

Shaw emphasize long-standing predispositions in every chapter, noting that when viewed in terms of partisanship, the American electorate appears much more stable and predictable than popular portrayals would otherwise indicate. The authors retain this big picture focus as they join others in debunking/clarifying the notion of a “divided” America: the empirics of chapter three reveal that such claims are overstated, and that to the extent polarization does exist, that it is due more to “general group attachments and aversions” than issue positions (64).

The middle chapters move on to more specific questions, first considering “swing voters,” or the portion of the public that is supposedly up for grabs in any particular election. Specifically, the authors concern themselves with defining this elusive group, investigating its size and demographic profile, and determining whether its members really break for the challenger consistently (as media reports suggest). A nice touch comes in the authors’ presentation of a careful distinction between swing voters and “undecideds,” and they ably demonstrate that swing voters cannot be neatly categorized by catchy terms like “soccer moms” and “rural voters”; they find no empirical support that such voters consistently break in a particular direction. Chapters 5 and 6 dovetail nicely, tackling a series of questions related to the gender gap and young voters, respectively.

In the final third of the book, the authors tackle turnout and campaign effects before offering some thoughts on the 2008 presidential contest. The examination of partisanship and turnout is engaging and well-presented—in what is one of the stronger chapters, Kaufman and colleagues convincingly dispel the notion that higher turnout benefits the Democrats. Moreover, they demonstrate this under several conditions, all the while making a case for why the question matters: indeed, the myth of a connection between high turnout and Democratic victory has had negative consequences for democratic practice, with legislators often engaging in partisan opposition to proposals that could potentially facilitate political participation (by altering registration and voting procedures).

Most political scientists will not be particularly surprised by the “myth busting” that goes on in *Unconventional Wisdom*, and some may question the choice of topics. Although these decisions are well-defended (and something that the authors tackle from the outset), one cannot help but wish that a few other areas were addressed—for example, religion is only mentioned in the context of the gender gap (and then only in passing), and the notion of realignment seems

like an ideal candidate for inclusion given its intuitive appeal, perennial popularity, and close ties to the theoretical core of the book: partisanship.

Still, there is much to like in this effort. Although the empirical analyses are written toward a general audience, the appendices contain the details of the modeling, and the endnotes hold thorough handlings of numerous literatures relating to elections, public opinion, and voting behavior. The book is perfectly suited for an undergraduate course, but it could also make a nice addition to a first-year graduate seminar. Thoughtful and entertaining, Kaufmann, Petrocik, and Shaw strike a nice balance between popular culture and academia, and one cannot help but appreciate their handling of American elections, and their public defense of why political science matters.

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*Redistricting and Representation: Why Competitive Elections are Bad for America.* By Thomas L. Brunell. (Routledge, 2008.)

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There is simply one word that best describes Thomas Brunell’s *Redistricting and Representation*: provocative. At least since the 1990s round of congressional redistricting, the vast majority of political scientists have advocated for an infusion of competition in U.S. House elections. Indeed, going back to the vast literature on the incumbency advantage that caught scholars’ attention in the 1970s and even before the existence of the American Republic, political competition has enjoyed a privileged position—one of the core principles believed to sustain a healthy democracy. In the most revered of the *Federalist Papers*, Essay No. 10, Madison argues that pluralism is the lifeblood of American representative democracy. Now enter Brunell, who turns a couple centuries worth of conventional political wisdom on its head. Instead of bolstering the competitiveness of U.S. House districts (and by extension state legislative districts), Brunell advocates packing them to the hilt with as many ideologically like-minded voters as geographically possible. Why? Because “competitive elections are bad for America.” Competition produces an inferior form of representation since it maximizes the share of voters who will be disappointed with the political outcome.

Brunell’s thesis is quite simple, but the prescription is probably viewed as radical by most political

observers. Starting with the premise that representation improves to the extent that the officeholder shares the views of her voters and acts on their behalf, it follows that districts should be drawn so as to maximize the number of voters with shared political preferences. Since members of Congress either vote “yes” or “no” on legislation, why not draw districts so that the proportion of voters who disagree with the actions of their representative is minimized? Using survey data from the American National Election Studies cumulative file, Brunell shows that there are significant differences in the political attitudes of voters, depending on whether their preferred House candidate won or lost the election. Voters whose candidate won are much more satisfied with their incumbent and also more approving of Congress as an institution. In addition, the greater the margin of victory the more satisfied are voters who supported the winning candidate whereas it has no effect on the political attitudes of voters whose candidate lost. So based on the empirical evidence Brunell recommends that districts be drawn to enhance voter satisfaction and this is done by increasing the number of winners—voters who supported the victor, and thus reducing the number of losers—voters whose candidate lost.

One of the more interesting subjects that Brunell discusses is the role of responsiveness. Responsiveness is viewed in two different ways: (1) the degree to which the representative satisfies the demands of constituents and (2) the number of seats won by a political party depending on its percentage of the vote. First, consider representative responsiveness. Contrary to the popular view that members of the most competitive districts are the most responsive, Brunell contends that this cannot be true with respect to taking stands that are congruent with the positions held by most constituents. If by definition, the most competitive districts are the most ideologically diverse then their representatives are necessarily the least responsive because they cannot take positions that satisfy the largest number of constituents. In addition, the fear that representatives who face nominal competition in general elections will be less responsive is unfounded because the locus of competition will naturally shift to primary elections.

As for the second conception of responsiveness, typically called the seats-to-votes ratio by redistricting scholars, Brunell argues why it is beneficial to minimize this. In a single member district (SMD) system the seats-to-votes ratio typically exceeds 1, which means that the party that wins the most votes

usually wins even more seats than their percentage of the vote. Contrary to the SMD arrangement, in a proportional system, the seats-to-votes ratio equals 1 since each political party gets the percentage of seats that corresponds with their percentage of the vote.

Brunell advocates packing districts with lopsided partisan ratios on the order of 90% Republicans (Democrats)/10% Democrats (Republicans) or 80/20, etc. because this would reduce electoral responsiveness, which in turn reduces the probability that elections result in a less preferred outcome—the election of the minority party candidate. For instance, Brunell presents a hypothetical scenario of a state that is equally balanced between the political parties (an electorate with 50% Democrats and 50% Republicans) and has a total of four congressional districts. Under the competitive plan the districts are drawn so that each party has an equal chance of winning all four districts. By contrast, under Brunell’s “fair partisan plan” two districts are packed with as many Republicans as possible and the other two are stuffed with Democrats. According to Brunell, this is a superior arrangement because it reflects the underlying statewide partisan distribution and makes it highly unlikely that every district is won by the same party, which would of course result in the greatest number of unsatisfied voters.

Not surprisingly, Brunell has plenty of critics, which he makes apparent in the preface. In fact, among political scientists and redistricting scholars in particular, Brunell is practically alone in forcefully advocating for less competition in redistricting plans. One can certainly disagree with his position and poke holes in parts of his argument, e.g., voters are perhaps more dynamic than Brunell portrays them to be, why is a partisan vote considered superior to a personal vote?, is it preferable to maximize the delegate role of our representatives?, and does it make sense to pursue proportional representation in single member district systems? Nonetheless, *Redistricting and Representation* is required reading because it takes issue with a canon of political wisdom: that electoral competition is a sacred democratic principle. Even if Brunell’s work fails to make converts, it will have performed an important service to the discipline by exhorting us to reconsider many of the core beliefs we hold with respect to the representational consequences of redistricting plans.

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