public scandal to the devilish details of implementation by administrative bureaucracies. Despite this attention to complexity, however, the analysis avoids the central lurking question of child welfare politics, and welfare politics in general—the question of the role of race and racism. Initially, the author hypothesizes that welfare politics will not be significantly connected to child welfare politics. In her quantitative analysis chapter (Chapter 3), however, she is surprised to find that "[t]he racial makeup of a state is found to be significantly and negatively related to spending levels" (p. 68). She finds this result counterintuitive, "because political discourse about the two policy areas tends to be different: The need to require adults to support themselves and their families rather than relying on government assistance is contrasted with the need to protect vulnerable children from abuse and help them find loving homes" (p. 68). Gainsborough does engage Dorothy Roberts's book, *Shattered Bonds: The Color of Child Welfare* (2003), about racism endemic in child welfare systems, but does not include any sustained analysis of race politics in any of the case study chapters. This is troubling because, as she herself notes in the conclusion, "in both Florida and New Jersey the children at the heart of the scandals were African American, as were the biological and foster/adoptive parents accused of harming them, and their pictures appeared regularly in newspaper coverage of the scandals" (p. 149).

One incident that gained national attention in Chicago in 1994, described by Gainsborough in the section on federal shifts in child welfare policy (p. 47), exemplifies both the importance of race politics at the heart of child welfare policy and the way it is interconnected with the politics of Aid to Families with Dependent Children/Temporary Assistance for Needy Families at a discursive and concrete level. Lucy A. Williams describes this same scandal in her article on the debate over welfare in the early 1990s ("Race, Rat Bites and Unfit Mothers: How Media Discourse Informs Welfare Legislation Debate," *Fordham Urban Law Journal* 22 [1994]: 1159–96). Unlike Gainsborough, however, Williams notes how this scandal, among others, was central to the national debate over welfare. Furthermore, the assumed class and race of the players in this scandal drove the rhetorical discussion of welfare. The race, gender, and class intersectionality of welfare politics (e.g., see Ange-Marie Hancock, *The Politics of Disgust: The Public Identity of the Welfare Queen,* 2004) and child welfare politics, particularly at the state level, seems hard to ignore. If racism was a central line of inquiry of this book, then the questions asked about the relationship among the media, scandal, and policy response become quite different. Indeed, the question of *why* particular scandals are reported and *how* they are covered becomes of central importance. Although Gainsborough notes that child welfare may become an area for policy solutions in search of problems, as in the case of the privatization of social services in Florida (p. 163), the political nature of the selection of these scandals is hard to ignore in the context of an intersectional politics of race, class, and gender.

Overall, *Scandalous Politics* succeeds in laying the groundwork for further inquiry into this important agenda-setting topic. The fact that the book provokes these questions is a sign that it is an area ripe for further investigation. Scholars of public administration, policy processes, and social welfare policy will find it of substantive interest. It also provides an engaging case study for graduate or advanced undergraduate policy processes courses.

The Nature of Supreme Court Power. By Matthew E. K. Hall. New York: Cambridge University Press, 2010. 262p. $90.00.
doi:10.1017/S1537592711003641

— Gregg Ivers, *American University*

Rare is the scholarly book in political science that continues, after 20 years, to drive a near-continuous debate not only among professional academics working in the field but also among the professional class about whom the book was written. In my professional lifetime, I cannot recall another book in the subfield of law and politics that has generated as much controversy as Gerald Rosenberg's *The Hollow Hope*. Published in 1991, Rosenberg's book has polarized political scientists and lawyers who work at the nexus of law and politics to such an extent that even devotees of the New York Yankees–Boston Red Sox rivalry might shake their heads in admiration, bewilderment, or a combination of both at the fervor with which Rosenberg's supporters and detractors stake their claims. In 2008, Rosenberg published a second edition of his book in which he addressed his critics in a fair and scholarly manner, yet gave no ground in defense of his original thesis—that the Supreme Court is far more constrained, bordering on impotent, to affect social and political change through its rulings. Up until the publication of the first edition of *The Hollow Hope,* the conventional wisdom in the literature on the relationship of the Court, interest groups, and litigation designed to remedy a perceived constitutional violation did not really question the Court's power to, as Rosenberg put it, "prod[uce] significant social reform" (Rosenberg 2008, 422).

Political scientist Matthew E. K. Hall, in *The Nature of Supreme Court Power,* has offered the first book-length argument to address head-on Rosenberg's thesis about the limited nature of Supreme Court power. Concise, systematic, rigorous, and fair, Hall's book stakes out two major goals: 1) to revisit, like many scholars before him, the core arguments of *The Hollow Hope* and 2) to advance, unlike many scholars before him, a more comprehensive, empirically centered argument that offers a more nuanced view of the conditions that favor or limit the power of judicial decisions. Although Hall demonstrates an admirable maturity for such a young scholar in addressing the arguments advanced by and against *The Hollow Hope,* in my view his book is a little too difficult for undergraduate courses—only a student well versed in social science methods and statistical analysis will understand what is under the hood of his study. But graduate students working in the law and politics field will gain a great deal by reading and studying his arguments.

Hall's central argument is this: that "the Supreme Court's ability to alter the behavior of state and private actors is dependent on two factors: the institutional context of the Court's ruling and the popularity of the ruling." Hall states that the "probability of the Court successfully exercising power increases when (1) its ruling can be directly implemented by lower state or federal courts; or (2) its ruling cannot be directly implemented by lower courts, but public opinion is not opposed to the ruling." On the other hand, "the probability of the Court successfully exercising power decreases when: (3) its ruling cannot be directly implemented by lower courts and public opinion is opposed to the ruling" (pp. 4–5). Hall describes the first set of conditions as "vertical," in that the direct line to implementing a Supreme Court decision comes from lower courts that are willing to carry it out, regardless of whether or not the decision is popular with the public. The second condition supporting judicial power occurs when a decision is popular with the public and the courts become part of the enforcement process. The author refers to this second set of conditions as "lateral," since the decision enjoys broad support among the public, from general public opinion to those segments of the population affected by the decision. Only when the Court confronts the third set of conditions is the nature of judicial power limited.

So what are examples that meet the criteria of Hall's three sets of conditions? He offers *New York Times* v. *United States* (1971), *Roe* v. *Wade* (1973), *Texas* v. *Johnson* (1989), and *United States* v. *Lopez* (1995) as cases which, while enjoying various levels of support among the public, were viewed by the lower courts as decisions that merited judicial implementation. Cases in which the Court's power was severely limited included *Brown* v. *Board of Education* (1954), *Lee* v. *Weisman* (1992), and *Printz* v. *United States* (1998). In the end, Hall finds that the Court can promote social change when it acts to relieve "individuals and government actors from legal penalties and spurring popular change against entrenched political interests. The Supreme Court is seriously constrained when it initiates unpopular change in the administration of the state" (p. 165). Readers of this book will have ample opportunity to assess for themselves whether his research design and methodological approaches to testing and defining the nature of judicial power are sufficient or problematic to advance his thesis. And he does not have anything to hide. Indeed, just over 50 of this book's 226 pages consist of appendices and descriptions of various survey instruments used in building his study. Whatever shortcomings scholars may find in the book, transparency and a willingness to let readers know exactly what he is doing and why he chose to do it are not among them.

No good work is without flaws, minor and sometimes more telling. For me, many political scientists and lawyers working in this field are too quick to put complex matters into simple boxes in the service of what they believe to be systematic scholarship. Moreover, there is still the temptation to see too many complex questions involving law, litigation, and the courts as "either/or" questions—that is, the Court is either all-knowing and powerful or it is not. I thought that was part of the problem with Rosenberg's thesis 20 years ago. Too much was ignored or downplayed to advance his argument of the Court as a relatively constrained institution. Nonetheless, Rosenberg deserves all the credit he has received for starting and maintaining an open and honest debate about the power of the Court to affect social change. Had Rosenberg never started this conversation, it is unlikely that Hall would have been inspired to write this important, sophisticated, and first-class study on the nature of judicial power.

Republican Ascendency in Southern U.S. House Elections. By Seth C. McKee. Boulder, CO: Westview, 2009. 272p. $32.00.
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— Richard Johnston, *The University of British Columbia*

This book is remarkably ambitious. It is both a text aimed at the classroom and an original interpretation of the most important recent transformation of the US party system. Unfortunately, it falls between the proverbial stools. There is not enough analysis to help much in explanation, but just enough—or just enough paraphrase of secondary debates—to puzzle, if not confuse, the intended audience.

Most of the book is addressed to changes in congressional elections in the old Confederacy since 1990. Particular emphasis falls on the Republicans' quite sudden ascent between 1990 and 1994, but much is also made of slower, precursor changes before 1990, as well as of the consolidation of the party's advantage after 1994.

The precursor period, the three decades of gradual Republican rise before 1990, is presented as an example of "issue evolution," in the spirit of Edward G. Carmines and James A. Stimson's (1990) *Issue Evolution: Race and the Transformation of American Politics*. On this argument, party repositioning comes first on the race dimension,