

## Book Reviews

Richard L. Engstrom, Editor

**B. Dan Wood.** *The Myth of Presidential Representation*. Cambridge: Cambridge University Press, 2009. 240 pp. (\$85.00 cloth; \$25.99 paper).

In Barack Obama's first year in office he engaged in ambitious policy making in the areas of defense and security, energy and environment, economy and job creation, and, of course, health care reform. He did so while his public approval, according to the Gallop Poll, sank from a high near 70 percent in February to around 50 percent in December—ironically, nearly identical to Reagan's at the end of 1981. It is in the context of President Obama's roller coaster inaugural year that we should read B. Dan Wood's recent publication *The Myth of Presidential Representation*. This is an impressive effort of rigorous data collection culled to answer some important and timely questions about the presidency. Specifically, Wood tests two competing theories of presidential policy making, centrist theories and partisan theories, in order to answer the question "Who is the president's constituency?"

Wood's most important empirical contribution to our understanding of the presidency comes from linking issue-specific measures of public mood to issue-specific policy making by presidents. Rather than studying actual policy choices, Wood uses carefully coded presidential rhetoric—nearly 200,000 sentences spoken by presidents from 1945 to 2005—to ascertain the liberal or conservative slant to each president's stance on nine major issues. This fine-grained data collection, in line with other recent research on presidential campaigns (Buell and Sigelman 2008), allows Wood to move beyond aggregated data on the subject to a nuanced understanding of the president as a policy maker and the president as a representative of the public. Using this method, Wood can study not just in imprecise annual intervals, but in monthly or even daily intervals. This is a more realistic portrayal of the modern presidency and a notable contribution to the field.

Satisfying party zealots of all stripes, Wood uses these data to demonstrate that Reagan was the most consistently conservative president and Clinton the most liberal! More significant is the data analysis presented in Chapters 4, 5, and 6. Wood disentangles the general policy orientation of presidents from issue specific orientations. While for most policy issues (urban affairs, welfare, education, and health) the evidence shows that presidential policy choices are not linked with public mood, but for two (environment and race relations) interesting relationships emerge. During the 1970s,

for instance, the public grew increasingly liberal on the environment, as did presidential policy making. With that said, Wood ultimately dismisses the centrist argument that presidents respond to public mood in favor of the partisan approach where presidents seek to pull voters at the center to their more polarized partisan positions. This is consistent with our notions of a “permanent campaign” and aggressive communication campaigns run by the White House to persuade the public. Despite these efforts, Wood shows that persuasion rarely works as intended and most presidents fail to convince those at the center to change their minds.

What is missing from Wood’s analysis is a complex notion of the institutional dimensions of presidential policy making. After reading this book, one might believe that a president operates as a solitary political actor, not influenced by factors endogenous to their administration. Of course we know this is not true. Presidents appoint diverse cabinets, drawn from a variety of backgrounds and possessing varying loyalties and opinions. Cabinet members advise the president on the very issues that Wood examines here, but they are absent from the analysis. Moreover, agencies themselves vary greatly, not just in terms of the policy issues they confront, but also in the extent to which they possess a dominant ideological orientation. Recent research by Daniel Lewis (2008) and Anthony Bertelli et al. (2008) suggest that we can measure this orientation and that agency-orientation is linked to policy. Is there a relationship between agency-orientation and a shifting liberal or conservative stance of a president on particular issues? Do liberal presidents govern conservative agencies differently than liberal agencies? These influences are multi-directional and complex, but omitted from Wood’s examination of the presidency.

Another realm where this research could extend is beyond the highly salient, hot-button issues studied here. For analytical reasons, Wood opts for Stimson’s measures of the nine major components of public mood, since they remain important over time and permit comparisons across presidencies (Stimson 1991, 1999, 2004). This is a compelling analytical argument, but it may overlook other issues which are salient for shorter, but no less important, periods of time. The salience of space policy in the 1960s and terrorism in the 2000s are two such theoretical examples. Do issues such as these adhere to Wood’s findings about the nine major issues or does their fleeting nature lead to idiosyncratic treatment? Pursuing such an approach might permit future researchers to better link the study of the presidency to other theories of the policy process, such as triggering or focusing events, not present in Wood’s book (Birkland 1997, 1998). Non-salient issues, which dominate the work of many federal agencies, but are often overlooked by the public, are also absent from the book and could offer an interesting comparison to these findings.

Nevertheless, Wood offers a substantial contribution to scholarship on the presidency and an interesting companion to other efforts in 2009 to demythologize the presidency, such as Dana D. Nelson's *Bad for Democracy: How the Presidency Undermines the Power of the People* (2008), and Glen Healy's *The Cult of the Presidency: America's Dangerous Devotion to Executive Power* (2009). Wood's conclusions do not stray into the vitriol of these less scholarly works, but all three advance a common thesis that the traditional mythology of the American presidency is deeply flawed and should be rewritten.

### Works Cited

- Bertelli, A., et al. 2008. The Ideology of Federal Executives and Their Agencies. Paper presented at the Annual Meeting of the Midwest Political Science Association. Chicago, IL.
- Birkland, T.A. 1997. *After Disaster: Agenda Setting, Public Policy, and Focusing Events*. Washington, DC: Georgetown University Press.
- Birkland, T.A. 1998. "Focusing Events, Mobilization, and Agenda Setting." *Journal of Public Policy* 18(1):53-74.
- Buell, E.H., and L. Sigelman. 2008. *Attack Politics: Negativity in Presidential Campaigns*. Lawrence: University Press of Kansas.
- Healy, G. 2009. *The Cult of the Presidency: America's Dangerous Devotion to Executive Power*. Washington, DC: Cato Institute.
- Lewis, D.E. 2008. *The Politics of Presidential Appointments: Political Control and Bureaucratic Performance*. Princeton, NJ: Princeton University Press.
- Nelson, D.D. 2008. *Bad for Democracy: How the Presidency Undermines the Power of the People*. Minneapolis: University of Minnesota Press.
- Stimson, J.A. 1991. *Public Opinion in America: Moods, Cycles, and Swings*. Boulder, CO: Westview Press.
- Stimson, J.A. 1999. *Public Opinion in America: Moods, Cycles, and Swings*, 2d ed. Boulder, CO: Westview Press.
- Stimson, J.A. 2004. *Tides of Consent: How Public Opinion Shapes American Politics*. New York: Cambridge University Press.

Heath Brown  
Roanoke College

**Thernstrom, Abigail.** *Voting Rights—and Wrongs: The Elusive Quest for Racially Fair Elections*. Washington DC: AEI Press, 2009. xx, 316 pp. (\$20.00 paper.)

In *Voting Rights—and Wrongs*, Abigail Thernstrom reviews the history of the Voting Rights Act (VRA) in America and concludes that while the original incarnation of the law in the 1960s was both important and desperately needed, over time both Congress and the Supreme Court have changed

the law in such a way that it no longer serves the interests of minorities in America. Though I do not agree with many of Thernstrom's arguments about the Voting Rights Act, this book needs to be read and taken seriously by anyone interested in racial politics in America.

Chapter 1 reviews the history of the VRA and its evolution over time. Thernstrom praises the original design and intent of the Act—to protect the ballot of racial minorities. One major problem with her history of the Act however is that she bemoans the inclusion of Texas in 1975 for coverage under Section 5, claiming this was largely a power play by the Hispanic interest group MALDEF. While Texas may not have employed literacy tests to restrict ballot access to minorities as she claims, the state was not without its own history of racial discrimination at the ballot box. In 1923 the state passed a law that banned African-Americans from participating in Democratic Party primary elections, which, at the time, with the Democratic Party dominating in the South, was the critical election. This law was the centerpiece of the famous “white primary” cases that was struck down by the Supreme Court in the 1994 decision *Smith v. Allwright*. So including Texas as one of the covered jurisdictions, even with perfect hindsight, seems quite reasonable.

Chapters 2 and 3 review the changing interpretation of sections 5 and 2 of the VRA respectively. These chapters are a superb review of the different amendments made by Congress and the relevant decisions by the federal courts for the two most important sections of the law. Both of the chapters extensively review the evolution of these two important sections of the VRA, along with Thernstrom's commentary about when, where, and how the courts or Congress changed the law for the worse. This book is very well researched and provides some incredible illumination about the history of the VRA.

Chapter 4 is dedicated to the enforcement of Section 5. Thernstrom objects to the enforcement by the Justice Department in the 1980s and 1990s of this section in which the focus, she argues, turned to “max-black” redistricting plans where covered jurisdictions were more or less required to draw majority-minority districts close to, or at, the proportion of minorities in the state. While I do not disagree with her interpretation of the law that proportionality was not required, my brief response would be “what's wrong with proportionality?” If we are going to judge the fairness of an electoral system what other standard should we use if not proportions?

Chapter 5 is on redistricting cases, mainly *Shaw v. Reno*, that invoke the 14th amendment to the Constitution. The decision in *Shaw* basically forbids redistricting based solely on race. Race was still an allowable factor, but the map-makers could not “forget” about all the other parameters, like compactness, contiguity, etc., and draw districts predominantly based on race.

Chapter 6 is Thernstrom's evaluation of the current state of racial election law, in which she borrows the phrase "Serbian Bog," where the legal land appears to be solid, but in reality is a quagmire. Here she reviews the 2006 renewal of the VRA, several recent cases, and she addresses what might be expected in the decision of the *NAMUDNO* case. The book was released prior to the decision in the *NAMUDNO* case, and Thernstrom, along with many other court-watchers, expected a blockbuster decision affecting Section 5, perhaps even vacating the section altogether. This, as we now know, did not happen. I am not criticizing her for this as I was surprised by the Court's decision as well.

Thernstrom spends a great deal of time and effort arguing that the election of Barack Obama to the American Presidency as a clear signal of just how far we have come in terms of racial politics in America. I do not disagree that times have changed since the 1960s and the average white American's attitude toward racial minorities has improved, but I do not think that we are past, or even on the brink, of a post-racial America. Racism exists in America and while it seems inconceivable that in the absence of the Voting Rights Act any elected official would try to overtly prevent minorities from voting, that does not mean efforts cannot be made at the margin to try to minimize the political power of racial minorities.

The more general point underlying Thernstrom's argument is an important one—in a non-proportional electoral system, what, if anything, needs to be done about minority voters? Here I am talking about any numerical minority, not just racial minorities. This is not an easy problem to grapple with since it is not clear for which minority groups something ought to be done and what exactly that something ought to be. I think the case for African-Americans living in the U.S. is quite clear, but what about other numerical minorities, Green Party members, gays and lesbians, etc.?

I agree with Professor Thernstrom that the VRA changed over time from a law that protected the right to vote for racial minorities to a system that forced jurisdictions to change their electoral method, like abandoning at-large districts, and, through *Thornburg v. Gingles*, virtually guaranteeing some seats that are represented by minorities. But, as some of my own co-authored work has demonstrated, most minorities in Congress and state legislatures across the country come from majority-minority districts. What would happen to these districts if Section 2 were scuttled? It is hard to know. Moreover, while we have clearly made great progress as a country in terms of race relations, I think Thernstrom's desire to scrap section 5 and the pre-clearance that goes along with it is also premature.

Agree or disagree with Thernstrom's argument about the state of the Voting Rights Act, this book is a must read for anyone interested in racial politics in America. The argument is powerful and well developed. Abby

Thernstrom has been one of the leading scholars of racial politics in America and this book is yet another demonstration of her prominence in this area of scholarship.

Thomas L. Brunell  
*University of Texas at Dallas*

**Joseph F. Zimmerman.** *Congress: Facilitator of State Action.* Albany: State University of New York Press, 2010. 233 pp. (\$75 cloth).

Students and practitioners of U.S. federal relations are well aware of Congress' increasing utilization of mandates, sanctions, preemptions, conditions, regulations, and the like as means to influence policy and performance at the state and local level. For many, the dramatic increase in the use of such devices and the resulting cost of implementation and compliance incurred by state and local governments threatens to seriously upset the traditional balance of federal-state-local powers and relationships in the American federal system. For some, that balance has already been unalterably changed, and the clear "losers" are governments at the state and local level. Increasingly, such terms as "creeping conditionalism," and "coercive federalism," are used to describe the impact that such actions and activities have had, and are continuing to have, for American intergovernmental relations.

In that context, it is refreshing to find a serious scholarly work that views such actions in a much less threatening light. As the subtitle of this book makes clear, Joseph F. Zimmerman sees Congress—even when considering the sorts of conditions, mandates, and sanctions frequently attached to federal assistance programs—as a "facilitator of state action." In his words, "Criticisms by state and local government officers of unfunded federal mandates, removal of state regulatory authority, and conditions attached to grants-in-aid should not blind the reader to the fact that Congress plays a major role in facilitating the initiation of actions by state governments." (p. 154).

In exploring his thesis, Zimmerman devotes various chapters to such topics as congressional devolution of certain of its powers to the states, congressional actions designed to assist the states in their enforcement of criminal laws, financial assistance provided by Congress to state and local governments, congressional facilitation of interstate compacts and implementation of various uniform state laws, and various congressional preemption statutes that he views as "state friendly." In a particularly interesting concluding chapter, Zimmerman sets out a number of recommendations that in his view would enhance even greater the role of Congress as a facili-

tator of state and local governmental actions, and he posits elements of what he believes ought to be included in a new, more thorough, theory of American federalism and intergovernmental relations.

Zimmerman's chapter on "Devolution of Power" is an example of the impressive thoroughness, detail, and logical approach that he applies throughout the book. Here, Zimmerman first identifies three types of power devolution as "*legislative*" (the "first and most important"), "*executive*" (those allowing "inferior units to make executive decisions"), and "*administrative*" (those allowing "lower tier units to administer programs previously administered by the general government") (p. 19). Examining "legislative devolution," Zimmerman further categorizes such practices as "unrestricted" or "restricted" and proceeds within that framework to discuss such activities as "marine activities," "insurance regulation," "cable television," "citizens band radio," "firearms," "gambling," and the like. Further, he identifies and discusses seventeen congressional statutes and one presidential order devolving national powers to state governors not granted to them by their state constitutions or their own legislatures. In a final section in that chapter, he identifies and discusses various powers that Congress has devolved to state attorneys general and other state administrative agencies. He concludes this chapter by finding that the various congressional preemption statutes removing regulatory powers from states have "not destroyed the federal system, as an *imperium in imperio* system remains in effect with states possessing important reserved powers not subject to formal congressional preemption and constitutional and congressional provisions excluding other specified state powers from preemption" (p. 51).

As thorough and as well researched as this work is, one might quibble with a few technical features. While the information and data presented are up to date in almost every respect, there appear to be a few (almost certainly minor) lapses. Although the book carries a 2010 publication date, financial data for local governments, as well as information on total federal grants-in-aid, is shown only as recently as the year 2004. More recent events, such as the grants made available through the 2009 "stimulus" package would alter the data and perhaps conclusions drawn, but probably only temporarily. Similarly, when discussing "block grant" programs, the discussion terminates with those grant activities occurring in the Reagan presidency. Also, in spots, there appear to be some redundancies in the information provided. For example, on p. 13 we learn that that "There are four types of partial preemption statutes," and again on p. 137 we are told that "... partial preemption ... assumes four forms," and then in each instance Zimmerman proceeds with a discussion of the typology. On pp. 14, 135, and 159 information is repeated on the decade by decade pace of preemption actions, concluding in each instance that "609 preemption statutes [had been enacted] by 2009."

These, though, are only minor points, and in no way distract from the importance or the thoroughness of this work. As Zimmerman sums it up, “The evidence presented in this volume fills a lacuna in the literature and demonstrates that congressional devolution of powers to states and facilitation of state actions improves the functioning of the federal system by encouraging states to exercise their reserved powers more effectively” (p. 172).

This reviewer agrees with that statement completely. This is an important work. It sheds new light on U.S. intergovernmental relations, and it presents information and data in new and interesting ways. I am confident it will shift the discussion of American federal relations, and especially as that discussion relates to the role of Congress as a positive, rather than negative, “facilitator” of state and local action and activities.

Richard L. Cole  
*University of Texas at Arlington*

**Iwan Morgan.** *The Age of Deficits: Presidents and Unbalanced Budgets from Jimmy Carter to George W. Bush.* Lawrence: University Press of Kansas, 2009. xiv, 375 pp. (\$34.95 cloth).

Iwan Morgan’s timely and ambitious book undermines long-held assumptions that the President (any President) is or can be *the* responsible steward of the nation’s finances. Balancing historic scope with political depth, *The Age of Deficits* tackles the entire budgeting history of the United States (U.S.) while delving into the complex subtopics that animate budget dramas year-to-year and decade-to-decade: elections, parties, institutional developments, monetary and fiscal policy, world economic events, and the mysterious bond market. As the U.S. government borrows today around 40 cents of every dollar it spends, we must be clear-eyed about its multiple causes and false cures. Morgan is blunt: “the notion that the President was the embodiment of the public interest in fiscal matters had no basis in reality” (p. 11).

The first two chapters are brisk journeys through the fiscal and political development of U.S. budgeting and serve as fine primers to the uninitiated. As a historian, Morgan utilizes a rich variety of sources to show how and why budgets and deficit politics are driven by ever-changing, politically motivated assumptions about the “proper” role of the national government. This ground has been covered by others, as Morgan acknowledges, but is condensed and rearticulated here to show that no one party, institution, political actor, or elegant economic theory can claim an objective, organic lock on “good” fiscal policy. Yet the U.S.’s metronomic election cycle and



two-party system provide constant short-term checkpoints to assess blame and reward.

Along these lines, Chapter 3 on Jimmy Carter is especially fascinating. Morgan weaves brief descriptions of Carter's childhood and pre-presidential career into a multi-layered analysis of how this nuanced latter day Progressive failed to overcome numerous economic challenges in the late 1970s that lashed him from the left and right—exacerbated by the administration's poor political communication. After going through Carter's economic presidency with a fine-toothed comb, the story comes to a head: "the budgetary politics of 1980 were an unmitigated disaster that contributed significantly to his election defeat by Ronald Reagan" (p. 66). Morgan takes the reader through the world economy of that year, as well as the extraordinary intra-party politics that kept Carter in the cross-hairs of liberal Democrats (most obviously Senator Ted Kennedy) whose base had no patience for austerity budgets. Morgan faults Carter on being politically tone deaf both in the choices he made and how he framed them, acknowledging that much of the economic crisis was out of his control. Although it may qualify as a political tragedy that challenger Ronald Reagan hammered Carter for deficit spending, Morgan argues Carter was too rigid in his adherence to a self-imposed balanced budget agenda that did not differentiate between cyclical and structural deficits. In addition, Carter's meat-and-potatoes communications lacked the rhetorical use of optimism that later served Reagan and Bill Clinton so well—even while delivering bad news.

So was Dick Cheney correct that Reagan proved that deficits do not matter—economically or politically? Morgan plumbs Reaganomics from the ideals of supply side economics to rosy budgeting scenarios to the political pragmatism that ultimately drove Reagan to deal with Congress on tax reform. The answer is: it is complicated. Chapter 4 shows that economic historians are not of one mind on the short- and long-term results of the skyrocketing deficit of the 1980s. Yet this chapter exposes the painful beginnings of our national confusion on where, when, and whom to blame for budget imbalances. Party politics run wild within and between Democrats and Republicans in the 1980s. Morgan describes how Democrats were left dumbfounded by deficit politics in the 1980s that "had the dual effect of funding their adversary's agenda and constraining theirs" (p. 120). Republicans also began decades of soul-searching on the meaning of deficits for party ideology; even David Stockman was fed up by the mid-80s and denounced deficits as "the irrationality that had descended on the nation" (p. 109). But unlike Carter, Reagan quickly pared balanced budgets from his original agenda and apparently did not look back. Along the way, the budget process itself became unhinged from reality and thus a target of "reform" from Gramm-Rudman-Hollings to the balanced budget constitutional

amendment movement. In other words, elites and their loyal voters made the mess of spending too much and taxing too little, so let's bring in the robots!

And then George H.W. Bush goes ahead and spills his own political blood in what Morgan describes persuasively as a genuine concern for deficit reduction. In analyzing the pre- and post-“read my lips” presidency, this chapter, like the others, must trot down familiar terrain. However, Morgan integrates a variety of political and economic information to bring it all to life anew from multiple political perspectives. Picking up where the other story lines leave off, Chapter 5 emphasizes continuing ideological and institutional developments with the fresh challenge of another worldwide downturn. Morgan again layers this larger perspective with the micro-level (but equally important) dust-ups within and between congressional party and committee leaders, administration officials, and other interested parties. One such example is a revealing snit between President Bush and then-RNCC co-chair Ed Rollins, who openly encouraged Republican candidates to oppose the famous hard-earned tax-spend-process reform compromise called the Omnibus Budget Reconciliation Act of 1990. Morgan grants Bush much credit for his contributions to the fiscal recovery enjoyed by his successor.

While Bill Clinton borrowed heavily from Reagan's communication strategies and picks up the mantle of hard political choices from Bush, Morgan argues in Chapter 6 that Clinton's economic priorities were at their core a successful version of Carter's moderate (and even conservative) ideology: market-driven prosperity, priority of inflation taming over unemployment, economic management through monetary policy, avoidance of dramatic short-term stimulus, and acute sensitivity to the bond market's skittishness about deficits. But the roaring 1990s did not lift all boats and Morgan reminds us that Clinton's broader economic successes masked growing income disparities. As for long-term economic growth, Morgan, like many others, blames the two bubbles that burst over the next decade on Greenspan's easy credit policies and Clinton's Wall Street deregulation agenda.

The final chapter on George W. Bush's poor economic leadership and epilogue on Barack Obama's staggering challenges speak for themselves. As the boom of the 1990s evaporated, Morgan argues that no one bothered to educate average Americans about how and why these extremes occurred. Of course, as a nation we simply put our angry heads in the sand. Since the 1980s, Americans have become addicted to unrealistic expectations: low taxes, high spending, and balanced budgets and presidents more than anyone feed us these impossibilities. Because I agree with Morgan's points, I have to raise a quibble. The Congressional Budget and Impoundment Control Act of 1974 deserves more attention because the history and results of this important (if not pivotal) moment in institutional history helps his argument in

many ways. A related criticism is that Morgan does not explicitly introduce wholly new theoretic or research models to account for the multilateral political and economic variables he describes. However, the absence of scholarly back-and-forth may make the book more accessible and interesting. *The Age of Deficits* is a lively and engaging discussion of the limits of representative democracy and, thus, more than a little depressing.

Jasmine Farrier  
University of Louisville

**Frances Fox Piven, Lorraine C. Minnite, and Margaret Groarke.** *Keeping the Black Vote Down: Race and Demobilization of American Voters.* New York: The New Press, 2008. xviii, 282 pp. (\$26.95 cloth).

Political campaigns illuminate the virtue and vice of democracy, moments when citizens witness the best and the worst elements of party competition. How do political parties *actually* win elections in contemporary America: by mobilizing more pro-party voters than can the opposition, by demobilizing supporters of the opposition, or by employing both strategies? How do election administration procedures enable parties to employ either or both strategies? What role, if any, does racism and subordination of blacks play in either election strategy? In this important work, Frances Fox Piven, Lorraine C. Minnite, and Margaret Groarke analyze federal data, reports from civic organizations, interviews, and controversies surrounding particular elections to proffer one unifying answer: racism and partisan control over election administration impacts how parties compete, a nexus advantaging voter demobilization, rather than mobilization, as the preferred option for winning elections. This nexus was predictable. The systems of voter registration and voter verification are “inherently susceptible to political manipulation” (p. 99), and black electoral mobilization remains unsettling to Democratic and Republican Party powerbrokers because it destabilizes electorate and officeholder coalitions. Parties win by *reversing* the direction of influence: politicians shape the voter universe instead of voters choosing their leaders. In short, politicians shape electoral outcomes by exploiting rules governing who registers to vote, how registration occurs, who shows up to vote, who is allowed to vote, how individuals cast their votes, and, finally, when and how votes are counted. Scholarly and public fascination with voter turnout and election results, therefore, is a myopic focus on outcomes when the real story is the *processes* that shape outcomes. This fascination neglects the noxious mix of racism, partisanship, and bureaucratic power enabling candidates to reap fruit (i.e., elected office) from a poisoned tree (i.e., election administration apparatuses).

Piven, Minnite, and Groarke reveal the primacy of voter demobilization by rebuking two analytical blind spots in treatments of party behavior. They criticize the “celebratory” proposition that “political parties competing for majorities necessarily work to mobilize new voters,” and fault disregard for “systematic and enduring vote suppression” (p. 15). This rebuke highlights an interwoven problem: academics laud parties as essential to democracy, extol the virtues of mobilization and party response to issues, and academics legitimize outcomes as evidence that *voters* have spoken. Yet, the “logic” of electoral competition works against itself upon closer inspection: “new voters are not reliable, nor are they reliably loyal to party, which makes it inefficient to pump resources into mobilizing them. So there is nothing inevitable in the logic of party competition that drives the parties to expand the electorate” (p. 16). Parties want to minimize uncertainty at the polls, and targeting “potential opposition voters whose discordant cultural identities and contentious political demands make them easier to isolate” (p. 17) is the most efficient way to ensure that *particular voters* speak. And, the most “discordant” segment of America has been blacks—as enslaved subjects, enfranchised Republicans, civil rights activists, beneficiaries of new federal oversight into state affairs, dethroners of white Democratic party machines, and as a voting bloc aspiring to influence or occupy elected office at every level. The virtue of electoral mobilization feeds off the vices of racism and ambition.

For the authors, “voter demobilization is often the more attractive strategy to campaign operatives because the consequences are more readily manageable” (p. 16). First, despite the racially disproportionate impact of a procedure, defenders can claim it was race-neutral and compliant with federal law. For example, the purging of peripheral or “inactive” voters and of so-called “felons” from voter rolls can be justified as compliance with the National Voter Registration Act (NVRA) or “Motor Voter” law. That the NVRA “permits deletions from the rolls in eight states that did not formerly utilize the nonvoting purge” (p. 180), and that the U.S. Election Assistance Commission’s (EAC) 2005–2006 report to Congress documented “blatant state violations” (p. 185) of NVRA speaks to the allure of “voter disenfranchisement by bureaucracy” (p. 137). Second, parties can orchestrate bureaucratic failure. Failure to register citizens—e.g., “zero registrations in public-assistance agencies and disability agencies” (p. 185) reported by some states in the report—its failure to process registrations, or its failure to perform due diligence in ensuring that only actual felons were purged, can be made to *look* coincidental. Third, parties can make bureaucratic success appear non-discriminatory and essential to protecting “ballot integrity.” Take, for instance, the 2004 directive by Republican Ohio Secretary of State Kenneth Blackwell to discount provisional ballots cast in the wrong voting precinct—

popularly caricatured as “right church, wrong pew.” Alternatively, the Department of Justice’s 1984-1985 precedent changing indictment of black voter activists in Alabama’s Black Belt for “voting fraud” associated with twenty-seven absentee ballots—a biracial jury acquitted the activists but taxpayers spent \$1 million. Or, the ferocity of “voter caging”—where registered persons not reached by nonforwardable mail are flagged to have their credentials challenged by poll watchers—as a misuse of the Help America Vote Act’s (HAVA) mandate to computerize statewide registration databases and NVRA’s mandate to cleanse ineligible voters.

This book is essential reading. Despite revisiting known incidents of malfeasance, the book addresses many lesser-known incidents and emerging issues (e.g., state requests to the EAC to change the federal voter registration form to reflect localism, like Arizona’s law requiring proof of citizenship or Colorado’s law requiring receipts for registrants documenting the unique identification number of the organization engaging in voter registration). Moreover, while the authors’ strident tone may offend casual readers, sophisticated observers will note its holistic perspective on history. Its analysis of the *pro-Republican* activities of the George W. Bush Administration adds dimension to its analysis of the *pro-white Democrat* activities by Democratic Party machines. The Democrats also employed purges, caging, and intimidation to stymie black political empowerment in big cities (e.g., in post-1965 Gary, Cleveland, and Chicago), and later the Democratic Leadership Council distanced the party from black interests and from registering low-income and minority citizens. The Epilogue explains why “We are all victims” of policies producing and produced by vote suppression.

Tyson King-Meadows  
*University of Maryland Baltimore County*

**Christopher Gelpi, Peter Feaver, and Jason Reifler.** *Paying the Human Costs of War*. Princeton, NJ: Princeton University Press, 2009. 280 pp. (\$65.00 cloth; \$26.95 paper).

In 2009, over 315 American soldiers lost their lives in Afghanistan, a number higher than the casualty total for the first five years of the conflict combined. What effect would these deaths have on Americans’ willingness to stay the course militarily? The conventional wisdom of a casualty phobic public suggests that popular support for war erodes quickly in the face of even modest numbers of casualties. In *Paying the Human Costs of War*, Christopher Gelpi, Peter Feaver, and Jason Reifler offer a more nuanced prediction. Public support for war, they argue, does not inexorably decline in

the wake of combat deaths. Instead, the authors identify two variables posited to drive both cross-sectional variance in support for war across individuals and changes in war support over time. The first captures individuals' retrospective evaluations of the initial decision to go to war. The second, which the authors accord the greatest emphasis, comprises individuals' prospective assessment of the mission's likelihood of success.

The authors situate their arguments within the growing literature on the forces driving public support for war and show how their work both complements existing research and breaks new theoretical ground. To test their claims, the authors marshal a truly impressive array of empirical evidence. A series of historical case studies of military ventures from the Korean War to the conflict in Kosovo draws on a wealth of aggregate and individual level survey data to demonstrate that casualties alone cannot explain patterns in war support. Rather, in many of the cases, changing assessments of the prospects for mission success correlate strongly with shifts in public support. Complementing these historical correlations, a series of original survey experiments shows that assessments of likely success or failure are among the strongest predictors of a respondent's support for a range of hypothetical military missions. In a particularly interesting experiment, the authors test for the influence of both expected success and expected casualties on war support simultaneously. Respondents are randomly assigned to different treatment groups that are then given different assessments by the Joint Chiefs of Staff (JCS) with respect to the probability of success of the proposed mission and the number of American casualties it is likely to entail. Both forces have a significant influence on support for the use of force; however, strongly consistent with theory, the experiment finds that public support for a mission remains high, even when respondents are informed that the operation would result in hundreds of casualties, provided that the JCS expresses even modest levels of confidence that the mission will succeed. Finally, the analysis concludes with an empirically-rich and intensive case study of support for the Iraq War and changes in it over time.

While it is possible to argue with some of the book's specific findings and interpretations, taken as a whole the evidence is compelling that both retrospective and prospective evaluations of a conflict shape public support for war, and that prospective judgments are particularly important. These represent significant contributions to our understanding of the dynamics driving public support for war and casualty tolerance. Nevertheless, an important question remains: are these retrospective and prospective policy assessments the ultimate causes of variance in popular support for war? Or are they intermediate causal variables that are themselves the product of other forces, including domestic and international elite cues, partisanship and other factors included as controls in the statistical models? When inter-

preting the regression results, the authors emphasize that the estimated effects for retrospective and prospective policy judgments are higher than those for other variables in the model. Yet, they also acknowledge that these other factors may have important indirect effects through their influence on Americans' beliefs about a war's justification and its prospects for success. Indeed, the authors' analysis of holes in the public's factual knowledge during the Iraq War strongly suggests that most popular policy judgments on both of these dimensions are unlikely to be based on a complete and accurate understanding of conditions on the ground alone.

The book begins to tackle this difficult question in the penultimate chapter; future research is needed to explore it in more detail. For example, to assess the influence of domestic political elites on respondents' evaluations of the prospects for success in Iraq, the model includes a dummy variable capturing whether respondents believed there was a bipartisan consensus supporting the war. Much of the wartime opinion literature (e.g., work by Brody and Larson) does focus on a simple dichotomy between elite consensus versus dissension. However, a number of recent studies (e.g., work by Zaller, Baum and Groeling, Howell and Kriner) have explored a more diverse set of pathways through which elites might influence popular beliefs concerning war. For example, Republican partisans may care little about the opinions of Democratic elites on the Iraq War. As long as key GOP opinion leaders stand behind the president and his policies, these identifiers may continue both to believe in the likelihood of eventual success and to support the war. In a similar vein, once key Democrats like John Murtha begin raising questions about whether the U.S. could prevail in Iraq, Democratic partisans might update their beliefs about the prospects for success and their support for the war, regardless of whether or not Republican elites shared these concerns. Expanding the model to account for these and other possibilities would yield a more complete picture of elite influence over perceived prospects for success. The analysis does explore one additional pathway of elite influence: President Bush's rhetorical efforts—efforts that were shaped in part by the authors' own research—to bolster public confidence in the mission's success and, in turn, support for the war. However, the results of these appeals were mixed (see pp. 232-233). Presidential capacity to lead mass opinion by reassuring the public of a mission's ultimate success appears limited both by the unfolding of events on the ground and by the response of other elites in the domestic and international environment. More research is needed to untangle these dynamics further.

The book also highlights the need for future research to explore both the direct and indirect pathways through which partisanship influences both citizens' retrospective and prospective policy assessments. While the analysis in chapter 7 finds a modest effect for partisan identification on retrospec-

tive war judgments, it finds no influence for party identification on a respondent's assessment of the war's prospects for success. As a result, in stark contrast to research by Gary Jacobson, Adam Berinsky, and others documenting stark partisan cleavages on multiple dimensions of Iraq War opinion, Gelpi, Feaver, and Reifler conclude that beliefs about success in Iraq are largely independent of partisan forces (pp. 218-219). However, there are reasons to suspect that this null result could be an artifact of model specification. Many of the variables in the success model are likely highly correlated with partisanship. One example is the measure of respondents' confidence in the Bush and Iraqi governments, which one suspects could be very highly correlated with partisanship. Such modeling choices might obscure strong relationships between party and judgments of success. Consider the striking partisan splits from a March 2006 Gallup poll that explicitly asked respondents for their assessment of the prospects for American military victory in Iraq. Whereas more than three quarters of Republicans believed that eventual American victory was at least likely, less than a third of Democrats shared this assessment. To be sure, the influence of partisanship on war support may be both direct and indirect; however, more attention must be paid to the varied pathways through which partisan forces shape both Americans' beliefs concerning the prospects for success in Iraq and their willingness to support the war, even in the face of continued casualties.

*Paying the Human Costs of War* raises new puzzles and questions even as it moves the scholarly debate forward. Gelpi, Feaver and Reifler make a significant contribution to the literature on the factors driving popular support for war and suggest a clear agenda for future research. Their work is of immediate interest to scholars in both American politics and international relations interested in foreign policy and the dynamics driving public opinion and war. The book is a strong addition to any graduate seminar on war and public opinion; moreover, the clear presentation of the theory and discussion of the empirical results also make it accessible to advanced undergraduates.

Douglas L. Kriner  
Boston University

**Kyle Scott.** *The Price of Politics: Lessons From Kelo v. City of New London.* Lanham, MD: Lexington Books, 2010. xvii, 160 pp. (\$65.00 cloth, \$26.95 paper).

Within the broader context of modern federal Takings Clause jurisprudence, there is no doubt that *Kelo et al. v. City of New London, CT, et al.*,



545 U.S. 469, 125 S.Ct. 2655 (2005) (hereinafter *Kelo*, not summarized for editorial space considerations) is an important case. It has certainly received its due in terms of the attention it has received by scholars, media, journalists and pundits since the U.S. Supreme Court issued its 5-4 majority and multiple dissenting opinions. *The Price of Politics* is a comparatively brief title within the body of commentary and other published work post-*Kelo* that argues, in a nutshell, “the Court’s decision was wrong. The Court’s reading of the Constitution and the nation’s legal heritage was flawed. The majority . . . suffers from a faulty jurisprudence. The primary consideration [in *Kelo*] . . . was the social effect of the property seizure.” (p. xii).

Unfortunately, one may detect a whiff of trouble in the author’s theme even in these introductory remarks; nowhere in a critical reading of the specifically *legal* issue before the *Kelo* court is there credible evidence that “social effect” had anything to do with the outcome. (The word “social,” in fact, appears only three times in the majority opinion, always in citation of earlier cases.) More troubling as the introductory theme is developed is the author’s explanation of the positions and assumptions regarding legal-constitutional analysis that will permeate the rest of the book. He says, for example, that his “methodology for Constitutional interpretation does not require an extensive treatment of precedent or legislative action. . . . When interpreting the Constitution, we should first understand for ourselves what the Constitution says and then look at the cases to see if they are consistent or inconsistent with the Constitution. . . . Cases should not cloud our vision of the Constitution. We should look to the Constitution for its meaning, rather than to the Court” (p. xiv).

Notwithstanding the elimination of the fundamental principle of common law that has guided its development since the Norman conquests of England, to say nothing of the evisceration of over two hundred years’ worth of specifically *American* jurisprudence dating from *Marbury v. Madison* (and perhaps the wholesale striking of Article III of the Constitution, which then becomes arguably superfluous as well)—Scott begins digging the first of a series of logical and jurisprudential holes in these early passages of the book from which he ultimately cannot escape. Billed by the publisher as a work of Constitutional Law and Legal Theory, its first two brief chapters start out as an aggressively presumptuous treatise on the latter which selectively and unquestioningly quote from Coke, Blackstone and Locke to support the author’s conclusion. This is not to suggest that Scott has the fundamentals of real property theory from such jurisprudential luminaries wrong as he expounds upon them, but applying theory to the empirical world is always problematic. The book’s remarkably unquestioning assumption of the correctness of its thesis and the lack of seriousness with respect to competing perspectives that we know were part of the “tough questions” before

and during the Constitutional Convention become more apparent in Chapter 3, entitled “Pre-Constitution America,” and again in Chapter 4, “The Constitution’s View of Property.” Still, even up to this point, at least the reader is able to discern a clearly normative theme to the work. In agreement or disagreement with this basic but questionable argument, a reader is nevertheless apt to note that the relevance of *Kelo* seems to have faded since the introductory remarks about it.

It is in the shifting of gears from this base that *The Price of Politics* develops its worst identity crisis in terms of what it wishes to do or represent. Chapter 5 (“Examining the Decisions”) attempts to return to a legal-doctrinal analysis of property rights under the Constitution, finally and thankfully by revisiting *Kelo*, but the reader is reminded as this exposition unfolds that we have already decided that courts and cases are “unimportant” to the Constitution; what matters is what the Constitution *itself* says—even for all its critically important vagaries. As for *Kelo*, it becomes for Scott a case in which the Court allowed “states to interpret for themselves what a particular part of the Constitution means [and] the Court has committed itself to an understanding. . . of property rights. . . that does not recognize them as unalienable or natural, but as things which may be defined and redefined according to the political disposition of an actor” (p. 114). To a first year law student, this statement would be an uninformed reading of *Kelo* at best and patently wrong at worst. One begs the text to provide more of an explanation of such an absolute statement—but as in so many parts of this book, such an explanation never comes. The assumption is to be taken *res ipsa loquitur* as correct.

Chapter 6 (“State Reactions to *Kelo* . . .”) shifts gears even more profoundly, going into an ill-advised quantitative focus to try answering questions that are by this point superfluous to the doctrinal attack presented earlier in the book—for example, “What Factors Influence a State’s Decision to Protect Property Rights?” (p. 127) and “Factors Influencing the Rapidity of Adoption” (p. 129). Notwithstanding the oversimplification of the models used in these equations, one might take Chapter 6 on its own as a reasonable, quant-oriented conference paper in judicial politics that at least spurs discussion on a *Kelo*-relevant theme. Placed here in this title with its preceding five chapters, the “State Reactions” chapter unfortunately seems like just that—a conference paper that was put into the middle of an otherwise normative-polemical argument to try to demonstrate something that does not, for the sake of the argument, require demonstration. So too, the brief and final Chapter 7 (“Implications”) brings even more data and statistics to bear into what has already become a quagmire of an argument and muddies the waters further at precisely the point where Scott would have

been far better advised to do whatever impassioned rescue of the normative argument he could have mustered.

There is no doubt that *Kelo* is a thought-provoking and important precedent that invites a great deal of research, commentary, argument and analysis with respect to what it means for condemnation and takings doctrine, our notion of fundamental rights, judicial federalism, state police powers, urban development and a host of other important contemporary topics in American law and politics. One cannot fault this book, upon conclusion, for being under-ambitious in its efforts to address a host of these topics nor Scott's obvious passion in wishing to do so. Given the competition among more comprehensive, organized and jurisprudentially sound titles in the post-*Kelo* analytical space, however, *The Price of Politics* struggles in vain to demonstrate its contribution.

John C. Kuzenski

*North Carolina Central University*

**David R. Jones and Monika L. McDermott.** *Americans, Congress, and Democratic Responsiveness: Public Evaluations of Congress and Electoral Consequences*. Ann Arbor: University of Michigan Press, 2009. 203 pp. (\$65.00 cloth, \$22.95 paper).

“Do you approve or disapprove of Congress?” The response to this question is routinely poured over by pundits, scholars, politicians and, occasionally, the general public. Yet, despite the monthly fascination with this question, until now we have been left to wonder what it really means when the public has a 16 percent approval of Congress compared to a 60 percent approval of Congress. David Jones and Monika McDermott give theoretical and empirical weight to this issue in *Americans, Congress, and Democratic Responsiveness*. Challenging the conventional wisdom of congressional scholars—that people are incapable of or simply prefer not to evaluate Congress in policy terms—the authors demonstrate that the public is not only aware of who controls Congress and what that means in terms of the direction of public policy, but also that the public responds, through the act of voting, to the policy direction of Congress. Further and perhaps even more surprising is that Congress, in turn, is responsive to the twists and turns of public opinion.

Jones and McDermott give a more optimistic view of the American citizen's level of political sophistication than is typically portrayed by public opinion scholars. Rather than an uninformed and uninterested citizenry, the authors argue that the public is indeed quite aware of which party has major-

ity control in Congress as well as the ideological direction of congressional policy. The problem, according to Jones and McDermott, is that previous survey techniques seem more interested in testing factual knowledge of Congress rather than the ability of the public to respond to the political environment. While citizens may lack perfect political information, they often use heuristics, as has been suggested by many scholars, when evaluating Congress. One such heuristic is majority party control.

A key finding from Jones and McDermott is that people tend to evaluate Congress in terms of the majority party, independent of their own party identification (p. 83). This tendency is important as the authors later show it to have an indirect effect on the likelihood of quality candidates challenging an incumbent (p. 119). As congressional approval increases, the likelihood of a quality challenger emerging to face an incumbent of the majority party decreases; this does however increase the likelihood of a quality challenger against an incumbent of the minority party (p. 119). In short, the public appears to be above average when it comes to political sophistication; a point the authors claim is lost on researchers using only factual or quiz-based surveys.

To test their hypotheses, the authors rely heavily on the use of survey data, primarily the ANES data collection. However, the authors also commissioned multiple surveys before and after shifts in party control of Congress; most notably the shift from Democratic control of the U.S. Senate in 2002 to Republican control in 2003. The use of such surveys provides an important test of whether the public is capable of responding to immediate changes in the political landscape. Indeed the authors find that approval among self-identified “conservatives” increased significantly following the Republican shift (there was little change among self-identified liberals between 2002 and 2003). Perhaps just as significant is that “conservatives” did not become more approving following electoral gains, but not a shift in party control, in the U.S. House. In other words, survey respondents were able to conceptualize the importance of a shift in party control. Although the sample size is rather small for the survey ( $N$  is less than 100 for a portion of the subgroup analyses), the significance of the findings should give pause to researchers who argue the public is relatively apathetic about shifts in congressional control.

The empirical evidence presented by Jones and McDermott raises some interesting questions. In Chapter 5, the authors present findings that at face-value appear to challenge one of the bedrocks of the political behavior literature: Fenno’s paradox. The authors show that congressional approval affects the likelihood of casting a vote for a majority party incumbent as well as majority party incumbent vote share (p. 110). As congressional approval increases, the likelihood of voting for and aggregate vote share for majority

party incumbents increases. (It should be noted that authors rely on actual vote records rather than survey data which has been shown to be highly susceptible to over reporting.) The implication is that people tend to vote for congressional candidates in part based on their approval of Congress as a whole (all else being equal), holding the majority party responsible for any disapproving acts. Does this mean that individual voters will vote out their own member of Congress (if they are in the majority), if their approval of Congress decreases? Given the ever increasing incumbency success rate, this seems unlikely.

In Chapter 7 the authors show that approval of Congress is a significant predictor of the number of seats gained or lost by the majority party. Congressional approval nearly bottomed out in the summer of 2008, reaching historic lows (as measured by Gallup) of 14 percent in July. Following Jones and McDermott's model, we should have witnessed an increase in the number of seats won by the minority party (all else being equal) in the 2008 election (approval of Congress was still very low at 18 percent). Instead we saw the opposite; the majority party in the House and Senate gained seats. Although the authors hint at mitigating factors, namely the economy and presidential approval, in the conclusion and elsewhere, more research is needed in this area.

The findings presented by Jones and McDermott are an important contribution to the study of both Congress and public opinion. For those who questioned the public's ability to think in policy terms or whether congressional approval ratings matter, Jones and McDermott provide solid empirical evidence in favor of both. As with any engaging research, the findings provide many important avenues for future research, as suggested above. In particular, given the relative sophistication of the mass electorate as shown by the authors, how does this translate to other aspects of American government? Jones and McDermott have provided much fodder for scholars of mass public opinion.

Christopher W. Larimer  
*University of Northern Iowa*

**Zoltan L. Hajnal.** *America's Uneven Democracy: Race, Turnout, and Representation in City Politics.* New York: Cambridge University Press, 2010. ix, 241 pp. (\$80.00 cloth, \$23.99 paper).

The election of Barack Obama to be the nation's first African-American president has invited many discussions about the so-called post-racial politics in the new era. Zoltan Hajnal's *America's Uneven Democracy*, how-

ever, sends a clear message that the notion of post-racial politics is premature. Hajnal's book interestingly did not address the historic 2008 presidential election since the empirical work of the book clearly was completed before the election, but the rich findings presented in this important volume sheds a light on the role of race in the American political landscape. Race still matters.

Hajnal's focus is on municipal elections. He sets a high goal for himself, to reexamine the literature of political participation by analyzing empirically the impact of the turnout gap between the white majority and racial minorities. Hajnal draws insights from the national election studies on turnout. His disagreements with previous studies are based on his assertion that national politics can be very different from local politics. It has been vigorously argued in the literature that the declining turnout in American elections does not create a substantial effect on democracy itself because non-voters' participation in the election would not significantly change election outcomes in the first place. Even if this observation is true, Hajnal forcefully argued, it can only apply to national elections. For local elections, his study shows that turnout matters, especially when racial groups' interests are concerned.

The Introductory chapter not only thoroughly reviews the turnout literature at the national level, but also directly stresses the important role of race, which very often was ignored by previous national studies of voter turnout. Hajnal emphasizes that "turnout is skewed much more by race than by party" (p. 12). What are the consequences of the skewed turnout on the nation's minorities and democracy itself? Chapter 1 offers the research design to engage in a systematic study of municipal elections. The whole book is based on a plan to answer three profound questions. First, "Does even turnout [in local elections] lead to different winners?" Second, "Does higher turnout lead to more equitable racial representation?" And finally, "Does higher turnout lead to spending on policies that minorities favor?" (p. 28). Hajnal's data are from mayoral elections in the 20 largest cities between 1990 and 2000. Many of his city-level variables concerning race and government structures come from 1986 International City/County Managers' Association Survey (ICMA), which contained questions about the book's primary variable—voter turnout at the local level. The 1987 Census data are also merged with ICMA data to assess the impact of turnout on local government spending on demographic (minority) groups.

Chapter 2 is devoted to explaining why turnout matters in terms of racial groups' divergent preferences in local elections. The key finding of the chapter is that in a typical local election with two candidates, racial groups are often "apt to vote as a bloc." More specifically, "64 percent of Asian American voters favor the group's preferred candidate," while the

percentages for African Americans, Latinos and whites are 83, 70, and 69, respectively (p. 45).

Using simulations, Chapter 3 examines the potential impact of “equal turnout” between whites and minorities. It turns out that if equal turnout did exist, Latinos would see the greatest gains in political representation in local government. While gains for Blacks and Asians would also be present, they would not be as consistent as those for Latinos. Whites would be the clear loser if minorities voted at the same rate as them in local elections. Chapter 4, instead of using simulations, compares cities with high turnout to cities with low turnout based on city council elections. The results once again show that the increase in minorities’ turnout can enhance representation, especially for Latinos and Asians, in city councils.

Chapter 5 is obviously the most important one empirically and theoretically in the whole book. It examines the impact of low turnout among minorities on local governments’ policymaking. Based on the statistical models of redistributive, developmental and allocation spending, which control many key urban variables, ranging from city type and region in the country to demographic change and federalism, Hajnal demonstrates convincingly that “when disadvantaged groups fail to vote, local officials are more likely to be unresponsive to their concerns” (p. 139).

Given the strong empirical evidence of a negative effect of low turnout on minorities’ own group interests, what can be done to increase their turnout level? Chapter 6 provides three reform proposals to increase turnout: moving local election dates to coincide with state-level elections; replacing an unelected city manager with an elected mayor; and switching from non-partisan to partisan local elections. The chapter also examines other possible institutional arrangements, and assesses the cost of each reform proposal.

Finally, Chapter 7 concludes the book by stressing the implications of turnout on American local democracy. The message is clear—turnout matters, and in order to protect the interests of disadvantaged groups, reform is necessary to increase voting participation of minorities. The conclusion sends a serious note to the general audience: America is far from a post-racial nation; in local elections, which influence ordinary citizens’ lives on a daily basis, white majority-voters do vote differently from minorities, and they gain special favors due to the fact that minorities are not voting based on their full potential. At the same time, minorities also often disagree with each other as to whom to vote for. Institutional reforms are necessary to make local elections more accessible. The good news is that these reforms can be done, and they need to be done quickly.

*America’s Unequal Democracy* is an important contribution to the understanding of the continuing significance of race in American local democracy. While the data used in the book (especially the 1986 ICMA and

1987 Census) may be out dated, as the nation has experienced rapid changes in the racial makeup of urban and suburban areas in the last three decades, the conclusion of the book is tremendously helpful for students of American politics in general, and urban and racial politics in particular, to engage in productive dialogues to build a more equal democracy in the future.

Baodong Liu  
*University of Utah*

**Frank J. Colucci.** *Justice Kennedy's Jurisprudence: The Full and Necessary Meaning of Liberty*. Lawrence: University Press of Kansas, 2009. xi, 243 pp. (\$34.95 cloth).

In his recent book, *Justice Kennedy's Jurisprudence: The Full and Necessary Meaning of Liberty*, Frank J. Colucci offers a revisionist interpretation of Justice Anthony M. Kennedy's approach to constitutional interpretation. Many scholars depict Kennedy as a "judicial minimalist" who avoids overarching constitutional theories in favor of narrow rulings that resolve only the particular disputes before the Court (Sunstein 1999). In contrast, Colucci contends that Kennedy has adopted a unified theory, of sorts, consisting of the primacy of individual liberty and the free development of the person over competing constitutional values such as equality or democracy. Colucci provides a thorough description of Kennedy's jurisprudence that engages but ultimately fails to resolve important questions about judicial review, substantive due process, and the future direction of the Court.

Colucci's normative approach to the study of judicial politics follows the path-breaking work of Sue Davis, whose portrait of Chief Justice William H. Rehnquist features content analysis of Rehnquist's judicial opinions, speeches, and papers as a means of charting Rehnquist's core values and jurisprudence (Davis 1989). Davis' methodology represents a conscious shift away from the empirical study of judicial behavior, and she stresses instead the reasoning and policy justifications of judicial actors. In applying Davis' methodology to the study of Justice Kennedy, Colucci benefits from historical circumstance. The acrimonious debate surrounding Robert Bork's failed nomination to the Court led Kennedy to be unusually forthcoming in his December 1987 testimony before the Senate Judiciary Committee. In contrast, later nominees became much more reticent, perhaps due to Clarence Thomas' negative experience before the same committee. Kennedy's candor serves as grist for Colucci's mill.

In his review of Kennedy's confirmation hearings, Colucci highlights the importance of judicial review, substantive due process, and individual



expression as core elements of Kennedy's legal philosophy. On the question of judicial review, Kennedy expressed his preference for generalized inquiries into "official purpose" and "official intent" rather than the narrow search for the framers' individualized "original intention." As Kennedy expressed it in response to a question about the intent of Congress in adopting the Fourteenth Amendment, "The Equal Protection Clause has far more validity and far more breadth than simply what someone thought they were doing at the time." On the issue of substantive due process, Kennedy voiced reservations about an implied constitutional right of privacy but then indicated his opposition to the "wholesale rejection of substantive due process as insufficiently protective of the textual guarantee of liberty." On matters of individual expression, he stated that he would strike down government policies that resulted in "the inability of the person to manifest his or her personality, the inability of a person to obtain his or her self-fulfillment, the inability of a person to reach his or her own potential."

Taken together, these views contemplate a broad conception of judicial power that, in appropriate circumstances, can be used as a vigorous defense of personal liberty. In his review of Justice Kennedy's major decisions in separation of church and state, death penalty, homosexual rights, and affirmative action cases, Colucci portrays Kennedy as consistently protecting a broad right to personal development and individual dignity that embraces, respectively, the interests of non-coercion, redemption, freedom of relational expression, and individual identity. In the First Amendment area, Colucci notes Kennedy's rigorous protection of the individual's freedom of expression in a wide variety of contexts, including campaign finance and campaign speech, political protest, and sexually indecent communication. Significantly, of all the justices on the Rehnquist and Roberts Courts, Kennedy is the one most likely to strike down laws on First Amendment grounds.

Throughout his tenure on the Court, and especially since he issued the *per curiam* opinion in *Bush v. Gore* (2001), Justice Kennedy has adopted a style of legal reasoning that relies upon international law, social science research, and broad moral considerations in his interpretations of the Constitution. This approach is particularly evident in his decisions concerning juveniles and the death penalty (*Roper v. Simmons* 2005), school prayer (*Lee v. Weisman* 1992), and homosexual sodomy (*Lawrence v. Texas* 2003). Colucci finds the influence of Catholic social thought in Kennedy's reasoning. As he articulates it, "Kennedy's citation of social science research, the direction of political consensus, and comparative constitutional law arises from his search for objective referents to support a vision of human dignity with apparent rhetorical and substantive foundations in Catholic thought" (p. 36). This interpretation is based upon similarities between Kennedy's

logic and a variety of post-Vatican II pronouncements, including *Dignitatis Humanae*, that stress free will, personal responsibility, and the overriding importance of human dignity.

Toward the end of his book, Colucci discusses the broader political implications of Justice Kennedy's libertarian ideology. He asserts that Kennedy's expansive conception of constitutional liberty will "reorient" the Court and move it beyond the paradigm of "New Deal justice" that in various forms has dominated constitutional interpretation since 1936. Broadly speaking, this paradigm stresses deference to Congress in matters of economic and social legislation and self-imposed limitations on judicial power in order to promote the policy preferences of democratic majorities. In contrast, Kennedy's approach to judicial review emphasizes the core value of individual liberty over vindication of the democratic process. Thus, Kennedy would replace the activism that the Warren Court asserted on behalf of egalitarian values with an activism that stresses the value of liberty.

Kennedy's approach is not without its critics. One of them is Justice Scalia, whose scathing dissents have challenged both the subjectivity of Kennedy's judicial method and his use of extra-constitutional sources in constitutional interpretation. Likewise, Justices Breyer and Souter have sharply criticized Kennedy's methods and apparent goals. Justice Souter's masterful dissent in *Alden v. Maine* (1999) is illustrative. There, Kennedy held that Congress lacks the constitutional power to require states to submit themselves to private lawsuits in their own courts. Writing in dissent, Souter rejected Kennedy's attempt to ground state sovereign immunity in the structure, theory, and history of the Constitution. He accused Kennedy of developing a natural law approach to sovereign immunity that resurrected the discredited industrial Due Process approach of *Lochner*.

Colucci unsuccessfully attempts to legitimize Kennedy's libertarian activism by noting its similarities to the activism of the Warren Court. However, as Jeffrey Rosen has persuasively argued, on the important question of campaign finance laws, Kennedy's activism differs fundamentally from the activism of the Warren Court (Rosen 2009). Whereas the Warren Court's school desegregation and legislative malapportionment decisions protected political outsiders and thereby engendered public support, Justice Kennedy's defense of unbridled campaign spending by political insiders and big corporations does not.

In the end, Colucci engages these debates merely at the descriptive level. He quotes extensively from both sides but then repeatedly refrains from resolving them. For readers who are unfamiliar with Kennedy's leading opinions and the dissenting counterpoints, this may be an acceptable approach. But the gravity of the subject matter and the risk that Kennedy will impose counter-majoritarian constitutional rulings that trigger wide-

spread public opposition demand more. Those who seek deeper insight into the political implications of Justice Kennedy's jurisprudence will have to look elsewhere.

### Works Cited

- Davis, Sue. 1989. *Justice Rehnquist and the Constitution: The Quest for a New Federalism*. Princeton, NJ: Princeton University Press.
- Rosen, Jeffrey. 2009. "The Trial of John Roberts." *New York Times*, September 13, p. A-18.
- Sunstein, Cass R. 1999. *One Case at a Time: Judicial Minimalism on the Supreme Court*. Cambridge, MA: Harvard University Press.
- Alden v. Maine*. 1999. 527 U.S. 706.
- Bush v. Gore*. 2001. 531 U.S. 98.
- Lawrence v. Texas*. 2003. 539 U.S. 558.
- Lee v. Weisman*. 1992. 505 U.S. 577.
- Roper v. Simmons*. 2005. 543 U.S. 551.

Paul Moke  
Wilmington College

**Alan Wolfe and Erik Owens, eds.** *Gambling: Mapping the American Moral Landscape*. Waco, TX: Baylor University Press, 2009. viii, 509 pp. (\$34.95 paper).

*Gambling: Mapping the American Moral Landscape* resulted from a conference on gambling and morality, held at Boston College in 2007. The volume contains chapters written by economists, psychiatrists, sociologists, historians, political scientists, business scholars, and philosophers of religion and morality. They range from an application of Jewish moral philosophy to gambling to the neuroscience of gambling to an analysis of the politics of legalized gambling. Not surprisingly, these chapters evidence a wide range of methodological approaches to the study of gambling and morality. Social scientists, however, may be disappointed with the lack of original data used by almost all of the authors. In short, there is little new here in the way of social science research. The authors generally opt for reviewing existing research with an eye toward commenting on the relationship between gambling and morality.

The offerings pertaining to culture studies base their analysis on literature and philosophy, often at a great distance from the reality of the activity of gambling. Although T.J. Jackson Lears' chapter, "Beyond Pathology: The Cultural Meanings of Gambling," makes an excellent comparison between the risk-taking of speculative trading and that of gambling, the analysis is hopelessly abstracted from real people who gamble. Lears argues that, "In

this welter of contradictory impulses, the gambler became a complex cultural icon. He embodied both chance and control, both sides of capitalist success” (p. 313). It is hard to observe elderly individuals playing the slot machines at the Wheeling, West Virginia “racino” and take this description seriously.

Unfortunately, many of the chapters look as though the author(s) tacked on a discussion of gambling and/or morality to lengthy expositions of their particular area of expertise. These (effectively) addenda fail to provide any enhancement of our understanding of the relationship between gambling and morality, even when it would have taken little effort to address that relationship. For example, the chapters addressing the psychology, psychiatry, and neuroscience of gambling could have done much more to discuss the morality of regulating gambling. Instead, the authors provided lengthy (sometimes far too arcane for most readers) discussions of the science that would eventually give you the information allowing you to address the morality of gambling regulation.

If the theme of the volume receives uneven and inadequate attention in many of the chapters, the history of legalized gambling appears in various versions in too many chapters. The book would have benefited from one chapter outlining the basic history of legalized gambling, to which all other chapters could refer. Instead, a number of chapters repeat the same information when they could have better spent their time addressing the theme of the volume.

Although the introduction fails to identify the last chapters in each section of the book as concluding or summarizing chapters, they seem to play that role. These concluding chapters could do much more to pull each section together. However, they do not perform that function and add little to the volume.

Some of the chapters provide interesting and (at least for me) new insights on gambling research and the relationship between gambling and morality. As most research on legalized gambling has focused on the states, David A. Skeel, Jr. and William Stuntz’s chapter on federal policy on gambling, “The Criminal Law of Gambling,” was a valuable contribution to the volume. Rachel T.A. Croson, Matthew Fox, and James Sundali’s chapter on “Behavioral and Brain Measures of Risk-Taking” was an excellent touchstone that almost all of the authors could have used to help them address the theme of the volume. Croson, Fox, and Sundali examine how cognitive and affective neural functioning interact with the brain’s controlled and automatic processes. To the extent that gambling activates automatic processes of the brain, assigning moral blame for the activity is problematic.

One odd error committed by most of the authors is to identify the religious source of opposition to gambling as evangelicals. Every study of lottery

adoption and casino legalization of which I am aware finds that *fundamentalists* form the basis of gambling opposition. And scholars of religion and morality should certainly know the difference between the two. Related to this point, the authors often seem to not remember the politics of early lottery adoption when they claim that gambling opponents were never very active. In the 1970s and '80s, fundamentalist Christians formed fairly vibrant groups opposing lottery adoption in the states. After all, the southern states (which are heavily fundamentalist) *were* the last states to adopt lotteries.

However, fundamentalist Christians *did* eventually lose their battle against legalized gambling in almost every state. John Dombrink's chapter, "The Unproblematic Normalization of Gambling in America," attempts to address the increasing acceptance of gambling, but the analysis of this question is cursory and fails to recognize some existing research on the "normalization" of gambling. Instead, two of the reasons cited for the broad acceptance of gambling consist of noting that its opponents were unsuccessful and that they used unsuccessful frames. This simply begs the question of why they weren't successful.

The most interesting contributions are chapters that offer different perspectives on the relationship between gambling and morality. For scholars of religion, the chapter by Kathryn Tanner, "Grace and Gambling," offers an interesting meditation on gambling and Pascal's Wager. And William Galston investigates how Jewish law and ethics have treated the morality of gambling in "The Memory of Sin: Gambling in Jewish Law and Ethics". These chapters explore the theme of the volume in ways that provide the reader with different ways of thinking about the relationship between gambling and morality.

The most intelligent and provocative chapter is offered by one of the editors of the volume: Erik C. Owens' "Civic Values and 'Education Lotteries': The Irony of Funding Public Education with Lottery Revenues." This chapter should have been a model for the rest of the volume. Owens provides coherent analysis of the meaning of using a lottery to fund education, uses more and better research on which to base that analysis, and discusses intelligently how an education lottery could subvert citizens' education.

*Gambling: Mapping the American Moral Landscape* provides some interesting thoughts on gambling and morality, which will satisfy those engaged in the study of popular culture. One wishes that more of the chapters had developed these thoughts a bit further and had used existing research more profitably.

Patrick A. Pierce  
Saint Mary's College (IN)

**Glenn Sulmasy.** *The National Security Court System. A Natural Evolution of Justice in an Age of Terror.* Oxford University Press, 2009. 237 pp. (\$29.95 cloth).

It's been almost a decade since "9/11" and this country continues to be engaged in armed conflict in Iraq and Afghanistan against al Qaeda terrorists with no foreseeable end in sight, and Guantanamo Bay remains operational with no viable solution to the prosecution of detainees held there. Although another administration has assumed the authority of leadership, the Afghanistan war is certainly now President Obama's, a clear strategy for winning and defining victory remains as elusive and effusive as it had been under the Bush administration. World-wide terrorist activity remains a viable threat not only for the United States but also its allies. Questions regarding juridical solutions for terrorist detainees are still unresolved.

Although Glenn Sulmasy's book was written prior to Obama assuming the Presidency, a significant similarity between the previous Bush administration's detainee policy and the current Obama administration's perspective is effectively developing not only a winning terrorist strategy but resolving the issue of whether or not al Qaeda and Taliban terrorists are criminals or enemy combatants. Determination of this impacts the selection of which juridical theory is appropriate regarding terrorist detention and prosecution. The current administration's announcement to try detainee cases in New York in federal court has been delayed. The delay certainly suggests another policy review regarding which juridical theory to apply is occurring. Is there a solution to this policy dilemma?

Sulmasy correctly identifies in *The National Security Court System: A Natural Evolution of Justice in an Age of Terror*, that the critical question still confronting national security policy makers concerns which advocacy paradigm to follow: those who view the conflict with al Qaeda as a law enforcement problem with the corresponding need to utilize the civilian courts and the due process guarantees ordinarily afforded U.S. citizens or those who view it as an armed conflict and desire to utilize the law of war paradigm which considers terrorists to be enemy combatants rather than criminals and rely on military commissions as the appropriate judicial solution. Sulmasy concludes that neither paradigm is working effectively and that the new armed conflict of the 21st century has rendered useless previous conceptions and notions of traditional warfare: the enemy is not a signatory to the Geneva Conventions, does not represent a nation state, does not wear the uniform of a country, violates the laws of war doctrine, and as a non-state has declared war on the United States. Obviously neither paradigm fits neatly or singularly as an appropriate solution and to exacerbate the dilemma further, paradigm advocates of each perspective are both right and

wrong in applying their analyses to the current threat. How can this be? Sulmasy argues that the current armed conflict is in fact a “hybrid” of law enforcement and land warfare and the al Qaeda fighter is indeed a mix of international criminal and traditional warrior. If one accepts Sulmasy’s argument and views the threat in this context, it follows that both paradigms will remain ineffective in providing a framework for detention and prosecution.

Sulmasy presents a third way to resolve the conflict between the two dominant paradigms. In doing so he provides a concise history of the original intent of military commissions, their historical use through WWII, the Military Order of 2001, and the legal issues presented in the two most recent court cases affecting detention and prosecution of terrorists: *Hamdan v. Rumsfeld* (2006) and *Boumediene v. Bush* (2008). Sulmasy waits until the last chapter to unveil his third way. As expected from his earlier characterization of the war on terror as between the U.S. and a hybrid warrior, which makes the war itself a hybrid, he argues that the best means to detain and adjudicate detainees is through the use of a hybrid court.

Although Sulmasy concedes the devil is in the details, his third way calls for a mix of Article III courts and the military commissions. The court would be run by the Department of Justice and the detention, trial, and incarceration held on military bases and properly invested with appropriate level human rights considerations. The value of such a system is that it would permit moving away from the Guantanamo model and still try those detainees accused of war crimes. Adoption of terrorist hybrid courts, like bankruptcy and immigration courts, would satisfy a very specific area of law and ensure civilian oversight of the process. Hybrid courts would distinguish the nature of the conflict and ensure military commissions are not removed from military jurisprudence. Sulmasy also recommends policy makers look at French, British, and Turkish models in considering adoption of a hybrid court solution. Interestingly, Sulmasy suggests that a National Security Court System (NSCS) may be appropriate as a judicial check with Congressional oversight, in cases involving the Foreign Intelligence Surveillance Act (FISA).

Perhaps the most controversial implementation of Sulmasy’s hybrid court is the equal treatment of both U.S. and non-U.S. citizens accused of terrorist activity. The National Security Court System would provide each with similar procedural and substantive safeguards rather than singling out the U.S. citizen to receive the traditional Constitutional safeguards provided U.S. citizens not charged as terrorists. Again, as Sulmasy concedes, the devil is in the details.

*The National Security Court System* is a recommended read for anyone interested in getting a clear and concise juridical overview of the detainee

dilemma and why the current juridical approaches will most likely continue to be ineffective. Although it remains a very highly politicized issue, Sulmasy presents his third way solution in a cogent, documented, and straightforward manner, devoid of partisan rhetoric, and readily acknowledges the contribution others have made to its development.

Sulmasy is critical of the ideologues of both paradigms in controlling the detention and prosecution policy debate and advocates his “hybrid” approach to resolve the problem of detaining and prosecuting terrorists. Sulmasy credentials to address this issue are exceptional; he is a professor of law at The Coast Guard Academy, a U.S. Coast Guard Captain, and a member of the Judge Advocate Generals corps. He is also a National Security and Human Rights Fellow from Harvard’s John F. Kennedy School of Government and has testified before the Subcommittee on International Organizations, Human Rights, and Oversight Committee on Foreign Affairs, United States House of Representatives in May of 2008.

WW Riggs

*Texas A&M International University*

**McGee Young.** *Developing Interests: Organizational Change and the Politics of Advocacy.* Lawrence: University Press of Kansas, 2010. vii, 211 pp. (\$34.95 cloth, \$19.95 paper).

In *Developing Interests: Organizational Change and the Politics of Advocacy*, McGee Young looks beyond the traditional questions of “representational bias” and public policy influence to ask, “what are the creative tensions in society that cause interest groups to develop in particular ways over time?” (p. 1). To answer this question, Young presents a theoretical framework for understanding interest group development that translates into the central propositions that he explores in his qualitative assessment that follows. The framework hinges around the following key ideas. First, organizational founding is critical. For Young, founding is an interplay between the entrepreneur(s) that establishes the interest group and the political and institutional context in effect at the historical moment of founding. Second, the structure of the competition from related interest groups, if any, influences development. Third as interest groups evolve over time, path dependency plays an important role in their development. This path dependency is the result of the benefits of increasing returns which result from: the adaptive expectations of other interest groups in the environment, the high initial investment for startup which reduces a group’s desire to engage in subsequent organizational reengineering, learning effects that result from increas-



ing expertise that compound over time, and coordination effects that result from collaboration among groups (p. 65-166). Finally, the political and institutional context is dynamic and therefore over time may continue to exert influence over interest group development as the context changes.

To evaluate this framework, Young conducts four qualitative case studies of interest groups: the National Small Business Men's Association (NSBA), the National Federation of Independent Business (NFIB), the Sierra Club, and the Natural Resources Defense Council (NRDC). The following constitute exceptionally brief summaries of these case studies and some of the highlights of the features of each group that Young uses to argue his case.

The NSBA was founded in the political upheaval of the 1930s by DeWitt Emery. Emery was a devout conservative and envisioned his organization as a counterpoise to the Roosevelt administration, which he perceived as a threat to small business. Emery's message was essentially anti-government. In contrast to the NFIB, he was not opposed to working with larger businesses to achieve that goal. Emery's imprint would have long term effects. When the federal government began reaching out to the small business community in the 1950s and 1960s, the NSBA opposed what they saw as collusion with the enemy. Another major impact of founding was Emery's choice of a very rudimentary recruitment strategy. Essentially, the NSBA recruited new members by word of mouth through existing members. Having invested in this strategy, the NSBA never really chose to pay the costs to change it. Because it was cheaper organizationally to maintain the existing strategy, this decision forced them down a path that led to them being eclipsed by the NFIB in the more contemporary era.

C. Wilson Harder founded the NFIB in the early 1940s. While no friend of the federal government, Harder was not as conservative as Emery and the organization was founded when relationships were not as antagonistic. The NFIB reflected Harder's desire for an organization which would fight large scale retailers that he saw as destroying "main street" small businesses. This focus led the NFIB to develop an expertise in the anti-trust domain. Despite mechanisms for limited feedback from members, NFIB lobbying efforts consistently maintained an anti-trust character. Another important impact of Harder was his choice of recruitment strategy. By the time of the NFIB there was considerable competition for the small business member. Harder employed an aggressive recruitment strategy based on an extensive sales force that received a portion of new member's dues as an incentive to enhance membership. While the dividends of a large membership were not initially noticeable, the long term impact of this early decision was considerable as the NFIB has become a major player in the world of small business interest groups.

The Sierra Club was born into a non-competitive interest group environment when it was founded in 1892 by John Muir. Muir was another strong personality and oriented the group around preservation efforts. Because of the lack of competition, the Sierra Club was the only major player in the early 1900s and developed a strong relationship with the federal government as it began its efforts towards preservation with the development of the National Park Service (NPS). Sierra Club members would fill many top leadership positions in the NPS and this relationship led to a general strategy of cooperation and influence through key leaders. Because of the lack of competition, it was not necessary to grow a large membership early on; consequently, the group's intimate strategy of taking potential members on "high trips" into the Sierra Nevada's to directly impress them with the importance of preserving the environment were quite successful and sufficient.

The club's preservationist approach persisted until this perspective itself began to threaten the environment through the development of the national parks for increased visitors. Fundamentally, the idea of developing the parks to increase visitors was a logical extension of the original high trips. At a certain point though this development itself threatened the viability of the environment and the club was forced to take a more conservationist approach. A change in leadership was required to achieve this shift in approach; however, even as David Brower steered the organization toward conservationism in the mid-1900s, he was ultimately forced out by more moderate members. While conservation continued to be a key issue, the membership reverted back to what it knew: a more cooperative relationship with the federal government. In this case, the Sierra Club was forced to change as existing policy threatened its core mission; however, even as it reacted to the need for change, it remained locked into its existing way of doing things based on its past patterns of relationships and the expertise that it had developed.

The NRDC by contrast was not born of a single personality but by the merging of a group of Yale Law School students, formed as the Environmental Legal Defense Fund, with a group of New York Lawyers already under the NRDC label. This merger resulted from both groups seeking funding from the liberal Ford Foundation to create an organization that would use the legal system to protect the environment. The NRDC was born into the political tumult of the 1960s. Similar to the civil rights movement that systematically used the courts to advance policy, the NRDC responded to the context of its founding and wanted to do the same for the environment. Also, because its funding originated primarily from the Ford Foundation, it did not need to seek extensive membership and therefore remained a relatively small organization. Tremendously successful throughout the

1970s, the expertise the NRDC developed in using the legal system to protect the environment left it in a quandary when the political climate changed after the election of Ronald Reagan. The NRDC could have changed course and reoriented itself with the new grassroots environmental movement. Not surprisingly though, the NRDC opted to stick with what it knew and deferred the costs of change.

Young articulately uses each of these case studies to provide ample evidence in support of his theoretical framework and the propositions that follow. The comparison and contrast of two different groups from two different substantive domains help to illustrate the importance of context and competition among groups on an historical timeline. In terms of writing, the book is straightforward, well organized and a pleasure to read. At times the message, particularly regarding path dependency, seems a bit repetitive but one could also make the case that this is just reinforcing the message. Young establishes a compelling case for how and why interest group organizations develop in the way they do; however, one is also left wondering how it will be possible to use his framework to systematically address the multitude of interest groups that exist in society when such idiosyncratic factors are necessary to understand their development. It will be interesting to see if Young or others can translate these findings into such a research agenda.

Thomas C. Shaw  
*University of South Alabama*

**Jason K. Dempsey.** *Our Army: Soldiers, Politics, and America Civil-Military Relations.* Princeton, NJ: Princeton University Press, 2010. 266 pp. (\$26.95 cloth).

*Our Army* presents a comprehensive assessment of the United States Army that contradicts much of the conventional wisdom about the military. The author asserts that much of what we think we know about our army is based upon projected prejudices and stereotypes. Thus, any formation of soldiers can generate our highest hopes or worst fears, especially among those with no direct experience with military service. The book offers abundant empirical evidence that much of what the general public may think it knows about the political and social attitudes of our soldiers is seriously flawed. In the place of commonplace but stereotypical perspectives, the author attempts to offer a more complex portrait and richer understanding of the army that is based on more complete data and sounder theorizing.

The author, Jason K. Dempsey, was exceptionally well positioned to do the research that provided the basis for this book. He is a lieutenant colonel

in the U.S. Army who served in Afghanistan. He graduated from the United States Military Academy and earned his Ph.D. in political science from Columbia University. Dempsey is now a professor in the Department of Social Sciences at the U.S. Military Academy and a member of Council on Foreign Relations. Funding for the original surveys relied upon in this book came from the Tomas Rivera Policy Institute, the Military Academy's Academic Research Division, and Columbia's Saltzman Institute. Dempsey was assisted in his research and writing by a large number of students, pollsters, pundits, military officers and educators.

The book's primary target is the conventional view which holds that the American military is highly politicized and overwhelmingly partisan, conservative, and Republican. Dempsey contradicts this general perception by presenting a wealth of original survey data demonstrating that at best this characterization conforms only to the senior officer corps who comprise only a tiny proportion of U.S. forces. In contrast, the author attempts to show that the average soldier in the lower officer and enlisted ranks holds views that are dramatically different from that of civilians. In fact, he argues that not only do rank-and-file soldiers hold values and beliefs that mirror those of the general public, but that most military personnel are much less engaged and partisan than most civilians.

The early sections of *Our Army* place the book's substantive findings in historical context while the later chapters discuss the implications of these findings for the relationship between Americans and their military. Chapter one introduces the issues associated with citizenship and service, chapter two provides a historical overview of politics in the army from the Revolution through the Cold War, and chapter three discusses the makeup of the army from its structure to its social composition. This introduction sets the stage for chapters four through eight that mine the author's 2004 random-sample surveys of army personnel and West Point cadets that attempt to answer specific questions about the degree to which the military is different from the public, the role of demographics, rank and self-selection in explaining these differences and, finally, the extent to which civilian perceptions of the military are accurate. The final two chapters speculate about the future role of the army in national elections and civil-military relations.

Dempsey's findings will go a long way in undermining the conventional wisdom that views the army as a conservative or even reactionary institution. While his surveys break little new ground in their characterizations of the army's senior officers, the similarity between the general public and today's ordinary soldiers and junior officers may be surprising to many observers. The bulk of our army, it seems, consists of soldiers whose ideological identification and opinions on specific economic and social issues are both moderate and somewhat incoherent. Moreover, rank-and-file members

of the military are much less likely to consider themselves members of either major party or to engage in various forms of political participation such as voting, registration, displays of support, or donating money. Nor does it appear that membership or socialization in the military does much to shape the beliefs or political behavior of most soldiers since this study reveals that this is exactly what you should expect from a demographic group that is disproportionately young, minority, male and in the middle in terms of income and education.

The book also shows that senior officers and young cadets come much closer to common stereotypes. Conservative attitudes and Republican partisanship increases steadily with rank. And these differences among officers, soldiers and civilians can't simply be explained by demographics since the author shows that a "Virtual Officer Corp" of civilians weighted to resemble the army officers in terms of race, age, income, education, and gender would be much less extremist than our actual military elites. Even here, however, Dempsey shows that while officers and cadets may feel pressure to conform to a conservative environment and affiliate with a single party, this conformity has little to do with the active efforts, indoctrination and sanctions by the military establishment or its training academy. Instead, the actual mechanisms behind conservative attitudinal and behavioral conformity are a self-selection bias, peer-group dynamics, blurred ideological boundaries, and individual expectations for success.

For anyone interested in military attitudes and public opinion polling, *Our Army* is a must read. Jason Dempsey presents unique surveys with original data and findings that have previously been unavailable. While demonstrating that the military holds political views that are not as radically incongruent with the American people overall, he does show that differences between the upper echelon of the army and the general public do exist on many important issues. Furthermore, he points to the dangers of having a military whose elite is aligned with one political party and is increasingly willing to directly engage in partisan politics. The book's 2008 Election Update and its Afterword are especially valuable with their speculation about the movement toward more balanced partisan identifiers in the army and its thoughts about the way in which the military experience and ethos might augment American ideals.

Despite the balanced empirical evidence that Dempsey provides that many ordinary members of our contemporary army are "up for grabs" in terms of their political affiliations, his evaluation of the service's uniformly conservative and increasingly engaged top leadership ranks is troubling. The author is clearly concerned that the rising visibility and politicization of active-duty officers and retired generals has given them a privileged voice within the state. He warns that this has created an environment that threatens

internal military cohesion, attracts unwanted scrutiny by elected leaders, and undermines the army's reputation for subservience to the democratic process. Unfortunately, Dempsey offers few persuasive prescriptions that might help to restore the institution's former reputation for political neutrality. His only recommendations for dealing with officers who misuse the military's prestige for political gain are symbolic penalties, professional opprobrium, and an appeal to the historical precedents established by George Washington.

James R. Simmons  
*University of Wisconsin Oshkosh*