

Book Reviews

Richard L. Engstrom, Editor

Kevin McMahon. *Nixon's Court: His Challenge to Judicial Liberalism and Its Political Consequences.* Chicago: University of Chicago Press, 2011. 343 pp. (\$29.00, cloth).

Since Bob Woodward and Scott Armstrong's *The Brethren* (1979) gave us a glimpse behind the veil cloaking the Supreme Court's inner workings, we've had a fascination with the Warren Burger Court and Richard Nixon's role in creating it. Their insightful journalistic account was followed by more scholarly work, usually situated in a specific subfield of political science—judicial politics, presidential studies or elections. Kevin McMahon's *Nixon's Court* joins this earlier corpus but with a broader scope.

McMahon's book weaves together court rulings, judicial politics, election strategies, voting behavior, executive branch policymaking, presidential politics, public attitudes, regional politics, and more to analyze Nixon's judicial policy. McMahon is interested in challenging some of the long held assumptions about Nixon's policy toward the judiciary. He argues that, "even though Nixon was ultimately forced to resign, he was far more effective with his judicial policy than his preemptive status suggests and conventional wisdom acknowledges." In fact, Nixon was able to "reshape the Court on the specific and limited grounds his administration emphasized" (p. 69). Those limited grounds were law and order, school desegregation, and busing. Even here, Nixon was not interested in reversing the Warren Court as much as stopping further judicial action. On many other hot button social issues before the court—such as abortion, pornography and profanity—the administration did not express an opinion.

This relates to McMahon's second point, that Nixon was motivated by pragmatic political calculations, even in his unsuccessful nominations of Clement Haynesworth and G. Harrold Carswell. Although they and William Rehnquist were clearly conservative selections, Nixon himself was driven by politics more than ideology in these appointments, despite his conservative rhetoric. In fact, he did not pursue a counter-revolutionary attack on Warren Court rulings, as many expected. As a pragmatic politician, Nixon understood the political environment in which he operated. McMahon draws on Skowronek's theory in *The Politics Presidents Make* (1993) to argue that Nixon was a preemptive president, in office at a time when his policy views conflicted with those of the still powerful previous regime. He could not be transformative; he could not aggressively push for change without incurring

substantial political costs (p. 68). And yet Nixon needed change. First, he had to improve his electoral chances in 1968 and 1972, given a third party Wallace candidacy. He also wanted to help craft a new GOP majority in the wake of Goldwater's crushing defeat in 1964. He sought "to alter the nation's partisan equation" (p. 68) by focusing on the federal judiciary, particularly the U.S. Supreme Court.

Nixon's 1968 campaign highlighted Warren Court rulings as out of touch with mainstream America; they fostered permissiveness that led to increased crime, urban strife and student unrest, and undermined the concept of neighborhood schools through forced busing. Once elected, Nixon followed a similar governing strategy, shifting unpopular decisions to the judicial branch. This tactic is evident in how his administration handled school desegregation.

Nixon blocked his Department of Health, Education and Welfare from exercising their authority to withhold federal funds from Southern school districts that dragged their feet on desegregation. This threw the issue to the federal courts. McMahon notes that, "by successfully moving control of the desegregation process out of the executive branch and into the judicial branch, Nixon assured that federal judges would most often be the ones forcing local officials—usually Democrats—to make tough and often divisive choices" (p. 250). Nixon could then accede to Court mandates, while making clear that it was not his choice. In this way, even when his preferred position lost before the Court, it could be a political win. Nixon realized that he could use the justices' unpopular decisions to discredit Democrats. In the end, McMahon notes, Nixon's approach did not have the legal impact that conservatives had anticipated. For example, he notes that GOP candidates continued to bash Court decisions in 1996 and 2000. They did so again in 2004, according to Epstein and Segal in *Advice and Consent* (2005).

In contrast to legal victories, Nixon's approach was successful in the electoral arena. By running against the Supreme Court, he divided the long-successful New Deal coalition and brought into the Republican fold both disaffected Southern Democrats and urban, largely Catholic Democrats in the North. When presenting his evidence, McMahon concedes that tying the attitudes of northern whites to specific Court decisions is "more suggestive than conclusive" (p. 215). But it is an intriguing idea that Nixon—long blamed for damaging his party—helped to forge a coalition that later Republican candidates could count on. In fact, McMahon credits Nixon with making "significant inroads with a group of voters—so-called Reagan Democrats—commonly thought to have been later entries into the GOP electoral coalition" (p. 250). Nixon's efforts also shifted Democrats to the right, as they sought to placate constituents who were concerned with the social issues that Nixon exploited.

Some of the positions that McMahon takes are not new, even though he characterizes them as contrary to conventional wisdom. For example, other scholars identified Nixon's use of the Warren Court as "a convenient political target, particularly on the part of Republican presidents bent on breaking the Democratic Party's post-Civil War hold on the South" (see Yarbrough 2000, 269). And others have noted the moderate conservatism of Blackmun, Powell, and Burger; Greenhouse, for example, quotes Rehnquist in assessing Blackmun's record on the court of appeals: "I think he can be fairly characterized as conservative-to-moderate in both criminal law and civil rights" (Greenhouse 2005, 47). And in his 1994 book *Judicious Choices*, Silverstein makes many of the same points that McMahon does, noting Nixon's attempt to woo northern white ethnics as well as Southerners, and his public support for civil rights even as he empathized with those opposed to busing.

Another minor observation is that there are numerous distracting typographical errors and misspellings (e.g., "publically") that his editors let slip through. But overall, the book is well researched and well argued, making effective use of manuscripts available at the Nixon Presidential Library, including the files of Patrick Buchanan, Charles Colson, John Dean, Harry Dent, John Ehrlichman and H.R. Haldeman. McMahon also uses Nixon's White House tapes to "shed light on the president's thinking" (p. 148), an important corrective to Nixon's public remarks, since his rhetoric did not always match his action. In sum, with the breadth and depth of his study, McMahon provides a valuable addition to our understanding of this pivotal time in American politics.

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Keena Lipsitz. *Competitive Elections and the American Voter*. Philadelphia: University of Pennsylvania Press, 2011. 264 pp. (\$49.95 cloth, \$49.95 electronic).

Most people believe that competitive elections are essential for democracy, or rather; most people *assume* this to be true. However, in *Competitive Elections and the American Voter*, Keena Lipsitz makes a rigorous empirical argument about the actual benefits of competitive elections. Rather than making yet another attempt to resurrect the “marginality hypothesis,” Lipsitz explores a more original and therefore more important question, What are the effects of the campaigns that occur during competitive elections on voters? Lipsitz argues that competitive elections create informational environments that provide voters not just with more information about the candidates, but with more *useful* information. That requires not just a high volume of information, but a balanced dialog of relevant information. When voters are audience to such dialog, Lipsitz argues that it is better for voters and hence better for democracy.

The argument that voters know more when elections are close is not new. What makes Lipsitz’s argument innovative is that she delves into the causal mechanism. Knowledge doesn’t materialize out of nowhere, and not all information is equally valuable. Information comes from the campaign organizations and through media coverage. So while previous scholars have examined the empirical associations between electoral competition and voters’ knowledge, Lipsitz actually constructs a set of measures of the quality of the information environment faced by voters across different levels of electoral competition in House, Senate and Presidential elections. Lipsitz examines the causes and consequences of (a) the volume of information easily available, (b) the degree to which that information comes equally from the competitors, (c) the diversity of sources available, (d) the degree to which the campaigns engage each other about the same issues, and (e) the amount of negativity in the campaign.

By building a multidimensional measure of the quality of the informational environment created by a campaign, Lipsitz is able to determine which characteristics of the informational environment are affected by campaigns in competitive elections, and which characteristics are most helpful to voters. That nuance allows Lipsitz to make some unexpected arguments. Most strikingly from my perspective (as one who has argued *against* competitive elections), Lipsitz does not argue that more competition is always better for voters. Lipsitz measures the degree of competition in a campaign with Charles Cook’s classifications, ranging from safe elections for one party to toss-ups. By most assessments, Lipsitz finds that the most useful informational environments occur not in toss-up campaigns, but in the

sweet-spot of campaigns that only *lean* towards one party. This is a tricky argument to make since one cannot logically derive the boundaries of the sweet-spot, making it difficult to design policies aimed at achieving the exact right amount of competition. However, the argument partially inoculates Lipsitz from some of my own arguments about competition because most of my arguments about the dangers of competitive elections have been aimed at elections that Lipsitz might consider too competitive.

Methodologically, Lipsitz takes measurement very seriously. The measures of campaign information volume, informational balance, source diversity, dialog and negativity take maximal advantage of available data to make generalizable claims about informational environments. The only major methodological weakness is that studying the effect of the informational environment created by a campaign on individual voters through survey data creates a hierarchical structure in the data that Lipsitz's econometric procedures do not address. Many of the critical findings are sufficiently strong that they may up with multilevel modeling, but it is difficult to make that determination without multilevel modeling.

Beyond the empirical results, Lipsitz argues that her findings demonstrate the need for a set of reforms that would increase the frequency of competitive elections. Ultimately, whether we should support these reforms or the diametrically opposed reforms that I have advocated comes down to the following issue. I have argued (Buchler 2011) that a competitive election is a punishment that occurs when elected officials fail to perform their jobs as their employers—the voters—wish. If so, then a competitive election is not beneficial for democracy—it indicates democratic failure. Lipsitz argues that competitive elections produce better informational environments than non-competitive elections. What if we are both right?

Suppose that public officials who perform their jobs well deter strong challenges based on potential challengers' belief that incumbents who do their jobs well are too difficult to defeat. The result is that public officials who do their jobs well avoid the types of campaigns that inform voters, but it doesn't matter because the end result is that worthy incumbents' contracts are renewed, even though voters may not know precisely why. On the other hand, public officials who do not perform their jobs well draw strong challengers, resulting in campaigns that inform voters about just how badly those officials have done. The result would be a more informed electorate than in the first instance, but only so that a necessary punishment can be carried out to correct for something that has gone wrong. If so, then competitive elections are not intrinsically good—they just have a characteristic that allows them to serve a corrective function that only needs to be served when something bad has happened. Encouraging competitive elections, then, means encouraging the negative antecedents that produce competitive elections,

which hardly seems good for democracy, even if the informational benefits of a competitive election partially mitigate the damage of those antecedents.

Alternatively, suppose that public officials who perform their jobs badly avoid drawing strong challengers for other reasons. The result would be uncompetitive elections in which incumbents win because the campaigns do not inform voters of how badly the officials have done their jobs. If so, then we need to encourage more competitive elections to correct for the negative antecedents that exist anyway.

The question, then, is whether the corrective value of a competitive election outweighs the damage done by potentially encouraging the negative antecedents to competitive elections. That is the central question raised by the current state of the literature on competitive elections. Lipsitz does not address the negative antecedents to competitive elections, but she provides a set of arguments that will be critical to resolving this question because one cannot weigh the benefits and drawbacks of competitive elections without defining each. Lipsitz makes a strong case for the benefits, thereby setting the terms for the debate. This book is required reading for anyone interested in the consequences of competitive elections, including those few of us who remain skeptical of their benefits.

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Jennifer L. Lawless. *Becoming a Candidate: Political Ambition and the Decision to Run for Office*. New York: Cambridge University Press, 2012. 279 pp. (\$99.00 cloth, \$27.99 paper).

At America's founding, the Federalists argued that ambition among citizens with diverse interests was essential to ensure that Americans would be well-served by their government. Yet in America today, political ambition can be hard to find. Local and state legislative offices are frequently uncontested. About a tenth of U.S. House incumbents were unchallenged in recent general elections and many more faced no or only nominal primary challengers. Current levels of political ambition do not provide all voters a choice among candidates, something essential to the fundamental right of citizens in a democracy to 'kick the bums out'—to hold officeholders accountable. What might change this state of affairs? What factors lead someone to develop interest in political candidacy and to pursue their ambition by

running for elective office? Jennifer Lawless answers this question in her informative and compelling book.

Most previous studies of ambition have focused on its presence among individuals who have already made the choice to pursue candidacy. This research has revealed how the political opportunity structure—constituency characteristics, legislative professionalism, and the like—influences political ambition. Lawless draws on qualitative evidence and previous research to agree that these factors influence what she terms *expressive ambition*—actually running for a specific office. However, Lawless further argues that understanding political ambition requires considering earlier stages in the process of candidate emergence. She focuses the majority of her research on explaining the development of *nascent ambition*, an initial willingness to consider running for office at some point. Lawless uses the Citizen Political Ambition Panel Study in her examination—a set of surveys she conducted with Richard Fox in 2001 and 2008.

Lawless distributed these surveys to individuals in the four professions that most commonly produce state legislative and congressional candidates: lawyers, business leaders, educators, and political activists. Drawing on this unique sample enables Lawless to evaluate ambition among even those individuals who are qualified for public office but have not yet expressed or developed political ambition. Additionally, by using answers from respondents surveyed in both 2001 and 2008, Lawless is able to evaluate the dynamic nature of political ambition, explaining what causes ambition to increase and decrease over time. Lawless supplements her survey data with qualitative evidence from 300 interviews with a sample of her survey respondents. No previous research has provided as substantial and thorough a set of data on candidate emergence, making its collection one of the biggest contributions of this work.

Drawing on this dataset, Lawless establishes that nascent political ambition exists and varies from person to person and over time. Even among the qualified professionals in her sample, only about half demonstrate nascent ambition, indicated by having at least considered running for political office. What determines whether a qualified potential candidate develops the nascent ambition necessary to eventually pursue a candidacy? In one of this book's most significant contributions, Lawless demonstrates that political ambition is shaped by events that occur far before a specific political opportunity structure develops. Lawless finds that early socialization into politics, through things like political discussions at home and parental encouragement toward student government candidacy, is a strong predictor of nascent political ambition. Consequently, ensuring adequate political competition in the future may require equipping diverse groups of people to engage with politics in their homes today. Interestingly, Lawless finds fewer

effects of current family circumstances—such as marital or parental status—on nascent ambition.

In addition to early political socialization, two other long-established traits—sex and race—also influence the ambition of Lawless's survey respondents. As Lawless has demonstrated in books co-authored with Richard Fox (2005, 2010), women express lower political ambition than men, even when controlling for variables addressing family roles, political skills, and recruitment experiences. Lawless builds on those findings in this work by looking at the interaction of sex and race and finds that the gender gap in ambition holds within each racial group. Lawless also finds that racial minority status influences political ambition. After controlling for the effects that racial differences in self-perceived qualifications have on political ambition, Lawless determines that black respondents have lower nascent political ambition than respondents from other racial groups. Lawless makes a strong case that racial and gender diversity among candidates is important for the quality of representation provided to American citizens, thus understanding her findings and determining how to address these ambition disparities seems particularly worthy of future study.

Some more immediate and variable factors also influence nascent ambition. Specifically, Lawless finds that those whose professions yield greater proximity to politics and who perceive themselves as having the qualifications needed to run for office are more likely to consider political candidacy, as are those who are recruited by a political actor. Cynicism about politics—which has increased from 2001 to 2008 in Lawless's surveys—and higher income depress political ambition. Furthermore, several of these factors influence not only nascent ambition, but also the expression of this ambition—actually running in a specific election.

Using her unique dataset, Lawless effectively demonstrates that political ambition is shaped by many factors in place before a specific political opportunity structure emerges. However, as she notes, her data limit her ability to analyze many aspects of the opportunity structures facing each respondent in her potential candidate pool. Collecting detailed contextual data for each of the thousands of survey respondents would be virtually impossible. Thus we are largely left to rely on previous research to understand that aspect of candidate emergence. Lawless also notes that some of her results may be affected by the sample of professionals her survey draws from, however she mentions this only briefly. Readers may wish to further consider the implications of the absence of other groups of potential candidates—who do not work in the four professions Lawless surveyed—both from her survey, and more importantly from most political candidacies.

Becoming a Candidate provides an extensive and unmatched look at the development of political ambition. This book makes significant contribu-

tions to our understanding of candidate emergence, representation by race and gender, political socialization, and why who seeks office matters for the functioning of American democracy, making it a must-read for scholars working in these fields. Due to its thoughtful research design, clear description of quantitative analyses, and engaging examples from Lawless's qualitative interview data, this book is also likely to gain attention from scholars in other areas and provide an approachable and enjoyable text for undergraduate readers.

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Michael E. Brown, Owen R. Coté Jr., Sean M. Lynn-Jones, and Steven E. Miller, eds. *Do Democracies Win Their Wars?* Cambridge, MA: MIT Press, 2011. vii, 294 pp. (\$25.00 paper).

This edited volume consists of a collection of essays (many of which previously appeared in *International Security*) that argue and debate the propensity of democracies to win the wars they fight. The contributions by David Lake and Dan Reiter and Allan Stam make the case for the democratic victory proposition, with the remainder of the volume dedicated to a debate between proponents and critics.

First articulated in 1992 and reprinted in this volume, David Lake's contribution conceptualizes states as rent-seeking firms that provide protection to citizens. Yet since states have a monopoly on the provision of protection, they have incentives to seek rents at the expense of society. In democracies, however, citizens can remove elites that engage in excessive rent-seeking, thus making democratic leaders more responsive to public goods provision, which in turn promotes economic growth and higher incomes. Democracies are thus more likely to win wars because they can mobilize resources more effectively, fight harder during wars to avoid policy failures, and come to the defense of other democracies to counterbalance hegemonic autocracies. The chapter concludes with an empirical test that demonstrates the propensity of democracies to emerge victorious from war.

The contribution by Dan Reiter and Allan Stam draws on their 2002 book *Democracies at War* and focuses on two factors that explain why

democracies are more likely to be victorious in war. First, the authors emphasize that selection effects induce democratic leaders to be more prudent in their foreign policy behavior. Since democratic leaders are accountable to the public and defeat in war is often associated with removal from office, leaders will avoid such policy failures by starting only wars they expect to win. Leaders in democracies are better at anticipating the probability of winning because they have high-quality information on the probability of victory from their bureaucracies, public debate, and vigorous discussion of policy alternatives. A second mechanism explains why democracies, in addition to selecting wars they anticipate winning, are more capable on the battlefield. Democracies have superior military effectiveness because of better leadership, higher morale, and a liberal political culture that promotes initiative on the battlefield. Empirical findings show that democratic initiators are more likely to win wars.

The critiques by Risa Brooks and Michael Desch point to theoretical and empirical deficiencies in the democratic victory proposition. Brooks argues that Reiter and Stam fail to develop an original argument and observes conceptual problems in their research. For example, while she applauds the authors' focus on military effectiveness, Brooks points out that democracy does not necessarily coincide with an individualist political culture, which questions Reiter and Stam's claim that democratic soldiers show more individual initiative. While Desch does not attempt to refute arguments on democratic victory, he maintains that democracy is less important in affecting war outcomes than claimed by proponents of the democratic victory proposition. He disagrees with many of the proponents' coding decisions, such as the inclusion of wars with mixed alliances, gross power mismatches, and asymmetric interests. An empirical test shows that the correlation between democracy and victory disappears when such cases are excluded from the analysis. Yet as Lake and Reiter and Stam explain their response, many of the cases excluded by Desch would follow the predictions from their arguments. Moreover, Ajin Choi's response to Desch's critique of the effectiveness of democratic alliances shows that alliances with more democratic partners are more likely to win wars. While Brooks and Desch suggest several possibilities for improvement and clarification of the theoretical logic and empirical evidence on democratic victory, neither author advances alternative theoretical arguments.

The critiques by Alexander Downes and John Schuessler prove more promising in advancing the debate on democratic victory. Both contributions question whether the likelihood of victory motivates democratic leaders as expected in Lake's and Reiter and Stam's arguments. Using Lyndon B. Johnson's concerns of being seen as too soft on communism during the Vietnam War as an example, Downes shows that domestic concerns can at

times push democratic leaders to initiate war even when they are unsure of winning. Similarly, Schuessler examines U.S. decision-making during World War II and finds that a threatening external environment can induce democratic leaders to start wars even when the chances of victory are unclear. In consequence, democratic leaders have incentives to deceive the public on the true probability of winning in order to get popular support for their policies. In addition, the chapter by Downes reexamines quantitative evidence presented in Reiter and Stam and shows that by adding a category of war joiners and including draws as a possible war outcome, the relationship between democracy and victory disappears.

The volume suggests several avenues for future research that could help address sensitivities in empirical findings and theoretical inconsistencies pointed out by critics. First, research should examine to what extent external threat environments might mediate the relationship between democracy and victory. For example, it may be that democratic leaders in international rivalry are subject to different domestic constraints than leaders outside of rivalry, which also suggests a possible connection between Downes' and Schuessler's arguments. Democratic leaders in rivalry may experience more pressure for hawkishness from their publics, which could then make them more susceptible to initiate risky military contests. Second, research should reexamine the definition of war outcomes and address whether the relationship between democracy and victory could be conditional on war aims.

Research has defined victory as the accomplishment of immediate war aims (Reiter and Stam, p. 168), but this coding rule seems questionable in light of the costly and lengthy U.S. counterinsurgency campaigns in Iraq and Afghanistan that followed the conventional war phase. Recent research on counterinsurgency wars has failed to find a relationship between democracy and war outcomes, which raises questions on whether democracies have an advantage in all types of wars. Third, there appears to be a contradiction in the two mechanisms emphasized in Reiter and Stam. Superior leadership, morale, and soldiering is expected to make democracies more successful on the battlefield, yet public opposition to long and costly wars reduces democracies' probability of winning within a fairly short time period and induces leaders to accept draws. Clarification on why the selection mechanism offsets the military effectiveness of democracies over time would be helpful.

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J. David Gillespie. *Challengers to Duopoly: Why Third Parties Matter in American Two-Party Politics*. Columbia: The University of South Carolina Press, 2012. xiii, 290 pp. (\$39.95 cloth, \$24.95 paper).

This volume covers an often-overlooked aspect of partisan American politics: the role of third or minor parties and independents in national politics. These groups have undoubtedly had a profound influence on the nation, yet most texts on political parties focus mostly, if not completely, on the duopolistic nature of Republican-Democratic competition. Professor Gillespie seeks to promote the importance of the other players in the field—those who have been mostly unable to attain office, but have still pushed onto the American political landscape and in many cases forced one or both of the major parties to face an issue that was previously ignored. This volume is the result of those efforts, and the result is a comprehensive and reasonably thorough look at the various minor party movements that have risen and fallen over the lifetime of the United States.

The first three chapters of the book lay the foundation for the more in-depth look at specific third parties that will follow. From the beginning the author takes a stand that the Republican-Democratic duopoly is far from the ideal setup for any political system:

Democratic principles may receive better service from a duopoly than from a one-party regime; but the case cannot be made that duopoly meets, or that it even aspires to, such internationally recognized benchmarks of best democratic practices. . . . This should be seen as a real dilemma for the nation that considers itself—and is sometimes regarded by others—to be the world's leading democracy (p. 2).

With this viewpoint established, the text describes how the duopolistic system developed, why, and some of the formal and informal measures that are in place to maintain the duopoly. Third parties face a wide variety of barriers, including high costs of gaining ballot access, a public campaign funding system that only supports two parties, and legislative bodies full of Republicans and Democrats that are not likely to change the rules to favor other parties. With this background established, the third chapter of this book finally turns the focus to third parties. A third party is defined, and a more extended version of the argument for why third parties matter is presented.

The approach to the rest of the book is somewhat interesting. It is not a chronological history, but does contain some elements of that approach. The flow is somewhat confusing, but the material contained in the chapters is thorough, interesting, and convincingly portrays the importance of these third parties in shaping the nation. There are essentially two “units” con-

tained within the middle chapters: chapters 5 through 9 tell the chronological story of minor parties that passed in and out of existence in the Nineteenth and Twentieth centuries, including the late Twentieth century independents movement that is discussed in chapter 9. Chapters 5 through 8 split up the history of the nation into three time blocks and discussed the minor parties that were active in each era.

Chapter 4 and chapters 10 through 13 focus on specific parties or groups of parties in the context of the parties' existence rather than chronological time. Chapter 4 discusses the most contemporary parties to the left and right of Democrats and Republicans (Constitutionalists, Greens, and Libertarians), and then the next chapter dives into the chronology for a few chapters before returning to parties tailored to specific groups in chapter 10. Chapter 10 looks at parties that promoted voting and civil rights for women, African Americans, and Latinos; in each of these cases, third parties were ahead of the major parties in pushing for women and minorities to have equal rights. Eventually, the major parties were forced to co-opt the rights that the third parties were pushing—eliminating the need for the separate parties.

Chapters 11 and 12 cover the so-called “doctrinal” parties; the Socialist and Communist movements within the United States are covered together, and the neo-Nazis are discussed in chapter 12. Chapter 13 covers parties that were not large enough to make it onto the national scene, but which did significantly impact state and local politics in their respective areas. Many of these were parties focused on specific regional issues and therefore lacked national appeal, but they deserve mention because of their influence on the development of state parties and politics. This chapter is particularly indicative of the level of detail the author brings to the book: it would have been easy to write this volume and completely ignore the state level, but Professor Gillespie takes the time to include this often-overlooked area.

The final chapter provides a brief wrap-up to the book and an interesting discussion of the Tea Party movement as a potential third party (although it still remains an offshoot of the Republican Party) and what the future of partisan politics could look like. The controversial Citizens United court decision is also discussed, although the effects the author expects the decision to have on third parties specifically is less clear than in the Tea Party discussion. Perhaps most importantly, the final chapter provides a table listing all of the issues and values that third parties have pushed onto the national agenda. These issues range from women's and minorities' rights, to presenting party positions as a formal platform, to currency issues, touching practically every area of public policy imaginable. This table drives the central point of the book home—third parties have had a profound impact on American politics by pushing issues and agendas that otherwise

might have gone unaddressed by the major parties for many years. The appendices to the book also provide large amounts of good information on the third parties that would be quite useful.

This book would be an excellent supplementary text for a course on parties or policy agenda development in the United States, as it would provide students a comprehensive look at the party system that exists beyond the Republican-Democratic duopoly. The order of the chapters could be slightly altered in assignments to work in the chronological (chapters 5-9) and specific parties (chapters 4, 10-13) units. Using the book in this way would make an excellent addition to courses on parties and agendas. Overall the book is very well-written, well-researched, and makes a significant contribution to the literature on parties in American politics.

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Tracy Sulkin. *The Legislative Legacy of Congressional Campaigns*. New York: Cambridge University Press, 2011. x, 221 pp. (\$85.00 cloth, \$25.99 paper).

In *The Legislative Legacy of Congressional Campaigns*, Tracy Sulkin provides an excellent follow-up to her first volume, *Issue Politics in Congress* (2005). As with *Issue Politics*, *Legislative Legacy* demonstrates a keen interest in connecting congressional elections and legislative behavior. The book asks a simple question: do legislators keep their campaign promises? Do campaigns provide voters with meaningful information on which to base their participation, and once in office, do legislators follow through? This is timely work, given the profoundly low level of public approval for the country's most democratic branch of government. Moreover, the topic is normatively important, in that it sheds light on representation.

In representative democracies, campaigns *should* be at the fulcrum of input and accountability. Sulkin's analysis, which skillfully unpacks this question, allows us to assess this dynamic. Much of the literature on representation focuses on policy congruence between legislators and their constituents. In assessing whether campaign messages signal future activity, Sulkin adopts an agenda-based approach to representation. Her central argument is that among legislators, sincere policy goals and electoral goals are complementary. In making and keeping policy promises, both goals are served, and as a result "promise keeping should be common . . . and should vary systematically" (p. 26).

Sulkin draws a sample of representatives and senators elected or re-elected in 1998, 2000, and 2002, and analyzes the content of their campaign appeals and then their legislative activity in office. Data on campaign appeals are taken from ads aired in the top 75 media markets in the country and archived by the Wisconsin Advertising Project. Data on legislative behavior are based on bill introduction and cosponsorships. Both are coded based on which policy issues they raise. Overall, eighteen exhaustive and mutually exclusive policy issue categories are identified. In this context, to keep a promise is to be active on the issues highlighted in the campaign. Campaign priorities signal legislative priorities, and legislative priorities are measured in terms of bill introductions and cosponsorships. As Sulkin notes, the benefit of this approach is threefold. First, it moves the study of legislative behavior beyond roll call votes, which are structured by leadership and beyond the control of individual legislators; second, it allows for the measurement of the intensity of legislators' priorities; and third, it focuses on the early stages of the legislative process, where alternatives are being developed and considered.

The analysis reveals that most of the ads in the sample of 2,254 (1,468 for winning House candidates and 786 for winning Senate candidates) discuss policy issues, and that candidates have narrowly focused agendas. It is not the case that they toss out many signals, hoping to stumble onto something that resonates with voters. Rather, House candidates tended to limit their appeals to four or five issues, and Senate candidates to seven or eight issues. Although no single issue emerged as dominant, the most numerous appeals were made on education, followed by Social Security, taxes, and Medicare. With respect to legislative activity, on average representatives introduced bills on about five of the eighteen issue categories, while they typically cosponsored bills across 17 categories. On average, Senators introduced bills across 11 issues categories and cosponsored bills across 17 categories.

Is mentioning an issue in a campaign ad related to legislative activism on that issue? Yes, and no. In the House, the relationship is positive and strongly significant across almost all issues. For the Senate, the relationship is less consistent. As Sulkin notes, at least for the House these results are normatively heartening, as legislators appear to follow through on their campaign promises. But the question remains: what explains these linkages, or the lack thereof? An important element of this is interest in an issue in the first place. Sulkin models legislative activity on each issue as function of past activism—which she posits captures legislator interest in an issue—and indeed finds that it is good predictors of activism. In short, legislators are active on issues because they are interested in them, and you know they are interested in them because they have previously been active.

Sulkin acknowledges the unfortunate “circularity” of this reasoning (p. 80). This does not seriously undermine the overall analysis and argument of the book; it’s just that both would have been stronger if the author had been able to utilize an independent measure of “interest.” In any case, while interest predicts activism in both chambers, it only predicts campaign appeals in the House. In the Senate analysis, this relationship is weaker. “Thus, for the House, the same factors that explain activity explain campaign agendas [i.e., interest], but for the Senate, there is a disjuncture, and this disjuncture explains why promise keeping linkages are much rarer among senators than representatives” (p. 84). Senate candidate campaign appeals, then, are “noisier signals about their true priorities” (p. 86).

Separate from the inter-chamber differences, promise-keeping varies systematically based on structural and legislature characteristics. Safe, junior legislators, and legislators with larger overall agendas, and those who are generally more active appear to be the most attentive to their campaign promises. Moreover, among senators and representatives, promise-keeping is positively related to vote share in the subsequent election. Legislators are not just advancing their policy goals, but they seem to be shoring up their re-election constituency and warding off strong challengers. Finally, Sulkin finds that there are important policy implications to promise-keeping. In the period under study, 140 promise-keeping bills/resolutions were passed into law.

Sulkin makes a compelling case for studying representation from an agenda-based perspective. Her analysis is careful, rigorous, thoughtful, and provides a number of important findings—most notably that when tied to interests, campaigns provide strong signals to voters as to what candidates will do in office, and that safe legislators are the most attentive to their campaign promises (which may explain why they are safe). However, the larger take-home point from this book is that the subfield distinction in legislative studies between electoral politics and legislative behavior—the “Two Congresses,” if you will—is largely artificial” (p. 205). The relationship between the choices legislators make in both domains is organic and sincere. Successful, savvy candidates are not necessarily manipulating the voters, as traditional electoral connection arguments imply. Rather, they are pretty clearly telling constituents what to expect. This helps us to understand the larger paradox of loving one’s representative while holding the institution as whole in low regard. Caveat emptor—the voters get what they have chosen.

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Reference

Sulkin, Tracy. 2005. *Issue Politics in Congress*. New York: Cambridge University Press.

Gustavo A. Flores-Macías. *After Neoliberalism? The Left and Economic Reforms in Latin America.* Oxford: Oxford University Press, 2012. xiii, 261 pp. (\$99.00 cloth, \$27.95 paper).

In the last decade or so, most Latin American countries have elected leftist governments. Yet, this electoral “left turn” has not generated a sweeping move to more statist or redistributive economic policies. Rather, we have witnessed a wide array of policy outcomes, ranging from an incipient socialist transition in Venezuela to virtually uninterrupted neoliberalism in Chile. Gustavo Flores-Macías has produced probably the best account of the causes of this variance, one that combines a parsimonious central argument with an appreciation of the diversity of factors at play in the different countries. The book also has a broad empirical base, examining eight countries that have elected leftist governments in recent years and devoting detailed attention to three of them: Brazil, Chile, and Venezuela.

The author argues that the key to understanding differences in economic policy among the region’s contemporary leftist governments is the degree of institutionalization of the political party system. Highly institutionalized party systems result in presidents who are team players committed to working within established institutions, and they provide both the president’s party and opposition parties substantial influence in the policymaking process. Consequently, they tend to produce moderate policies. In contrast, weakly institutionalized party systems may give rise to maverick, anti-system presidents who seek to ignore or intimidate parties and legislatures, rather than negotiating with them. Such settings are more likely to produce abrupt, sweeping policy change.

Flores-Macías argues that the three countries with the least institutionalized party systems at the time that the leftist government was elected, Venezuela, Bolivia and Ecuador, also deviated most strikingly from the neoliberal status quo. In contrast, the two countries with the most institutionalized party systems, Chile and Uruguay, experienced the least change. Argentina, Brazil, and Nicaragua are arrayed between these two poles in terms of both party system institutionalization and the extent to which leftist governments implemented statist reforms. The author also looks at two countries, Colombia and Mexico, that have not elected leftists in recent years, in order to assess whether leftist governments in general have brought more statist policies than conservative governments. He finds that they have.

In addition to party system institutionalization, Flores-Macías evaluates five explanatory variables culled from the literature: natural resource wealth, economic conditions, the depth of previous market reforms, the powers of the presidency, and labor movement strength. He argues that most of them shed light on some of the cases, but they do not explain the outcomes as

comprehensively as party system institutionalization. He also dialogues with an earlier current of theorizing on Latin American parties that emphasizes the deleterious impact of party system fragmentation and polarization on economic policy reform. While acknowledging that these variables can be important in institutionalized party systems, Flores-Macías underscores that they are of little significance in cases in which parties are not influential political actors.

The idea that strong institutions encourage moderate policymaking has been made before but this book develops a sophisticated, tightly-argued version of it and applies it in a disciplined way to an important and timely empirical puzzle. The author uses data and graphics effectively and includes a wealth of information on Latin American economic policies since the transition away from import-substitution industrialization in the 1980s. The breadth of the analysis and the clarity of the exposition make it a good choice for upper-level undergraduate classes, as well as graduate seminars.

These positives notwithstanding, there are certain aspects of the book that this reviewer found somewhat objectionable, related to both the conceptualization of the independent variables and the analysis of certain country cases. An example of the former is his treatment of the effects of extractable natural resource wealth on the likelihood that a leftist government will adopt statist policy reforms. For Flores-Macías, this variable operates exclusively through its impact on fiscal policy, the idea being that governments in resource-rich countries undergoing an export boom may be more statist simply because they have an easy source of revenues. The problem with this point of view is that it ignores the political significance of state control over natural resources. Natural resource nationalism has been a major element of the left's appeal in resource-rich Venezuela, Bolivia, and Ecuador, and in each of these countries the assertion of greater state control over hydrocarbons has been a core aspect of recent leftist reforms.

The author's exclusive focus on the fiscal side of this variable allows him to present the Chilean case as powerfully contradicting it, since Chile has enjoyed a major influx of copper revenues in recent years but has continued to adhere to fiscal austerity despite the election of two presidents from the Socialist Party. However, if we consider the issue of state control of natural resources, Chile's significance is not so clear. In Chile, unlike Bolivia, Ecuador, or Venezuela, the key natural resource was already firmly in the hands of the state when the left took power in 2000. President Ricardo Lagos could not nationalize copper for the simple reason that Salvador Allende had already done so in 1971. Had the right-wing dictatorship of Augusto Pinochet (1973-1990) reversed this nationalization, one can well imagine that contemporary pressure to assert greater state control might be substantial, given the extraordinary profits generated by the copper industry.

In other words, his narrow view of the natural resource variable leads Flores-Macías to downplay its explanatory power to an extent that is somewhat misleading.

Objections can also be raised to the author's treatment of certain countries, especially Argentina and Chile. In the former case, he overlooks the major (around 10% of GDP) increase in tax revenues the country has experienced since 2003, when Néstor Kirchner, from the left wing of the Peronist party, assumed the presidency. This revenue windfall, based in substantial measure on relatively progressive taxes on agricultural exports, helped to fuel a major increase in social and infrastructure spending, giving rise to what has been described as "export-oriented populism." Despite this change, Flores-Macías characterizes Kirchner's tax policy as "neutral" relative to that of his predecessors.

With regard to Chile, the problem lies in the author's conflation of party system institutionalization with the institutional constraints left over from the Pinochet regime, especially the "binomial" electoral system, which is generally understood as inflating the right's representation in the legislature. In his zeal to show that Chile's highly institutionalized party system is behind the lack of a leftward shift in economic policy, Flores-Macías suggests that the electoral system and other institutions established by Pinochet helped to perpetuate neoliberal policies by strengthening the party system. Yet, by his own admission (as well as that of many other scholars) Chile's party system was already strongly institutionalized before the 1973 coup d'état that brought Pinochet to power. What the dictator's reforms did was simply to force the center-left governments that followed him to compromise with the conservative opposition by artificially boosting the latter's legislative strength.

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Carol McNamara and Melanie M. Marlowe, eds. *The Obama Presidency in the Constitutional Order: A First Look*. Lanham, MD: Rowman & Littlefield Publishers, 2011. v, 250 pp. (\$75 cloth; \$74.99 electronic).

The Obama Presidency in the Constitutional Order: A First Look assesses the Obama Administration from the perspective of constitutional theory, structure, and power and aims to answer three questions: "First, what is President Obama's understanding of executive power under the Constitution? Second, how does he exercise his executive power within the parameters of the Constitution, especially in relation to the other branches of

government? And, how does this relate to the nonconstitutional, or informal, powers of the presidency—in particular, the rhetorical and educative responsibilities of the modern presidency?” (p. vii). These questions demarcate the efforts of Carol McNamara and Melanie Marlowe from the profusion of scholarly and nonscholarly works regarding the Obama administration and the institution of the presidency that have inundated the public and, concomitantly, scholarly communities during the past year.

Our understanding of the presidency and the development of the concept ‘the modern presidency’ has been heavily influenced by Richard Neustadt’s *Presidential Power*. Neustadt’s emphasis on the informal powers of the presidency—the power to persuade—offered a sharp rebuke, perhaps too much, from the institutional and legal perspective that had dominated presidential literature. *The Rhetorical Presidency* by Jeffrey Tulis restored balance to presidential scholarship by conclusively demonstrating that the formal Constitution, in addition to being a legal source of presidential power, influences how the executive branch interacts with other branches of government and influences the occupant of the executive branch. Tulis recognized that public opinion is still important, but is a mysterious and unpredictable device by which to govern. *The Obama Presidency in the Constitutional Order* embraces and advances this rhetorical presidency framework.

In the Introduction, Joseph Bessette argues our constitutional structure produces “a necessary tension between the constitutional character of the presidential office and the occupant’s connection to public opinion” (p. xi). This point is buttressed by Anthony Corrado, who argues that Obama, despite being a prolific campaigner, has had a difficult time transitioning from campaigning to governing; the skills and tools needed to win a presidential election—“plebiscitary politics”—are less compatible with governance. Corrado contends the eventual passage of health care reform “was an outcome shaped more by institutional and party pressures that encouraged compromise and political bargaining than by the influence of the President’s personal constituency” (p. 18).

David Nichols examines past syllabi, exam questions, memos, and student anecdotes to draw conclusions about the constitutional knowledge of professor Obama. Nichols finds that professor Obama held “a progressive conception of law, in which history moves the law in an ever more positive direction” (p. 33); however, president Obama exhibits a conception of law closer to George W. Bush (and less progressive than many of his supporters would have preferred). Similarly, Joshua Dunn delves into Obama’s memoirs, first State of the Union, and campaign remarks to draw conclusions about Obama’s view of executive power, judicial power, and the Constitution. Dunn believes Obama is a legal realist—personal policy

preferences influence judges, a judicial minimalist—courts are incapable of creating social change, and a pragmatic—willing to sacrifice minimalism for political gain. He concludes, “We should expect him to behave like a legal realist and judicial minimalist, but we should not expect him to be consistent” (p. 105). Nichols and Dunn conclude that the institution has shaped the behavior and beliefs of Obama.

Knott also finds a discrepancy between the words of candidate Obama and the actions of president Obama in the realm of national security and the war on terror. Knott is intensely critical of, even alarmed by, Obama’s approach, and believes Obama is dangerously ceding the role of an energetic executive to an imperial judiciary. He argues that expansive notions of the presidency in the realm of national security are as old as the Constitution itself and consistent with the original intent of the founding fathers. Melanie Marlowe compares and contrasts candidate Obama with president Obama in the area of unitary executive theory of presidential power. Candidate Obama hinted of a more progressive role for the executive and deference to Congress. Marlowe examines four issues—administrative access to information, signing statements, control of administration, and national security—to conclude president Obama has been an avid practitioner and defender of the unitary executive theory of power.

Andrew Busch compares candidate Obama with president Obama regarding congressional–executive relationships. On the matter of signing statements, candidate Obama had campaigned against them, but president Obama has used them, albeit less than his predecessor. Busch undertakes a brief case study of four key pieces of legislation—the economic stimulus, health care reform, cap-and-trade, and financial regulation—to conclude president Obama appears deferential, but he is incapable of determining whether this deference is genuine or tactical. Busch concludes that collusion appears to be the best framework for describing Obama, writing, “In this model, Obama controlled the agenda, established broad principles of legislation, allowed a friendly Congress to work out the details, gave public backing and cover to the enterprise while it was ongoing, and actively intervenes only when necessary to iron out disagreements or push for final passage” (p. 86). Busch concedes, rightfully so, that the absence of divided government weakens his ability to make any firm conclusions about Obama’s executive-legislative framework.

Marc Landy provides the obligatory Obama-FDR comparison, seemingly appropriate because both were progressives coming to power during a period of economic tumult and proposing massive governmental spending as a solution. Landy finds Obama to be less like FDR and more like LBJ. This innocuous essay comes alive when Landy writes, “They must wear a mask of command sufficiently sturdy and fearsome to impress Americans to work

longer into old age, care more for their elderly relatives with fewer health care services, and, more broadly, develop levels of self-abnegation and public spiritedness previously reserved for war” (p. 167). Written before the recent debt ceiling crises, Landy’s observation is incredibly accurate and all too prescient.

The final essay, a rather ominous piece, is written by James Ceaser. Ceaser believes Obama is “on the brink of abandoning the presidential role” because he has created a presidency rooted in charisma, populism, and demagoguery (p. 212). Ceaser argues that Obama’s rhetoric denigrates the standards of the office and lacks sincerity, writing, “For a president to wage a populist campaign outside of the campaign seasons, for a president to call out directly by name another citizen, for a president to pose himself as ‘fighting for you’, pulls the office down and the occupant with it” (p. 212). He finds Obama’s behavior more suitable for a candidate than a chief executive. Ceaser’s argument is persuasive, but short-sighted.

In summary, *The Obama Presidency in the Constitutional Order* improves our understanding of the institution of the presidency, builds upon constitutional theory/rhetorical presidency literature, and places the early days of the Obama administration within these frameworks; however, substantial variation exists within this book in how effective each essay is in contributing to these tasks, and the book is limited in its analysis to the first year-and-a-half of the Obama presidency. Readers focused on these latter concerns will miss the contribution this volume makes to our understanding of the presidency and presidential scholarship.

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Mitchell A. Sollenberger and Mark J. Rozell. *The President’s Czars: Undermining Congress and the Constitution*. Lawrence: University Press of Kansas, 2012. xi, 298 pp. (39.95 cloth, \$24.95 paper).

The President’s Czars is the next installment in the growth of modern presidential aggrandizement. Although not as provocative as presidential war-making or warrantless wiretapping of civilians, Sollenberger and Rozell raise alarm bells with their four-count indictment:

What has evolved over the nation’s history seems to us a worst-case scenario outcome: czars have further fueled the concentration of executive power, undermined democratic controls, added more layers of decision making in government, and, for all of that, they have generally not done a good job of

making the executive branch bureaucracy more effective and efficient (p. 169).

As such, *The President's Czars* joins that body of scholarship by congressional legalists whose detailed scrutiny of statutes is ample proof that the use of czars is not grounded in the law.

Since the term czar has been used and abused by journalist and pundits for so long, the first task is to establish a working definition to guide this analysis. Here “a czar is an executive branch official who is not confirmed by the Senate and is exercising final decision-making authority that often entails controlling budgetary programs, administering/coordinating a policy area, or otherwise promulgating rules, regulations, and orders that bind either government officials and/or the private sector” (p. 7). Most importantly, the overwhelming number of czars was appointed by presidents using unilateral presidential power without statutory authority.

The President's Czars is organized into eight chapters. The first looks at czars and the Constitution; the second reviews the nineteenth century; and the following five chapters are devoted to the modern presidents: first Roosevelt, then Truman to Johnson, Nixon to Carter, Reagan to Clinton, and finally George W. Bush and Barack Obama. The conclusion offers legal reforms designed to restore the constitutional balance. Given the influence of APD scholarship, modernity is more a contested term today than previously when conventional wisdom identified Franklin D. Roosevelt as the first modern president. Although FDR “established new czar positions and set into motion many of the conditions for future presidents to continue, and to expand, the use of executive branch czars” (p. 53), I am not convinced that FDR’s initiatives became institutionalized in the modern presidency.

Sollenberger and Rozell provide a detailed listing of all erroneously “media-labeled” and actual “czars” from Wilson to Obama (this 27-page inventory alone is worth the price of the book). Counting only the number of “actual” czars yields these totals: Roosevelt (44), Truman (10), Eisenhower (2), Kennedy (0), Johnson (5), Nixon (17), Ford (0), Carter (0), Reagan (5), George H.W. Bush (2), Clinton (2), George W. Bush (11), and Obama (22). It is noteworthy that 42 of FDR’s czars were appointed during World War II, whereas both his New Deal czars were not unilaterally appointed but authorized by statute. Any observer reviewing the post-FDR era, therefore, would conclude that czars were a rarity and the spike under Nixon was simply further evidence of his monarchical paranoia. After Nixon the numbers stabilize at low levels until the tenures of George W. Bush and especially Obama.

The definitive work on the evolution of White House staffing by Charles E. Wolcott and Karen M. Hult (*Governing the White House*, 1995)

identify three causes for the creation of new administrative units: environment pressures (group demands), organizational dynamics (specialization), or the idiosyncratic needs of the incumbent. Can there be any doubt that the extraordinary use of presidential czars by Nixon, Bush-II, and Obama are due to their quirky personality needs? Sollenberger and Rozell acknowledge that “the legacies of these two administrations disturb us greatly” (p. 162), but perhaps they are too pessimistic. Given that Bush and Obama are deviant cases coupled with the fact that presidential czars have been barely institutionalized since FDR, we can hope that the backlash against Obama will influence his successor and bolster the political will of Congress to follow the prescriptions in chapter 8 and strengthen the legal parameters guiding the appointment of presidential czars.

During the nineteenth century the only “czar-like” appointments were the use of presidential commissions and diplomatic envoys. Both began under Washington, though the use of presidential commissions was “sporadic and relatively unimportant” (p. 29) until Theodore Roosevelt deployed six commissions for study and analysis (e.g., inland waterways). But more problematic is their criticism of special diplomatic envoys, such as when Washington sent Supreme Court Chief Justice John Jay to England to negotiate a trade deal. Their interpretation will raise eyebrows among Hamiltonians who celebrate presidential prerogative in foreign affairs, particularly their assertion that the Framers “rejected the view that the chief executive would exercise inherent powers” (p. 35). The fact that presidents have continued to utilize special envoys until the present day begs the question of how presidential prerogatives endure and why politics trumps the law in sanctioning their use. The driving force behind presidential prerogatives is not the elegance of the law but the failure of Congress to assert its political will.

And Sollenberger and Rozell are acutely aware of this political reality, as they concluded: “With widespread scholarly and public support, combined with presidential ambition and congressional acquiescence, the modern vast consolidation of executive powers is no surprise” (p. 178). Knowing full well that we cannot resolve that political conundrum, at least it is comforting to know that presidential czars are simply troublesome but not terminal for the health of our republic. Beyond that, this historical chronicle argues that the use of presidential czars represents yet another lost opportunity rather than any strategic breakthrough by the modern administrative president who seeks to achieve some kind of managerial hegemony over the bureaucracy. Professors Sollenberger and Rozell have authored a volume of high scholarship that will be regarded as the definitive study of presidential czars for decades to come. It should be essential reading for students and

scholars in presidency studies, public administration, and American political development.

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Reference

Wolcott, Charles E., and Karen M. Hult. 1995. *Governing the White House: From Hoover through LBJ*. Lawrence: University Press of Kansas.

Neal G. Jesse and Kristen P. Williams. *Ethnic Conflict: A Systematic Approach to Cases of Conflict*. Washington, DC: CQ Press, 2011. 415 pp. (\$47.00 paper).

Neal G. Jesse and Kristen P. Williams offer a book that uniquely addresses the disparate manner in which the study of ethnic conflict is conducted. To help remedy this they attempt to integrate the literature in international relations with that of comparative politics and apply a “levels of analysis” approach to cases of ethnic conflict. On the whole, I find their integration of these varied literatures very good indeed. Although brief, the substance of the literature review achieves conceptual breadth while the levels approach provides structure for a phenomenon that is immensely complex. On the other hand, the case analyses demonstrate that what they master in ontological clarity, they sacrifice in the way of parsimony and predictive power. Ultimately they leave the reader with an advanced knowledge of historical facts, but a much more limited ability to identify the most important among them. To the extent that a discerning analysis is antecedent to theoretical advancement, the book is lacking. However as a pedagogical tool for graduate or advanced undergraduate courses, it is certainly one of the better books available.

In the first chapter, “Ethnic Conflict and Approaches to Understanding It,” the author’s provide a review of the ethnic conflict literature. First, they produce a succinct but fairly comprehensive discussion of ethnicity, nationalism, and gender. They make use of Ted R. Gurr’s typology of ethnic groups, which is based on two dimensions: group composition, and group preferences. They follow this presentation of the comparative literature with a discussion of the somewhat limited international relations work on ethnicity, focusing on the three most popular paradigms, realism, liberalism, and constructivism. It should be noted that with the possible exception of some variants of liberal theory, most of this work has only recently been extrapolated to explain ethnicity.

In Chapter Two, “Levels of Analysis and Ethnic Conflict,” the author’s introduce the levels of analysis framework (i.e., individual, domestic, and international) as the foundation for the discussion of the book’s case studies. At the individual level, the authors appropriately contend that it is elites who ultimately make decisions on behalf of ethnic groups. Thus, to a very large extent conflict resolution depends upon the instrumental goals of political leaders, their role in the promotion of ethno-nationalism, and their interaction with other elites. Even when accommodation ranks high among actor preferences, peaceful outcomes hinge on the ability of leaders to contain the ambitions of spoilers and effectively manage attempts at ethnic outbidding.

At the state level the author’s catalog the many actors and processes that contribute to ethnic conflict. The important actors include group elites, political parties, and mass social movements. Each of these groups possesses a unique set of preferences, a variety of resources, and each pursues different organization strategies. At the international level balances of power/threat structure the opportunities associated with insurgency (i.e., influences cost and benefits and the availability of resources) and in turn influences radicalization patterns among rebel groups. Similarly the porous nature of many international borders, the flow of refugees, and the preferences of interventionist states, adds a critical international element to what are ostensibly domestic conflicts. Finally the author’s discuss those ethnic conflicts that require international intervention, both coercive and non-coercive.

The authors use these analytical tools to assess five cases, Northern Ireland, Bosnia, Sudan, Sri Lanka, and Israel. In each, they provide a brief overview of the conflict and then discuss the most important factors operating within each level of analysis. Their analysis of these cases eventually demonstrates that each is immensely complex and that there is no generalizable solution to ethnic conflict.

In terms of dispute settlement, the cases range from relatively successful to clearly deficient. In Northern Ireland decades of moderate but persistent violence have given way to a lasting peace, albeit with a dysfunctional government. In Bosnia, years of peaceful integration were suddenly disrupted by the disintegration of the Soviet Union and the ambitions of opportunistic leaders. However, this case marks one of the few in which international intervention yielded significant results. In an extreme counter example, detente between Israel and its neighbors has delivered neither security for Israel nor statehood for the Palestinians. A permanent resolution to ethnic conflict in Sudan hinges rather precariously on the success of partition. Finally it appears that the ethnic insurgency in Sri Lanka ended in 2009. However, the conflicts non-accommodative and brutal outcome was clearly suboptimal. Ultimately it seems neither statesman nor ethnic nationalist have learned the intricacies of dispute resolution.

This book fulfills an important need and benefits from a careful and logical organization. However, on my reading it suffers from at least two serious limitations. First the authors' attempts to merge the International Relations and Comparative approaches to ethnic conflict fall short. Comparative perspectives on ethnic conflict tend to be more sophisticated than those found in the international relations literature, due to the tendency for the latter to treat ethnicity as epiphenomena. Therefore, the decision to simply list the approaches in the comparative and international relations literature (essentially equating the two) fails to place the two disciplines in conversation with one another.

Furthermore, if outcomes are actually contingent upon the interaction of factors at different levels of analysis, then simply listing the various factors is inadequate. Therefore a more fruitful approach would have been to use the levels of analysis framework to actually integrate the two literatures. For instance, realists find that a break down in state capacity can lead to a security dilemma among (particularly geographically isolated) ethnic groups. In addition, some comparative scholars have found that individual leaders often exaggerate perceptions of threat to suit their own instrumental objectives. Therefore, it seems that radicalization is probably a function of the interaction of systemic conditions and individual ambition. If this is the case, than opportunistic leaders are more likely to fan the flames of nationalism when state capacity is low. Such an explanation can only be understood by integrating international relations arguments with comparative theories.

Second, although it provides much needed structure, the "levels of analysis" framework becomes a liability when uninformed by decision rules governing its use. That is, its strict application to the cases can become more of a straightjacket than an analytical lens. In each case and at each level, the authors choose to include some elements while excluding others. Although this is an appropriate part of any analysis, they offer little guidance as to how these discretionary decisions are made.

Without a decision rule the amount of attention paid to some phenomena seems incommensurate with the importance of those elements in the actual conflict. Consider the role of gender in the North Ireland and Palestinian cases. In each it is given roughly equal treatment. In both cases, as in all ethnic mobilizations, women are intricately involved as the perpetrators of the identity group and in their roles as the initial educators of group members. The author's should be applauded for their attention to this generally overlooked element of conflict. However, in Palestine and Sri Lanka, the role of women moved well beyond that of caregivers and educators into the tactical realm. Their role as suicide bombers and the important cultural implications of their involvement has relevance for women as a group as well as strategic importance for the conflict process. The Irish case has no

equivalent. Therefore, my concern is that the tendency to treat gender with equal weight in each of these cases is perhaps a dictate of the “levels of analysis” approach and not necessarily the relative importance of the role of gender in these three conflicts.

In other instances, it is difficult to justify the inclusion of some elements of the conflict and the exclusion of others. For instance in the analysis of Bosnia Slobaden Milošević, Franjo Tudjman, and Alija Izetbegović are discussed at length, while other important figures are mentioned in passing or ignored all together. In fact, critically important figures such as Mate Bopan and Stjepan Kljuić receive scant attention. Certainly Bopan’s cunning brutality (e.g., the assassination of Blaz Kraljevic) influenced the success of Miroslavic and Tudjman’s goals of carving up the Bosnian state. Again a firm decision rule would help provide clarity.

Ethnic Conflict: A Systematic Approach to Cases of Conflict is a valuable attempt to integrate theories on ethnic conflict in order to more effectively conduct analysis of these disparate cases. As civil wars and political violence continue to outpace inter-state wars, it is clear that the need to analyze ethnic identity and its role in political violence will continue. A “levels of analysis” approach is perhaps a useful way to integrate these disparate approaches to ethnic conflict, but such an approach also runs the risk that it will become too inclusive. Every factor at every level may be interesting but each cannot be equally relevant if one hopes eventually to generalize across cases. Therefore the primary weakness is that the book provides little guidance that would allow one to adjudicate between theories of ethnic conflict, and as such does little to push theorizing forward. However, their comprehensive and thorough approach serves as an excellent rubric for navigating the complexities of ethnic conflict, and is perhaps most useful as a pedagogical tool for graduate or advanced undergraduate students.

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George C. Edwards, III. *Overreach: Leadership in the Obama Presidency.* Princeton, NJ: Princeton University Press, 2012. ix, 248 pp. (\$29.95 cloth).

For more than two decades George C. Edwards has been carefully laying out a clear and consistently articulated argument about the nature of American politics. In a series of books he has demonstrated that presidents best lead Congress “at the margins,” that when presidents go public their rhetoric generally falls upon “deaf ears,” and that presidents do not have the power to persuade. What is needed instead is a “strategic presidency,” one

based on a rational appraisal of the current political dynamics. Edwards's body of work is interesting in that each book builds upon the others. His newest book, *Overreach: Leadership in the Obama Presidency*, links his prior insights together and extends them into an analysis and critique of one incumbent president, Barack Obama.

Edwards' basic argument in *Overreach* is that Barack Obama and many of his top administration officials were convinced that the newly elected president had the ability to reach across the aisle and promote a spirit of bipartisanship. Obama was after all a talented politician with obvious political skills. Yet, the reality of Washington politics was that even as Obama took the oath of office, he faced a Congress with high levels of partisanship and polarization. He had won the 2008 election with but fifty-three percent of the vote, impressive but not overwhelming. After the election, various surveys indicated that American public opinion had not shifted in a liberal direction. Independents and conservatives were still skeptical of big government. His base of liberal Democrats remained a distinct minority of the overall electorate.

In spite of these cautionary statistics, Obama overreached by stacking his policy agenda with too many issues (health care, immigration, cap and trade, financial reform), each of which required the expenditure of considerable political capital. And while individual items on the agenda were undoubtedly important, many were or at least seemed to be unrelated to the realities of a collapsing economy. Edwards cites surveys that demonstrate that the public wanted jobs, not health care reform. And finally, Obama's agenda included a series of highly divisive issues, particularly comprehensive health care reform, that simply were not in accord with the demands of public opinion, particularly among the key group of independents.

Why then did Obama take on such a formidable agenda? First, Edwards notes that Obama was committed to carrying out the agenda upon which he was elected. Second, and by far Obama's biggest strategic mistake, was his belief that he possessed a unique ability to convince others to change their political stripes. Edwards documents Obama's various speeches and behind the scenes attempts at persuasion. Yet while Obama made Herculean efforts at promoting bipartisanship, the great irony of the Obama presidency is that the only measure of bipartisanship the president achieved was uniting Republicans together with disaffected Democrats to vote against the president's agenda. Given Obama's problems with overreach, why then did he succeed in passing some of his signature legislation? The answer is that while Obama failed at persuasion, he succeeded at the margins, winning over a few key congressional Democrats in the House and the Senate to vote for his legislative initiatives.

While Edwards acknowledges that Obama achieved a great deal, he also believes that the political cost to the Obama presidency was extra-

ordinarily high. Rather than one legislative accomplishment facilitating the achievement of the next, as the administration expected, overreach promoted broad and growing dissatisfaction with the entire Obama presidency. Lower approval ratings and rising voter anger translated into an historic repudiation of congressional Democrats in the 2010 midterm elections. As a result, Obama was unable to govern effectively during the remainder of his term. Edwards concludes that Obama would have been better served by adopting a smaller agenda and one that was less ideological. Edwards also makes a larger point related to the American presidency in general: *presidential power is not the power to persuade*. In this sense, his book is a direct challenge to Neustadt's age-old maxim. He offers instead that leadership is best served by effectively utilizing available political resources to get things done, particularly by building support at the margins, which requires fortifying one's own political base, not converting hostile partisans.

Edwards' work is fascinating, but his findings also create a bit of a dilemma for modern presidents going forward. Edwards suggests that there are few real opportunities for transformational leadership. And for those candidates who promise a wide-ranging agenda, Edwards suggests they are unlikely to succeed if the agenda is too vast or too ideological. Presidents should understand that to be successful they must take careful stock of the current state of public opinion, the allocation of partisans in Congress, and avoid the trap of believing that the newly elected incumbent has such extraordinary persuasive powers that he and soon she will be able to convince lifelong partisans to vote against their interests and for the president's agenda. It is a cautionary tale and one that presidents ignore at their peril.

There is a flip side to Edwards' advice, however. If presidents are wise to avoid risks, they also have sound reasons to be less bold, more cautious, and certainly both more analytical and practical. This may be sound political advice and I do not argue with it here. I do, however, wonder if the incentives for bold leadership are disappearing from the American presidency and if so, does this development usher in an era of circumspect presidents guided by analytical models, a largely technocratic and uninspiring presidency?

The answer is probably no because, if one reads between the lines, Edwards does not expect all presidents to adopt his more reasoned and rational approach. Edwards appears to believe that most presidents, convinced of the rightness of their agendas, and having won a national victory for the nation's highest office, will still be convinced that they alone have the personal persuasive abilities to change the national dialogue. If so, then we have not seen the end of presidential overreach.

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Scott Yenor. *Family Politics: The Idea of Marriage in Modern Political Thought*. Waco, TX: Baylor University Press, 2011. xiv, 385 pp. (\$39.95 cloth).

If an author is to be credited for wading into hotly contested waters with interpretations and arguments meant to invigorate an entirely new conversation about a perennial and perennially controversial topic, then Scott Yenor deserves a great deal of credit indeed. His project in this book is as ambitious as it is timely. It is timely because while the subjects of sex, marriage, and family have always generated cultural and public policy tensions and conflicts, recent battles over same-sex marriage and related issues, along with disquieting social science data about family structure, have contributed to a particularly acute sense of uncertainty and social vertigo with regard to the family. We see Yenor's ambition in both the scale of his treatment of key modern thinkers from Locke to Beauvoir, and in his attempt to begin a new conversation in which he takes issue with both progressive critics, or even abolitionists, of the family as well as more conservative forces who advocate for a retrenchment of an older iteration of marriage and family. In describing his work as a beginning, Yenor's ambition is wisely tempered. This work is meant as a sort of prolegomena, not a conclusion.

This book should certainly attract its own share of critics and interlocutors. Yenor describes our culture's marriage divide as a contest between those who believe the traditional family threatens individual autonomy and equality and those who defend the traditional family because of the benefits it provides for children and by extension the body politic. Darting in between the battlements of these factions, Yenor puts forth a view of marriage that is communal in nature and a good for its own sake. Some defenders of traditional marriage miss the mark precisely because they focus too much on marriage as a social institution and a mere means to social goods measurable by social science. Critics of marriage—Yenor has in mind feminists like Susan Moller Okin and Simone Beauvoir—overemphasize the drudgery of marital life at the expense of “genuinely satisfying marital love [that] creates mutual dependence” (p. 5). Mutual dependence, far from being an evil to be eradicated, is what makes love possible and ultimately it is a particular understanding of love that Yenor is attempting to establish as a corrective to modern theorists' enervated conceptions of marriage and family.

This thin conception did not happen all at once, and the majority of Yenor's book is devoted to chapter-length treatments of crucial figures beginning with Locke's contractual and thus dissoluble conception of marriage. Substantive treatments of Rousseau, Hegel, Mill, Comte, Durkheim, Marx and Engels, Freud, and Beauvoir follow, though understandably limited to each thinker's approach to the topic at hand. While specialists will

likely find objections significant and quibbling, Yenor manages to balance giving each thinker a substantive read with maintaining the narrative thread of his primary focus. Summarizing this multi-chapter narrative is difficult, but Yenor's story is one of marriage and family life undergoing a transition from being understood as rooted in nature to that of a human construct that can be consciously refashioned in the interests of prevailing norms such as autonomy, equality, and individual freedom. This story culminates in what Yenor describes as the anti-family and anti-marriage (and ultimately anti-love) thought of Beauvoir and Okin.

It is at this point that Yenor's introduces a two-part narrative twist into his story. In part one he transitions from his intellectual history of modern political thought about marriage to the empirical work of David Popenoe and other contemporary social scientists that identify themselves as part of the "marriage movement". This movement, also buoyed by the findings of sociobiology, has yielded reams of evidence linking human well-being with more traditional understandings of marriage and family. While Yenor does not think these arguments are entirely successful, the marriage movement provides him with his "hook" to link his theoretical outline to the troubled state that marriage and families are currently in.

In the second twist Yenor introduces John Paul II as a sort of response to the modern devolution previously described. One strength of the book is that Yenor's treatment of his authors is sympathetic, even of those with whom he has the strongest disagreement. He raises questions and points out difficulties without malice, but also without speaking in his own voice. The chapter on John Paul II seems meant to remedy this absence, as it not only introduces John Paul as a thinker to be considered in his own right, but sets up Yenor's most forthright expression of his own views in the final chapter.

Keeping in mind that Yenor's book is framed as the beginning of a conversation, these last chapters of the book comprise a sort of "reveal" that should elicit responses from theorists of various dispositions, particularly feminist thinkers. Given that every theorist considered by Yenor was, in some fashion, responding to or criticizing some manifestation of Christianity in thought or practice, bringing in John Paul II—the philosophically trained pontiff—as a dialogue partner is rather provocative to say the least.

The success of the task Yenor has set for himself is harder to gauge than its boldness or timeliness. It is hard to imagine a common argumentative framework in which Yenor and John Paul could make claims on, say, Beauvoir or Freud, and it's not clear that Yenor disagrees. Moreover, Yenor would have done well to do more to anticipate and respond to objections to his own invocation of Christianity. He notes, for example, that John Paul's conception understands marriage as the social institution which "buckles betrothed love to marriage, marriage to parenthood, and sex to procreation"

(p. 250). But if John Paul's understanding of marriage relies on "natural laws of operation," laws presumably authored by God, then it is unclear why such natural laws would need so much help "buckling" in the first place.

There are, of course, responses to this objection, and others, and perhaps Yenor will further the conversation by articulating these responses in subsequent work. A civil conversation about marriage and family is crucial so long as the divisions remain among citizens who nevertheless must live together. As it stands, Yenor's contribution is sure to please some and disappoint and even anger others. It is a provocative foray into a subject that is so contested because it is so important.

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Kristina C. Miler. *Constituency Representation in Congress: The View from Capitol Hill*. New York: Cambridge University Press, 2010. 224 pp. (£60, cloth).

Constituency Representation in Congress is a useful, thoughtful, and important addition to the literature on congressional representation. The main contribution of the book is marrying ideas from cognitive psychology with the literature on dyadic representation. In doing so the book provides a solid theoretical foundation for a number of relationships, that while not entirely surprising, largely rest on intuitions rather than being derived from an explicit theory—in this case, information processing and the use of cognitive heuristics.

The important takeaway of Miler's innovative work is that representation—viewed as how members of Congress perceive their districts—is incomplete and biased. The result is a strong prediction of unequal representation. That is, in spite of our common normative expectation of equal representation, given demonstrated human limitations in processing information in complex environments, Miler argues it is all but inevitable that some subconstituencies will be better represented than others.

Miler's focus is on what she calls the "congressional enterprise," which refers to an assumed shared congressional office perception of the member's district. It rests on the sensible argument that members are not lone decision makers but share beliefs and perceptions with staff. And it is the overall "enterprise" that provides representation of the folks, in the perspective of the book, the subconstituencies, back home. Based on this perspective, she develops an interesting data set derived from interviews with 81 congressional offices. In each of these she located the staff member responsible for

one of the two issues areas she investigates which are natural resources and health policy. Interviews focus on two specific bills in each of these policy areas and how they fared in the 107th Congress (2001-2003): The Patients' Bill of Rights and Medicare Regulatory Reform in the health care area and the Securing America's Future Energy Act and the North American Wetlands Conservation Act in natural resources.

The interviewing in each congressional office was restricted to one policy area so the data set includes 41 interviews about natural resource legislation, and 40 interviews on health policy. The staffers were asked in an open-ended fashion about who in their districts would be concerned about each bill. The main data source of the project comes from the answers to these questions, which and how many subconstituency groups are mentioned. Miler identified the subconstituencies which a complete listing would entail from a survey of the natural resource and health policy literatures. For example, for the Patients' Bill of Right she identifies the interests as business, physicians, patients, organized labor, senior citizens, insurance companies, and attorneys. Each of the four bills has such a listing and these seem perfectly reasonable, although one may question the implicit assumption underlying Miler's analysis that these interests are even remotely equally important in all congressional districts. But it is not unreasonable to assert that a full accounting of the interests in most districts would, indeed, include the interests she identifies.

The empirical analyses examine whether these subconstituencies are perceived, what affects whether these interests are perceived, and how those perceptions influence members' participation (speaking in committee or the floor concerning the each bill, and bill introductions) as well as the extremism of roll call voting in those policy areas.

On basic perceptions of the district, Miler finds congressional offices' views are incomplete and biased. They are incomplete because the set of subconstituencies named constitute only a small subset of those with potential real material interests in the bills. The argument here is that if a subconstituency is not perceived (mentioned in response to the open-ended query), its interests are probably ignored by the office. The results are biased because, as one would expect, whether a subconstituency is perceived depends in large part on whether it contributed to the members' campaigns, and the volume of communications the office received from the group. Unorganized, quiescent, interests, even when they exist in the districts in substantial numbers, are seldom perceived as relevant by the congressional offices.

Miler shows some ways in which perceptions of constituencies influence congressional policy behavior. One is participation. Along with being on the committees with jurisdiction, members are much more likely to

address the needs and concerns of the subconstituencies which are perceived by the offices. What is somewhat surprising is the pattern in which there are still speeches and statements on behalf of subconstituencies that are not perceived. In any case, being perceived make it significantly more likely that members participate on behalf of a group within the district.

The analysis extends to bill sponsorship. Here Miler reports support for the importance of subconstituency perception, but does so with a shift that looks at the district as a whole. She invokes the concepts of “completeness” (how many subconstituencies are identified by the office) and “balance” (whether groups on both sides of an issue are part of the office’s perceptions of the district). The hypothesis is that more complete perceptions of the district would yield electorally induced greater bill sponsorship. And it does, but only for sponsorship of bills in the natural resources area. Completeness seems unrelated to health care bill sponsorship. The same pattern occurs for balance: in spite of some previous work that would suggest that opposing subgroups within the district would lead to avoidance of a bill (so as to not anger any significant group), Miler reports that bill sponsorship is higher for offices with a balanced view of their subconstituencies, but only for natural resources. Balanced views of the constituency seem not to affect bill sponsorship in the health care area.

Finally, on voting Miler looks at roll call extremism using as measures interest group ratings in the two policy areas. More complete views of the district yield a bit more extremism while balanced perceptions yield the expected moderation. But this pattern holds only for resource policy; health care voting seems unrelated to district perceptions.

This unevenness of findings across policy areas remains an unresolved puzzle, but not a problem the author addresses in any systematic way which leaves the reader to assess just how useful the theory underlying the analysis is. I believe the theory is sound; it makes perfect sense and provides a non-cynical account for why those groups with resources and organization are better represented. It is understandable, given the challenges of data collection, that the number of offices interviewed and the number of policies covered were limited. My suspicion is that if coverage was more extensive, it is likely that the author would have found more support for important parts of her theory. As it stands, the limited data provide only partial support.

The perspective of the book is hyper-pluralistic, seeing congressional districts as sets of (potentially competing) subconstituencies, or what an earlier literature called issue-publics. Without denigrating the author’s important insights about biased representation, one can still wish she had addressed two important features of the modern Congress. One is the extreme ideological polarization of the parties that today seems to dominate almost every aspect of congressional policy making. Interestingly, this

polarization was quite visible in the final disposition of the bills considered, but this fact is barely discussed by the author.

The second aspect is the marked inequality in representation in which important works by Larry Bartels and Marty Gilens, each using different methodologies, show that congressional voting and policy outcomes are highly responsive to the issue preferences of the affluent and largely ignore the preferences of low income constituents. These patterns seem to fit with Miler's findings; her perspective could lend additional importance and theoretical depth to that work. If she had incorporated it into her discussion, our understanding of unequal representation would have been even more enriched.

While addressing the extreme partisanship of current congresses and congressional attentiveness to the preferences of their richer constituents would strengthen the book, it is nevertheless an original and useful contribution. It is certainly a "must read" for serious students of representation as well as those concerned about patterns of inequality in the U.S. policy process.

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The American Review of Politics is Seeking a New Editor

The American Review of Politics is currently published through the Department of Political Science at the University of Arkansas. The journal covers an array of topics in American politics, but has focused in recent years more heavily on political parties and Southern politics.

Given the wide range of articles included in the journal, the editor is expected to be an established scholar who understands the breadth of the American politics field and be conversant and knowledgeable about the diverse scholarship of this field. The editor is expected to be responsive to the individuals who submit manuscripts to the journal and able to provide constructive feedback to those individuals. In addition, the editor will be responsible for a variety of organizational and managerial functions associated with editing a journal. These will include appointing members of the editorial board, appointing associate or co-editors, appointing a book review editor, soliciting manuscripts for publication, and maintaining subscription and financial records for the journal.

The current publication cycle includes one print run that combines both spring and summer issues and a second printing including the fall and winter issues. The current editorial team will publish the journal through Volume 34, which will cover the Fall 2013 and Winter 2014 issues. The new editor and staff will be responsible for soliciting manuscripts for and publication of the Spring and Summer 2014 issues.

Interested individuals should provide a vita, a statement of interest describing their vision for the journal, and a letter of support from the host institution. Proposals will be considered after December 15th, 2012, and a decision will be made as soon as possible after that date. Please contact Andrew Dowdle, 428 Old Main, Department of Political Science, University of Arkansas, Fayetteville, AR 72701 or via email to adowdle@uark.edu.