexcludable first-order reason to avoid death and a binding exclusionary second-order reason to obey the sovereign. Exactly why this second-order reason should trump most but not all first-order reasons remains unclear to me. We still seem to have a contradiction between two contradictory principles, each of which is regarded as absolutely obligatory. But second, I also doubt the particular distinction between first-order and second-order reasons upon which Sreedhar relies. Following Raz, Sreedhar wants to say that the “sovereign’s command offers reasons for action that hold regardless of what the command says,” hence that authoritative directives are “content-independent” (pp. 111, 114). But it’s hard to see why this should be so. After all, the decision not to decide among various first-order reasons because of some exclusionary second-order reason remains a decision. It is a choice, just as much as the choice among first-order reasons would be a choice. As such, it is presumably made with some substantive end, goal or value in mind. The end, goal or value may not be of the same exact kind as those contemplated by first-order reasons, but that hardly defines the second-order reason as content-independent. Certainly my complaint here is as much with Raz as it is with Sreedhar, and is hardly original with me. But the general point is, if anything, especially clear in the case of Hobbes himself. For the decision to authorize the actions of the sovereign—hence to acknowledge the absolutely binding nature of sovereign directives understood as exclusionary second-order reasons—has as its explicit goal, hence as its determinate content, the pursuit of peace in particular and the laws of nature in general. One accepts the absolute authority of the sovereign because of the desire for security, and that’s hardly a mere formalism. It seems, then, that the relationship between such authority and the equally absolute right of self-defense remains, *pace* Sreedhar, a serious problem.

I should say that a number of other, smaller issues raise concerns. For example, Sreedhar criticizes an account according to which disobedient Hobbesian subjects have in fact decided that the threat posed by the sovereign constitutes an abrogation of the original contract and constitutes, thereby, the reestablishment for those individuals of the state of nature. Sreedhar



rejects this approach by arguing that the Hobbesian right to resist is explicitly understood to be a right of “subjects,” hence is a right that presupposes the continuing authority of the original contract (p. 104). But surely there is nothing incoherent or even remotely odd about citizens *qua* citizens choosing to renounce their citizenship, provided they have good reasons for doing so. In a rather different vein, Sreedhar sometimes fails adequately to acknowledge that certain of her claims have been made elsewhere and in some detail—e.g., regarding the breadth of reasons that Hobbes explicitly enumerates as justifying resistance (pp. 58-75) or regarding the importance of sovereign directives that actually subvert the purposes of the original contract (pp. 119, 123). In a book notable for its scrupulous attention to scholarly precedent and careful argumentation, these strike me as odd lapses.

Obviously, I am not at all persuaded that Sreedhar’s central arguments are sound. Indeed, quite the contrary. But as suggested above, I could well be wrong. It’s possible that I have misunderstood or misinterpreted what she says. And the simple fact is that, my grave doubts notwithstanding, this is an intelligent, bracing and stimulating book. Its account of Hobbes is challenging and serious, and deserves a deeper consideration than I have been able to provide here. It would be interesting and, I would guess, rewarding to engage its author in conversation regarding the kinds of questions I have posed. While I truly believe the argument of the book to be fundamentally incorrect, that’s an initial, provisional judgment, and is perfectly consistent with my further judgment that this is also a book to be recommended with considerable enthusiasm.

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