voting, were strategically deployed to maximize partisan gain. Indeed, Engstrom's accounts of redistricting events in specific states illustrate how the manipulation of electoral rules and maps was a regular part of campaign strategies in most electoral cycles from 1840 to 1900. He concludes that members did not develop a "home style" when they knew "that the legislature can, and likely will, carve up your district in two years" (p.187).

Still, adherents of the "minimal gerrymandering effects" thesis may not be convinced by the author's analysis of the impact of statewide districting plans on the composition of the House of Representatives. On the surface, the frequent turnover and instability observed in state congressional delegations suggest that efforts to entrench partisan majorities were not very effective. Engstrom identifies only two elections during the partisan era, 1878 and 1888, in which gerrymandering likely determined party control of the House. However, his empirical analysis demonstrates how state parties reversed previous disadvantages and imposed their own biased plans for Congress, even if short-lived.

Engstrom accomplishes his primary goal in Chapters 5 and 6, demonstrating that partisan redistricting has profoundly shaped both electoral and policy outcomes in the House, and by extension the construction of American democracy. Arguably his most important discovery is that the nineteenth-century gerrymanders were primarily "dispersal" or "efficient" gerrymanders, which attempt to create a microcosm of the state in each district, allowing a majority party to sweep the election, rather than the "packing" of opponents into inefficient districts that we increasingly see today.

From 1840 to 1900, the average *bias* for Democratic-drawn plans was 8.25, meaning that if Democrats received 50% of the statewide vote, they would receive 58% of the seats. Republican-drawn plans were biased in their favor by an average of 5.7 points (Table 5.2). But both types of plans were *responsive* to a degree of 4 points or more, meaning that a 1% vote increase/loss would yield a 4% seat increase/loss. Compare these averages to the gerrymandering that occurred after 2010, according to *Gerrymandering in America* (2016) by Anthony J. McGann et al. Overall bias nearly tripled to a 5.02 point advantage for Republicans in 2012, and was likely responsible for their House majority, but responsiveness was much lower compared to the 19th Century, 1.52, and virtually unchanged from the previous plans. By shaving district majorities so thin in order to maximize efficiency, bias in nineteenth-century plans was amplified by their hyperresponsiveness, so that a gain (or loss) of just a few points would result in a massive seat swing.

How massive? In the "critical" elections of 1854, 1874 and 1894, widely understood as historically transformative expressions of public demand against the status quo, Engstrom shows (in Fig. 6.4) vote swings ranging from 6.8% to 7.6% against Democratic (1854, 1894) or Republican (1874) controlled Houses, substantial but not historic. Yet these three elections handed seat losses exceeding 30% to the governing parties, among the largest seat losses in congressional history. The author cleverly demonstrates the impact of hyperresponsiveness through a simulation of vote-to-seat translations using standard seat/vote ratios, which would have resulted in much less severe seat swings.

This is a major revision of electoral history, a revision that elegantly links the impact of mobilizing events, namely, the 1854 passage of the Kansas-Nebraska Act, the economic Panic of 1873, and the Panic of 1893, to "manufactured competitiveness, an overabundance of marginal seats created by redistricting, and a vote swing against that current dominant party" (p.128). Additionally, Engstrom's simulations demonstrate that partisan gerrymandering played a central role not only in providing Democrats elected in 1852 with the votes they needed to repeal the Missouri Compromise, but also in the federal abandonment of suffrage rights in the South after 1874, and repeal of the Federal Enforcement Act in 1892. He makes a persuasive case that our political history might have played out much differently were it not for these hyperresponsive gerrymanders.

In its entirety, *Partisan Gerrymandering and the Construction of American Democracy* provides an electoral life history of the House of Representatives. That is a major accomplishment. Yet the significance of this research extends beyond the past, subsuming our dominant theory of incumbent office seeking into a broader theory of partisan competition. Engstrom provides a framework for both electoral scholars and practitioners to better understand what the reemergence of partisan polarization, increased manipulation of redistricting in the absence of judicial restraints, and restrictions on voter registration and participation mean for the future of American democracy. In this era of renewed attention to what constitutes an unconstitutional partisan gerrymander, and how we might implement judiciable standards, Engstrom provides an important contribution to a growing body of research that urges us not to be dismissive about the political consequences of institutional choice.

How Policy Shapes Politics: Rights, Courts, Litigation, and the Struggle Over Injury Compensation. By Jeb Barnes and Thomas F. Burke. New York: Oxford University Press, 2015. 272p. $39.95 cloth.
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— Ken I. Kersch, *Boston College*

Its broad title notwithstanding, this book addresses a focused question involving the respective virtues, vices, and politics of bureaucratic versus adversary legalist modes of governance. The former operates according to formal, hierarchically determined rules and procedures that value consistency and socializing costs. The latter proceeds by privately initiated, (past) event, and bilateral litigant-focused disputes resolved by blame-assigning, triadically structured courts of law. As such, the book is part of an extensive academic literature on the pros and cons of adversary legalism. That literature was initially motivated in part by the claim that adversary legalist modes of governance were atypically prominent in the United States—allegedly to ill effect. It was subsequently fueled by the observation that the rest of the world seemed to be moving in the same direction ("judicialization").

Since it was launched in earnest in the 1970s, this literature has become so voluminous and sprawling, and flies under so many different guises, that Jeb Barnes and Thomas Burke are concerned that it is no longer clear what we know and whose findings are relevant to whom. Not the least of the accomplishments of *How Policy Shapes Politics* is that in its opening chapter, the authors hack their way through this dense overgrowth, skillfully cutting, pruning, organizing, and systematizing. They then move on to the rigorous and methodical testing and assessment of that literature's core question. This is an adjudicatory study of adjudication.

The authors approach their comparative accounting of the relative virtues, vices, and politics of bureaucratic versus adversary legalism through an ingeniously designed study of injury-compensation regimes. They justify their choice of this policy domain as a "hard case" test of the literature's reigning understandings of adversary legalism—because injury compensation is inherently redistributive, and thus especially likely to prove highly contentious, thus confirming the literature's conventional understandings. While this book may seem on first blush to be a case study, it is actually a painstakingly designed comparative assessment: This well-chosen policy domain provides a range of variation along an array of axes.

Barnes and Burke examine three injury-compensation regimes: Social Security Disability Insurance (SSDI), harm from asbestos exposure, and harm from vaccines. These vary by their orienting institutional structure (bureaucratic versus adversary legalist, with due recognition that no regime fully tracks ideal types) in ways that both are durable and also alter developmentally across time (inception, expansion, revision, reform, retrenchment, shifts in salience, and interaction effects involving related initiatives and institutions within the same policy environment). As such, the study is structured by cross-sectional and longitudinal comparisons. The book is anchored in intricate but clear narrative accounts of the creation and temporal evolution of each of these three injury-compensation regimes, in conjunction with well-targeted, data-driven quantitative comparisons and assessments.

Like others before them, Barnes and Burke find "sharp and clear differences between the politics of adversary legalism and the politics of bureaucratic legalism" (p. 190). Their major contribution here is to demonstrate that, in this important area at least, those differences do not track the premises of most of the current academic literature on adversary legalism. That literature, they explain, has frequently argued (asserted, even) that the turn to litigation as a form of policymaking crowds out other forms of goal-directed political participation, like lobbying and electoral and social movement mobilization. So far as injury compensation is concerned, they find that this was not the case. The literature has argued that adversary legalism is highly path dependent—it tends to lock in early policy determinations on a set course, blocking adjustment and evolution. Barnes and Burke find, however, that when it comes to injury-compensation regimes at least, adversary legalism is no more path dependent than bureaucratic legalism; indeed, consistent with patterns typical of common law evolution, it was arguably even less so. The literature has argued that adversary legalism is especially susceptible to triggering "a polarizing backlash that hinders the creation of diverse coalitions" (p. 196). Again, not true, the authors find.

The one claim of the literature for which the authors find support is that "by organizing social issues as discrete conflicts between individuals, adversarial legalism individualizes politics, undermining social solidarity." Here, they confirm the literature's findings, but offer a refinement by observing that adversary legalism individualizes and fractures through two chief mechanisms: distributional effects (creating "unequal, unpredictable, and unstable costs and benefits") and blame assignment (p. 196).

This—and here the book's title, *How Policy Shapes Politics*, is most apt—sparks conflict and contention. The authors' data show that participation in hearings on adversarial legalist policies in Congress is more diverse—indeed, "a melee, in which a range of groups descend on Washington, representing a mix of business, claimants, consumer groups, legal experts, and lawyers from both sides of the payer/payee divide" (p. 46)—while that on bureaucratic legalist politics is "spikier" (p. 43) (as mapped on a radar graph) and "much more sedate" (p. 47), largely dominated by government officials, on the one hand, and claimants, on the other. These are significant, substantive findings, which any scholars writing in this area will need to assimilate or address.

Barnes and Burke devote considerable care and attention to matters of research design, measurement, and method, and the book aspires to influence on this score as well. The authors creatively collect and assess data on interest-group participation in relevant congressional hearings, as divided by type (hearings on bureaucratic politics, hearings on adversarial politics, "referral hearings," "budgetary hearings," and "oversight hearings" [pp. 34–35]). They make sense of that data not simply in crude terms but in ways that take into account the character of the congressional committee (as mapped by seniority, ideology, and other relevant dimensions) and the hearing's conflictual versus consensual dynamics. And, critically, the authors do so across a 40-year time span. Departing from the atemporal assumptions of much of the empirical literature in the law and courts subfield, this book boldly confronts the public policy landscape of "downtown Tokyo" (in Karen Orren and Stephen Skowronek's vivid metaphor)—that is, in the world as it is, with all of its temporally constituted layerings, intercurrences, interactions, and complexities (what Paul Pierson has called "politics in time").

In significant part because the authors bring to bear both a deep, substantive understanding of the history and politics of these policy areas and of the political science of public policy more generally—they are substantively knowledgeable generalists—their measurements are much more finely calibrated than is typical of recent empirical work in the law and courts subfield, which problematically aspires to ever-greater precision in measurement through increasing technical sophistication, at the price of a decreasing substantive knowledge and conceptual facility. Barnes and Burke's (quantitative) measurements offer an impressive contrast: They are laser guided, and a model for future (mixed-methods) empirical work in this area—and, one hopes, in others.

The authors scrupulously itemize their study's potential limitations, including questions about generalizability. But their ambitions are grand: In concluding, they call for a new departure in the study of the politics of judicialization, characterized by precision and consistency in definitions and the broad adoption of a comparative/developmental approach (both of which, of course, they impressively model here). Their hope is that the long period during which scholars of judicialization "speak past each other rather than learning from each other's efforts" is over, and that there will be "greater collaboration among scholars interested in studying the consequences of burgeoning judicial power" (p. 199).

While this is a book about social insurance and regulation and not about everything, Barnes and Burke ask an important question, and design and execute an exceptionally well-specified study to arrive at a new baseline with which all future research in this area will have to reckon. *How Policy Shapes Politics* represents a major step forward in sorting out the nature, causes, and consequences of litigation as a form of governance. Students of Congress, interest groups, political voice and representation, public policy, and the temporal dimensions of politics, moreover, will also learn a lot from this book—not least about substantively motivated and empirically sophisticated mixed-methods research design. To the extent that real-world politics can be understood scientifically, this is an impressive attempt to do it well.

Presidential Swing States: Why Only Ten Matter. Edited by Stacey Hunter Hecht and David Schultz. Lanham, MD: Rowman & Littlefield, 2015. 386p. $115.00 cloth, $49.99 paper.
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— Helmut Norpoth, *Stony Brook University*

Swing states are the Holy Grail of American presidential campaigns. Target the right ones in a given election and victory will likely be yours. For students of American politics, the major challenge is to explain this phenomenon. What gives some states "swing" status? Or as the subtitle of this timely volume tantalizingly puts it, "Why Only Ten States Matter." This is a team effort of nearly two dozen experts on national and state party politics—too many, unfortunately, to cite each of them by name here. Bookended by introductory and concluding chapters written by editors Stacey Hunter Hecht and David Schultz are chapters on each of these ten states by various contributors; there is also a broadly framed chapter on campaigns and voters in swing states, plus one chapter each on two borderline swing states.

To evaluate any study of the swing-state phenomenon, one has to assess, above all, how states were selected for inclusion in this distinctive group. How valid and reliable is the measurement of swing states? The editors lay out three criteria: competitiveness (defined as a vote margin of 5% or less), bellwether status, and incidence of flipping, all during the last seven presidential elections (1988–2012). These are reasonable guideposts, but there is no indication of how frequently a state had to meet each of the three criteria to merit selection as a swing state. In fact, the select list presented in Table X.2 is largely the same (minus two) as an initial one suggested by "the political science literature, media accounts, and campaign activity" (p. xxx). The vaunted swing states of this volume are largely the ones of conventional wisdom. The formal criteria simply help weed out a couple of suspects. The editors are quite frank when they admit, though not too elegantly, that "states that demonstrated more as opposed to less of these criteria became the basis for inclusion as swing states" (p. xxxi).

Spoiling any suspense, here is the chosen list of swing states: Colorado, Florida, Indiana, Iowa, Missouri, Nevada, New Hampshire, New Mexico, North Carolina, Ohio, Virginia, and Wisconsin, hence two more than the 10 alluded to in the subtitle. It would have been less confusing had the subtitle made it a round dozen or if the two add-ons (Indiana and Missouri) had been dropped altogether. Without much doubt, Indiana is a long shot. It qualifies neither as competitive nor as a state that flips often; only once between 1988 and 2012 did it deviate from being a GOP lock. On the face of it, even some of