

In the first portion of the book, we are informed that there is “a public finance apparatus” and that it consists of the Treasury, the Fed, the Office of Management and Budget, the Council of Economic Advisers, and the National Economic Council. But some of those were created during the study period. Why were they created? Usually that question is addressed in the second portion of the book. How did the new institution affect the president’s ability to control the policy area? What other agencies with new and expanding functions and related interest groups also affected the president’s ability to control public finance policy? These are “second-part questions” that very much deserve our attention.

To be sure, the second part is not detached from the initial conceptualization. Sometimes those references are less helpful than one would hope. The analysis of the first part shows that *within eras*, there are shifts in policy problems, their salience, and patterns of policy outcomes. So applying the label “Roosevelt era” in the second part may suggest a coherence that is overstated.

Twice, O’Brien characterizes a president as “relinquishing control” of the public finance policy apparatus to their predecessor. Truman relinquished control to Roosevelt. George H. W. Bush relinquished control to Reagan. Apparently “control” in this context means the definition of policy goals and objectives. O’Brien’s point is that the successor president embraced more or less the same objectives (i.e., echoing Stephen Skowronek). But this notion of control seems quite unlike the “control of administration” discussed in Part 1.

A hallmark of the “Reagan era,” O’Brien writes, was the “restructuring of the subdomain of monetary policy” (p. 181). But, O’Brien also describes Reagan’s reinforcing and carefully respecting the Fed’s independence. Rather than being an interesting theoretical surprise—strategic renunciation of control—O’Brien says that in doing this “Reagan strengthened his control” over monetary policy (p. 181). How does this demonstrate strengthened *control*? Reagan and his close advisors were often unhappy with the Fed. Certainly, Volcker was unhappy with the Reagan administration. Nonetheless, by establishing credibility in fighting inflation, the Fed’s independence and standing were greatly enhanced. This enhanced independence did constrain subsequent presidents.

As for Barack Obama, it seems dubious that Obama’s public finance policies were constrained primarily by “the apparatus” rather than by Congress working with well-organized financial interests (see chap. 12). True, the Dodd-Frank Act did *not* significantly simplify the fragmented regulatory structure. But wasn’t that primarily due to organized interests working through Congress rather than the public finance apparatus? Were members of the apparatus plainly wrong in forecasting that Congress would not accept more fiscal stimulus? In Dodd-Frank, consequential new institutions *were* created including the

Consumer Financial Protection Agency and the Financial Stability Oversight Council. Important new regulatory authority was given to the Commodities Futures Trading Commission. These institutions proved consequential in the Biden administration.

O’Brien writes that unlike Roosevelt and Reagan, Obama’s financial reforms “did not alter the central bank’s primary statutory governing objective” (p. 234). But the prior changes in the Fed’s primary statutory governing objectives occurred in the 1946 Employment Act, after Roosevelt’s death, and in the Carter years in 1977 and 1978, not under Reagan. Part of the Reagan era story is the willingness of the Fed to subordinate its statutory employment objective to its inflation objective.

I would have welcomed a final evaluation of the theoretical framework in light of the “part 2” historical analysis. What is “control,” and does control have any reliable link to policy outcomes? What modified research strategies could help us assess the degree of presidential control? Was the period covered too long? Not long enough? Should scholars consider some alternative way of defining a policy domain? Are there some subdomains of public finance in which there was substantial policy consistency across presidencies?

There is no question that this is an interesting and engaging work. O’Brien’s basic puzzle is a good one and important. His framework may be applicable to multiple policy domains. I think scholars would benefit if others would follow him in doing case-study research of presidential leadership in particular policies over a long period. O’Brien’s challenge to policy scholars is quite profound. This is a book worth reading—and debating.

#### **African American Statewide Candidates in the New**

**South.** By Charles S. Bullock III, Susan A. MacManus, Jeremy D. Mayer, and Mark J. Rozell. Oxford: Oxford University Press, 2022. 304p. \$99.00 cloth, \$27.95 paper.

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The changes in Southern Black politics have been rapid, particularly in the last five years. So much so that as *The South and the Transformation of US Politics*—Charles Bullock et al.’s 2019 predecessor to the work reviewed here—was released, a number of important races in Florida, Georgia, and Virginia with competitive Black candidates were heating up. *African American Statewide Candidates in the New South* is the sequel, taking us through the salient statewide races in 2021.

Like its predecessor, the book is a collaborative effort, with each author taking the lead on a different case study. Charles Bullock takes Georgia, examining Stacey Abrams’s narrow loss in 2018, and Raphael Warnock’s win in 2020. Susan MacManus investigates Florida, and Andrew

Gillum's loss in the governor's race. Mark Rozell authors the chapters on Black statewide candidates in Virginia and Jaime Harrison's US Senate bid in South Carolina. And Jeremy Mayer takes the lead on the chapter reviewing African American presidential candidates, focusing on Kamala Harris, Cory Booker, and Deval Patrick.

Before diving into the case studies, the book zooms out, and looks more broadly at the factors influencing the likelihood of success for statewide candidates in the South. Specifically, the authors address two questions: (1) what are the patterns of success for Black candidates in the south, and (2) what is driving wins among Blacks—race or party? They build a dataset of statewide contests for the US Senate and constitutional offices in the South between 1998 and 2018 ( $n = 540$  election contests). The first clear pattern is that Black statewide candidacies have been infrequent—74 contests over the 30-year span, and of these the vast majority (69) ran as Democrats. Second, Black candidates were more successful in the first 15 years of their analysis, although greater numbers of Black candidates ran in the second half.

The authors attribute the rise of Black statewide candidacies to several factors, including increased political activity among racial and ethnic minorities, changes in the share of racial and ethnic minorities in the Democratic Party, and more “qualified” candidates running. Together these factors have led to a weakening in the link between white population size and the failure of Black statewide candidate success.

To answer the question of race versus party, the authors run multivariate analyses, which suggest that it is often party. Indeed, the greatest driver of Democratic victories is Democratic incumbency, and Black Democrats consistently fare worse than their white counterparts. The authors conclude from this that although Black Democrats have won primaries more often, they have been less successful in converting those to general election wins.

The introductory chapter provides a detailed exploration of the statewide races between 1998 and 2018, but it is unclear how this multivariate modeling informs our understanding of the upcoming case studies. Extrapolating to the five case studies presented in the book, the results suggest that the Black statewide candidates examined in the book would be unlikely to win, and thus it is not surprising that only two of the five won their general elections. And the authors concede that additional factors not included in the multivariate models—being a political insider, having united party backing, campaign fundraising, multiracial coalitions—may also be important. So perhaps the cases are outliers, although the authors never concede this point.

Each of the case studies zoom into the elections, and includes all the “nonquantifiable factors” that influenced these elections. And for scholars interested in the deep dive, they will not be disappointed. Information about campaign finances, media and campaign strategy, legal

battles during the campaign, poll results over time, election precinct maps, and historical analysis are provided in each of the case studies. The depth of data provided for each of these elections is impressive, and provides the reader with a play-by-play account. For scholars of state politics looking for detailed information, these chapters are captivating.

For example, the chapter centered on Stacey Abrams's campaign and eventual defeat begins with a deep dive into her political experience prior to running for governor, and her success in rising through the ranks in the Georgia legislature. Bullock further details Abrams's unique strategy—rather than looking to white voters, Abrams focused on increasing minority-voter and low-propensity voter turnout. Indeed, Brian Kemp (Abrams's Republican rival in the general election) was accused of attempting to squash these efforts by removing options for mail-in absentee ballots and reducing the number of precincts in majority-Black wards. And Bullock concludes that although Abrams lost, her strategy was effective—she reduced the gap in votes by 75% compared to the last two Democratic governor candidates.

MacManus builds upon her earlier work on Florida's Black trailblazers to elected office by extending her analysis to Andrew Gillum's failed attempt for the governor's office. She details his primary election, including registration rates by race and age, fundraising, television advertising, and the debates. She attributes his surprising win to many of the same factors Bullock discusses for Abrams in Georgia—particularly mobilization of Black voters who often have low turnout in midterm elections. MacManus then takes us through Gillum's eventual loss to Trump acolyte Ron DeSantis, noting how race and racial cues clouded the debates and campaigning. Indeed, MacManus concludes that the significance of racialized language will influence and mobilize a new generation of Black candidacies in Florida.

The case study of Jaime Harrison's race in South Carolina was equally engrossing. Rozell provides comprehensive details on voter characteristics, polling results, fundraising successes, and key endorsements, often month-by-month for the 12 months running up to the election. And despite expectations of a close race, Harrison lost to incumbent Lindsey Graham by a landslide—54 to 44%. Rozell's analysis of the loss concludes that this outcome was predictable, following other elections with similar characteristics, particularly in the South.

Missing from the individual chapter case studies, however, is a unifying analytic framework. How do these cases collectively speak to larger changes in the South that the authors talk about in the introductory chapter? How do we understand the successes and failures in these case studies, as part of larger mechanisms at play? For scholars who want to examine the details of these elections, there is much to like. However, these chapters read more like an

edited volume, rather than a coherent argument about how and why we see changes in the likelihood of Black statewide candidates.

In chapter 7, Mayer takes up the question of the presidency, and the role of Southern voters in the aspirations of Black presidential candidates. How, Mayer asks, do we understand the paradox of a crucial Black electorate in the South, but no Black candidates from the South? The author walks us through the last 50 years, starting with Shirley Chisholm in 1972; Jesse Jackson in 1984; Barack Obama in 2008; and Cory Booker, Kamala Harris, and Deval Patrick in 2020. Was Obama's win transformative for Southern politics post 2012? How do we understand his win within the context of the other losses? Mayer argues that a crucial factor is Black voter turnout, as well as new voter regulations, and the racialized and polarized environment post-Trump. And he concludes that the barriers to a national Black candidate from the South have weakened over time.

The authors come back to the larger picture in the conclusion, reviewing the key factors that explain the success of Black candidates in the South, including changing demographics, partisanship, and the politics of race and religion. And then they compare the candidates explored along key variables—vote share by race and party and other demographics, and electability. The conclusion: Black victories require a mobilized and unified Black electorate and a substantial coalition of other Democrats, particularly white voters.

The last few paragraphs and postscript of the book remind us that the dynamics at play in these case studies are most likely the beginning of a new story of Black politics in the South, as we continue to watch candidates emerge for many statewide and national offices. *African American Statewide Candidates in the New South* will provide scholars with a foundation to understanding future Black electoral success.

**New Democracy: The Creation of the Modern American State.** By William J. Novak. Cambridge, MA: Harvard University Press, 2022. 384p. \$45.00 cloth.  
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This is one of the most ambitious and interesting books I have read in a long time. William J. Novak, a historian who works at the University of Michigan Law School, is well known for challenging standard accounts of American politics in the nineteenth and early twentieth centuries. This book weaves together many of his prior insights into a coherent whole. Students of American political development are strongly encouraged to read *New Democracy*, which tackles big questions much like classic books by Stephen Skowronek, Theda Skocpol, and Daniel

Carpenter. Students of comparative political economy may be interested in this book as well.

One defining feature of *New Democracy* is its depiction of historical eras. We typically read about Reconstruction, the Gilded Age, and the Progressive Era as distinct periods in US history. Novak argues that the years from 1866 to 1932 should be understood instead as a Second American Revolution with respect to governance. “Nineteenth-century traditions of local self-government and associative citizenship were replaced by a modern approach to positive statecraft, social legislation, economic regulation, and public administration still with us today” (p. 1). The modern American state took shape over decades, the product of multiple forces at the national, state, and local levels. Thus, the Great Depression was not the main reason why government expanded, and the New Deal was not the watershed moment that many believe.

Novak is one of many scholars who have studied changes in governance prior to the New Deal, and he knows it. Rather than focus on bureaucracy, federalism, or liberalism, his book puts democracy at the center of the story. Key actors in the late nineteenth and early twentieth centuries believed that government had to play a larger role in order to ensure that ordinary citizens had a meaningful voice. Otherwise, growing inequalities of income and wealth would seriously damage the polity.

Another defining feature of *New Democracy* is its deliberate flouting of disciplinary boundaries. The argument builds on previous work by historians, legal scholars, social theorists, and political scientists. This combination allows the author to investigate empirical and normative dimensions of governance, which is a plus. It also presents organizational challenges because these disciplines often ask different types of questions and rely on different types of evidence. One common thread is the importance of ideas, and the book can be read as an intellectual history of state building in the United States around the turn of the twentieth century. The leading thinkers include many familiar names (e.g., Henry Adams, Jane Addams, John Dewey, Felix Frankfurter, Roscoe Pound) and others perhaps less so (e.g., Ernst Freund, Walter Weyl).

Each chapter analyzes the development of a core idea—citizenship, police power, public utility, social legislation, antimonopoly, and democratic administration. Entire books can and have been devoted to any one of these ideas, but Novak wants to show their interconnections. Understanding how conceptions of police power expanded (chap. 2), for instance, makes it easier to appreciate how governments justified a variety of social and economic regulations. Each chapter carefully describes the status quo before the Civil War. The developmental paths that emerge in these chapters are not identical. Prevailing ideas regarding citizenship changed quickly and dramatically with passage of the 13th, 14th, and 15th Amendments. In contrast, thinking about police power and