government and trusting it less, might help explain their outsized influence on the Republican and Democratic Parties, respectively (“Sixties Civics,” in Sidney Milkis and Jerome Mileur, ed., The Great Society and the High Tide of Liberalism, 2005). After Reagan recruited the Christian Right as a critical ally in his push for a national conservative offensive, the Republican Party no longer wanted to roll back the state. Rather, it became committed to redeploying state power in the service of conservative objectives: fighting communism and upholding “traditional” values (Nicholas J. Jacobs, Desmond King, and Sidney M. Milkis, “Building a Conservative State: Partisan Polarization and the Redeployment of Administrative Power,” Perspectives on Politics, 17 (2), 2019). With Barack Obama’s election as the first African American president, the Democratic Party became identified with the causes that animated the Great Society, strengthening, at least at the presidential level, what Ronald Brownstein has called the “Coalition of the Ascendant”: millennials, minorities, the LGBTQ community and educated professionals, especially women (Brownstein, “The Clinton Conundrum,” The Atlantic, 2015). Obama grappled the scepter of partisan leadership somewhat reluctantly; however, in the face of fierce Republican resistance, which cost Democrats the House in 2010 and the Senate in 2014, he began to use the powers of the modern executive to advance policies centered on the economies and the accompanying inequality in a Diversifying America (By Bernard L. Fraga. Cambridge: Cambridge University Press, 2018. 286p. $105.00 cloth, $26.99 paper. doi:10.1017/S1537592719003219)

It has been more than four decades since scholars first sought to understand why voter turnout has declined despite increasing levels of educational attainment and relaxed registration laws. This “puzzle of participation” was famously introduced by Richard Brody (“The Puzzle of Political Participation in America,” in Anthony King, ed., The New American Political System, 1978). The implications of this puzzle continue to loom large for political campaigns and scholars interested in the study of American democracy. These same scholars should similarly pay attention to Bernard Fraga’s important new book, which seeks to understand an equally perplexing puzzle of why the gap in voter turnout between white and nonwhite voters has persisted in the United States.

Despite the removal of de jure and de facto barriers to nonwhite electoral participation, and an initial decrease in the turnout gap in the 1950s and 1960s, Fraga finds that this disparity actually increased in the 10-year period between 2006 and 2016. He draws not only on a comprehensive range of existing sources to present his case, but also on 190 million individual voter registration records from 2006 to 2016 to compare the effects of place and turnout. The specificity with which Fraga is able to consider the consequences of geography and population concentration on the turnout gap between whites and nonwhites is noteworthy. His theory of electoral influence and the accompanying empirical analyses not only complement and supplement established theories of political participation but, more importantly, also challenge the reader to consider the ways in which voting is not simply an individual act but rather one driven by contextually contingent behaviors.

The book can be divided into three sections. Rather than beginning by extensively describing the book’s most ambitious theoretical concept of “electoral influence” and clarifying its constituent parts, Fraga astutely explores the relationship between race and turnout in historical context. This chapter should be a primer for students of political participation. Whereas others may briefly reference the history of laws that sought to explicitly exclude nonwhite voters from politics and the subsequent legislative and judicial interventions that

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expanded voting rights in the 1960s and 1970s, Fraga begins with this exploration as a necessary first step to understanding patterns of the turnout gap over time. Chapter 3 similarly considers sociodemographic factors as the most prevalent variables believed to affect voting, and whether they can explain the turnout gap between white voters and minorities. One of the unexpected findings is that Black, Latino, and Asian American citizens who are young, low income, or less educated vote at higher rates than their white counterparts. This finding is important given that the strategic nature of political outreach leads to negligible mobilization of these less-resourced communities. That means that these nonwhite voters are voting in spite of the neglect, not because of mobilization by political campaigns and parties.

The second section consists of three chapters. In chapter 4, Fraga draws on existing literature to explain variation in racial/ethnic turnout and finds that, even though extant work can shed light on some aspects of what motivates individual groups to participate, there is no unified theory of voter turnout. He seeks to bridge the contributions of the Downsian calculus of voting, the empowerment theory, and elite mobilization. His theory of electoral influence “is built on the notion that political behavior of both citizens and elites is a product of the electoral context they find themselves in” (p. 81). The intuitive nature of this proposition is not altogether novel, but what is unique is the operationalization of individual empowerment, group relevance, and elite mobilization, as well as the possible cyclical relationship of these three turnout perspectives with electoral institutions, party coalitions, voter turnout, and his key indicator of electoral influence: relative group size. The notable conceptual contribution of the book is that it is not focused on individual turnout, but rather on the turnout gap, which is a zero-sum measure of participation that is “a measure of political (in-)equality between groups” (p. 94).

Having made the case that variation in turnout can matter, chapter 5 harnesses the utility of nearly 200 million voter records to determine where it does matter. The key takeaway here is that relative group size affects turnout in distinct ways across different geographic units and across the three racial minority groups. The significant relative group size of Black eligible voters in the South reduces the Black–White turnout gap in that region, with no similar regional effects for Latino or Asian populations. Where this new geography of the turnout gap is increasingly evident across all three groups is at the state and county level. Though the strength of the relationship varies by group, it still lends support to the notion that the turnout gap with Whites is smaller where there is a higher concentration of a given racial/ethnic group. Chapter 6 tests for this impact in congressional districts, given that these units are not static and the politics of redistricting matters. First, Fraga finds that the minority–White turnout gap is smaller in districts where minority groups compose a substantial portion of the potential electorate, not whether there is a co-ethnic candidate on the ballot. He then considers the causal connection of group size and turnout by matching similarly situated voters before and after redistricting, using a sample of the larger data and tracking the participation patterns of those five million voters over a 10-year period. He finds that the Black–White and Latino–White turnout gaps do shrink on placement into majority-Black and majority-Latino districts, but that this effect is largely due to sharp drops in White turnout in those districts.

The final two chapters are useful in that Fraga looks at possible effects of modern election policies (chap. 7), the future of minority turnout, and the possible political consequences if there was no turnout gap (chap. 8). The conclusions in both chapters are more tentative and hypothetical, given the lack of appropriate data to make strong causal connections. It should not be surprising that the lack of ideal data here and in other chapters in the book moderates his assertions of the causes of the gap between minority and White turnout. However, it was unclear why he opted to include data that, by design, are not intended to measure this gap. He claims to have considered the turnout gap since the 1940s, yet woefully small samples of minority populations in the ANES throughout most of its history inherently prevent any conclusions from being asserted about non-Whites. Although the ANES has been useful for theory building and testing political behavior and attitudes among Whites, it is not necessarily useful for measuring minority behavior, including turnout. A critique of the ANES and other such data would have been warranted not for the sake of being critical, but because it gets at the heart of the determinants of political behavior and one of the key variables in the book: elite mobilization. Failure of the ANES to survey Latinos and Asians, and to a large extent Blacks, is indicative of the perception that minorities are not consequential. If social scientists did not find these groups meaningful, why should we expect political parties and campaigns to do so?

These criticisms should not detract from the value of The Turnout Gap. Fraga skillfully identifies the disparity in turnout by race and demonstrates its significance with respect to group size across a variety of geographic units. His theory of electoral influence is not simply an empirically driven explanation, but one grounded in historical context. The obvious challenge to the discipline is to further probe why, despite the partisan capture of Black voters, the neglect of Latino and Asian voters by political campaigns, and the

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obstruction to voter registration of all minorities, the
turnout gap is not larger.


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Largely lost in the narrative of the 2018 midterm election as a referendum on President Trump’s tenure was the presence of 62 initiatives across 20 state ballots. The absence of these citizen-initiated policy proposals from the national discourse represented a deviation from the prominent attention that at least a handful of these opportunities for voters to bypass elected officials and directly legislate themselves usually garner, attention that is often due to expectations that the ballot measures might increase turnout and alter the electorate’s composition (such as occurred with efforts to ban same-sex marriage across states in 2004). That anticipated positive impact on the decision to vote is frequently cited by proponents of this institution as reflective of its capacity to foster greater political involvement and better equip individuals for that participation. In their important challenge to these expectations, Joshua Dyck and Edward Lascher paint a much more pessimistic (and they argue realistic) picture of direct democracy’s effect on civic engagement.

A prominent line of research, which weds arguments from the Progressive Era (when most of the 24 states that represented a deviation from the prominent attention that at

First, campaigns will target peripheral (or casual) voters uncertain about turning out for the election but who are sympathetic to the initiative’s outcome. This will generate higher turnout but not a corresponding spike in political interest or voter registration rates, and will widen the partisan gap in participation. Second, the expansion of conflict will make information about the parties’ stances on the issue more easily accessible (because they or their candidates will voice their preference) and thus enhance, not reduce, the influence of partisanship on vote choice. Third, that expanded conflict will serve to reduce trust in government, because mobilizing agents have an incentive to prime government incompetence and signal that legislators are failing to do their jobs.

After establishing this framework, the authors turn to empirically supporting it. Here Dyck and Lascher exploit a host of survey data that are well suited for this task due to their large samples representative at the state level (an improvement over prior studies relying on the American National Election Study) and validation of voter turnout (to overcome concerns about bias in self-reported turnout). Consistent with their expectation that peripheral voters will be targeted, they identify higher participation rates only in midterm elections (when there are larger numbers of these voters) and no corresponding increase in levels of political interest or rates of voter registration, with turnout effects more concentrated among partisans and noting that many seem somewhat tenuous. For example, the notion that citizens desire to take on the greater participatory responsibility created by direct democracy contrasts with other research showing that people often find engagement unpleasant. Similarly, Dyck and Lascher assert that “there is little cause to think that simply making more decisions at the ballot box prompts people to feel better about their own participation capacity or about their elected officials” (p. 23). This is particularly true because initiatives need not (and frequently do not) address citizens’ primary concerns—those matters with the greatest potential to draw individuals into the political process. Rather, the difficulties related to enacting policy via the ballot (such as financial costs and logistical issues) often lead to placing atypical (and extreme) matters before voters, generating at best a weak connection between citizen concerns and on what they can vote.

Instead, the authors contend that the story is much simpler. In expanding the scope of the conflict around an issue, initiatives provide for a more active campaign environment by creating an incentive for supporters and opponents to activate those currently on the political sidelines. That mobilization, however, can be (and is) done without enhancing citizens’ civic capacities or making them feel better about those capacities, as improvements to those engagement metrics are unnecessary to get them to the polls. This scenario yields a number of consequences that are contrary to the expectations of the educative effects thesis. First, campaigns will target peripheral (or casual) voters uncertain about turning out for the election but who are sympathetic to the initiative’s outcome. This will generate higher turnout but not a corresponding spike in political interest or voter registration rates, and will widen the partisan gap in participation. Second, the expansion of conflict will make information about the parties’ stances on the issue more easily accessible (because they or their candidates will voice their preference) and thus enhance, not reduce, the influence of partisanship on vote choice. Third, that expanded conflict will serve to reduce trust in government, because mobilizing agents have an incentive to prime government incompetence and signal that legislators are failing to do their jobs.

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