hand, but it is not always conducive to crisp observable implications that might make the theory clearly portable to other places and times.

The book also provokes questions about the extent to which elites in these three countries observed and learned from each other. Britain, France, and the United States were deeply entangled with each other as colonizers and colonized, allies and antagonists, trading partners, and intellectual and political interlocutors. The American and French Revolutions and the waves of democratization that they set off were signal events in transatlantic history, celebrated by some as models to be emulated and reviled by others as horrors to be avoided. What, if anything, did these countries learn from each other as they fumbled their ways toward democracy? If there was transnational learning among these (and other) countries, we might have to rethink theories of democratization that treat individual nations as independent cases and to take questions of timing and sequence more seriously.

These cavils aside, David Bateman has produced an essential study that no student of American political development or comparative democratization—or indeed of American or comparative politics more broadly—can afford to ignore.

The Supreme Court: An Analytic History of Constitutional Decision Making. By Tom S. Clark. New York: Cambridge University Press, 2019. 450p. $99.00 cloth, $29.99 paper.
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Tom Clark's *The Supreme Court: An Analytical History of Constitutional Decision Making* is a tour de force. It is, in many respects, a Court-nerd's dream. The book connects major models of judicial behavior and constitutional development with recent advancements in text and ideal-point estimation to provide a series of nuanced and detailed descriptions of the Court's behavior over the last 130 years. There are seemingly countless analyses that are carefully described and undertaken with a high degree of rigor and precision. In sum, there is a lot to like about this book, and it is a must-read for any scholar who studies the Court.

The book has eight chapters. Chapter 1 introduces the central argument and provides a motivating example. It also introduces the two-dimensional descriptive model that gives way to four intellectual traditions that are reviewed in chapter 2. Chapter 3 details the previously introduced case-space model that generates estimates used throughout the book. Chapter 4 reviews the data, and chapter 5 begins the analysis, estimating the six legal preference dimensions. Chapter 6 analyzes the development of constitutional law from the period of Reconstruction through 1937. Chapter 7 analyzes the Court from the 1930s through 2012. The final chapter offers some lessons learned, limitations, and discussion of some remaining puzzles.

The book's argument consists of several parts. It starts with a basic descriptive process model that argues that social disputes give rise to cases of different types that then determine what types of preference cleavages are created among justices. It next argues that justices' preferences are multidimensional. Crucially, the argument assumes that the median justice determines the disposition and that the justices engage in collegial bargaining over opinion content.

The book takes this process model and argues that the path of constitutional law over time is best described by different approaches that are organized along two dimensions. The first dimension is labeled structural and agency. The structural end of the dimension represents the broad forces that drive behavior into common patterns. These are things like institutions, collegial courts with majority rule, and separation-of-power structures. At the other end is agency. This end of the spectrum emphasizes the role of choice and preferences being exercised by political actors (think judicial preferences or electoral forces). The second dimension is characterized by the locus of attention—whether the focal point is on the internal dynamics of the Court or if it is on things external to the Court.

Clark uses this two-dimensional framework to organize the four main approaches to studying legal decision making and constitutional development: (1) judicial institutions (internal-structural), (2) judicial behavior (internal-agency), (3) social structure (external-structural), and (4) social conditions (external-agency). This organizational framework provides structure for when he interprets various empirical patterns in the rest of the book. For example, he argues that external-structural forces "will be likely to affect how litigants, lawyers, and other branches of government interact with the Court" (p. 9).

This analytical framework is then put into action with the introduction of the case-space model that is used to map judicial preferences onto different legal dimensions. At the risk of oversimplifying it, Clark models the text of Court opinions by applying a latent Dirichlet allocation (LDA) topic model to identify six dimensions of constitutional conflict onto which judicial votes can be mapped: (1) judicial power, (2) economics and business, (3) central authority, (4) balance of power, (5) crime and punishment, and (6) individual and civil rights. Cases are then decided along different dimensions, and the importance of these dimensions changes over time, with different forces and actors playing a role in the dimensional nature of the decision.

The book has many findings, too many to detail here, but it is worthwhile to highlight a few. First, it finds that in the period after the Civil War, the Court largely interpreted legal questions in terms of their views on the dimension of judicial power. Then, toward the turn of the century, the predominant dimension of conflict began to shift to more ideological ones focusing on business–labor conflicts, as well as First Amendment claims of the "Individual and Civil Rights" preference dimension. The book also describes well the Court's transformation of its docket beginning in the 1970s and 1980s by focusing a lot of attention on the "Crime and Punishment" dimension, which correlates with Nixon's law-and-order campaign, the War on Drugs, and mass incarceration. From an analytical standpoint, perhaps the most impressive finding is how Clark shows that cases would have been decided differently had they been decided along a different dimension of conflict. For example, he finds that in "44% of the Criminal Procedure cases decided between 1950 and 1965—54 cases—the case dispositions hinge [on what dimension is activated]" (p. 243).

The book makes many notable contributions. Chief among them are the detailed analyses of the period before the 1940s. As most scholars know, most Supreme Court studies focus on the post-1945 time period, and this book is one of the first to analyze quantitatively the earlier era of the Court. It also offers a number of detailed and nuanced examples of how case conflicts connect to larger political and social dynamics. A clever example examines the correlation between the size of states' Communist Party membership and the number of First Amendment cases.

In addition, the book provides a critical piece of evidence demonstrating the need for scholars to start thinking about judicial preferences in multiple dimensions. Although this is not a novel development, the evidence presented here should push scholars in fruitful directions for many years to come. Finally, the book does an excellent job of attributing change to many factors, rather than trying to claim that a single force dominates the evolution of legal doctrine. This is a unique achievement that might otherwise be undervalued by scholars who wish to argue for a singular or more parsimonious approach.

Although the book is an excellent advancement in many respects, it is not without issue. First, from a theoretical standpoint, the book develops the argument about the importance of framing legal disputes along different dimensions without acknowledging the literature on legal framing. Ignoring the work in this literature is unfortunate, because it raises challenging issues for the book's central argument that would have been useful to confront. For example, although Clark admits that cases can be framed anywhere along each of the six dimensions, what the framing literature has shown is that litigants are already offering competing frames of the same case on the same dimension, not to mention that the Court has to confront other frames (e.g., from the lower courts, amici curiae, and a controlling precedent).

There are three other quibbles worth raising. From a substantive perspective, the book's findings about the shift away from the "Economics and Business" dimension as being important in terms of the quantity of cases it hears seems at odds with colloquial descriptions of the John Roberts Court making seismic decisions in favor of business. Next, although Clark does a good job of being transparent in picking the number of dimensions, and he makes several defensible decisions, narrowing down the number of dimensions is still an atheoretical exercise. It begs the question of why not fewer or more dimensions. More could be done to address this. Third, the assumption that the median justice determines the disposition of policy, although defensible, raises questions about the robustness of the findings if we were to assume that a different actor controlled the disposition (e.g., the majority-opinion author or the median of the majority coalition). This is important because the literature has moved away from a median-only view.

However, these issues should not take away from the major achievements of this book. It will speak to legal scholars of all types and should generate debate for many years to come. We could not ask for more from a book.

Red State Blues: How the Conservative Revolution Stalled in the States. By Matt Grossmann. New York: Cambridge University Press, 2019**.** 200p. $79.99 cloth, $24.99 paper.
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There's an old saying that goes, "If you've seen one state's Medicaid program, you've seen one state's Medicaid program." The same applies to US state politics writ large. However "nationalized" the compound republic becomes, and whatever structural similarities state governments possess, studying subnational politics requires attention to an unwieldy number of variations in the quality of representative government. It also necessitates analytical trade-offs. One approach, best exemplified in Matt Grossmann's *Red State Blues*, widens the analytical lens to focus on macropolitical dynamics in the states. By forsaking some of the analytical depth of, say, single-policy case studies, Grossmann offers a more encompassing set of insights about who governs the 50 states and to what ends.

The empirical setting for *Red State Blues* is a revolution in the control of state governments. Between 1990 and 2017, a combination of cyclical partisan swings and secular changes in the electorate produced a massive series of political gains for the Republican Party. Despite their increasing ideological extremity, however, Republicans have largely avoided electoral backlash in the states. Outside a small number of solidly "blue" states, Democrats