spending in context but are not new to the literature. The chapter also shows that direct lobbying and campaign contributions are more common among large than small corporations, which seems to run against the previous evidence that large companies feel more at risk of boycotts. The likely explanation is simply that large companies have greater resources available. The chapter also includes a qualitative discussion of what drives corporate political activities based on interviews. The anecdotes are intriguing, but the refusal of most companies to participate in interviews limits the analysis to describing the views of lobbyists and journalists.

The book has a running thread on the hypothesis that companies with women as their top executives are more likely to be targeted by activists, and therefore more likely to avoid political activity. It argues that female CEOs—because of their rarity—might attract more attention and more boycotts. Unfortunately, the rarity of female CEOs essentially precludes studying them—all the estimates with female indicators are statistically insignificant—and it is hard to share the book's conclusion that gender is an important factor driving boycotts and corporate political activity.

Overall, the book brings to bear a dazzling variety of research methods—surveys, textual analysis, spending and lobbying data, interviews—and ranges widely over a broad set of issues related to public opinion and corporate political activity. The disparate and somewhat indirect nature of the evidence does not lend itself to producing airtight findings, and I suspect the ultimate value of the book will not be seen as its individual statistical tests but rather in calling attention to the issue and framing the links in the chain that need to be studied. Its central message, not widely appreciated, is that although corporations have by far the deepest pockets to influence politics, they are also the most constrained because of potential backlash from market participants. More remains to be done, but the book provides a road map for future research, especially evidence on three key questions: How do corporate stakeholders (consumers, investors, workers) react to corporate political spending? What effect does this have on corporate value? And the biggest question—how does this affect the political behavior of corporations and democracy overall?

Presidential Control over Administration: A New Historical Analysis of Public Finance Policymaking, 1929–2018. By Patrick R. O'Brien. Lawrence: University Press of Kansas, 2022. 344p. $44.95 cloth.
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This book reflects a hugely ambitious research project. It should be of interest not only to students of the presidency and economic policy but also generally to students of public policy. The project is notable for its extended historical reach.

Patrick O'Brien's central argument is that "presidential control over administration is a foundational component of policymaking and operates as a historical variable" (p. 5). For presidents to bring about large and enduring shifts in policy, they need to control the administrative apparatus. The degree of control varies across time and across policy areas—control is a "historical variable."

In developing this argument, O'Brien basically combines two very different analyses in one book. The first part (chaps. 1–3) ties the work into contemporary presidential scholarship arguing that many scholars assume a degree of executive control of administration that is doubtful, at best. This first part also has a quantitative analysis intended to demonstrate systematically the connection between problem severity and salience and the degree of administrative control. O'Brien hypothesizes sequences of degrees of presidential control beginning with *innovation* and then shifting to *stabilization*, *constraint*, and *collapse*. Following collapse, a new sequence begins. In the period studied, 1929–2018, O'Brien identifies two complete cycles, the first is the "Roosevelt era" and the second is the "Reagan era."

The first part is dense, and the quantitative analysis in chapter 3 may fail to persuade. There are many instances when measurements, definition of categories, and estimations are problematical. A key graph (Figure 3.5, p. 56) suggests that the New Deal control sequence moved from modest to *greater* presidential control, but the Reagan era moved from modest control to *lower* control. In both eras, problem severity and salience both seem to have changed from high to low. At points, O'Brien seems to anticipate objections about variable selection and interpretation—his comments in this respect are useful.

The second part, chapters 4 to 14, is the bulk of the book. This part is much more satisfying because it is a sustained historical narrative that is complex and nuanced. The historical account will be mostly familiar to students of economic policy. O'Brien reviews presidential policy dilemmas (often involving unhappy economic surprises) and frequent tensions with other policy makers. Presidents Kennedy and Johnson are essentially skipped; Nixon and Ford get a light touch.

Some topics not central to the first part are more squarely raised in the second. For example, the role of Congress in public finance policy is more explicit, as is the importance of the president's partisan support in Congress. While the quantitative analysis of the first part coded Federal Reserve (Fed) independence as a constant, in the second part there is considerable attention to shifting presidential relations with the Fed. In the first part "the problem" is restricted to inflation plus unemployment. But in the second part, other economic conditions enter the account.

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