

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 Ascendancy 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,

and only later are other issue dimensions, class and religion in particular, reordered. Here the argument is mainly derivative, a review of existing literature with only a modest suite of original analysis and exhibits.

The heart of the book is arguably its account of 1990–94 shifts, with special emphasis on the creation of majority-minority districts after the 1990 census. Although 1994 produced the Republican majority, 1992 showed the way. Notwithstanding Democratic presidential strength in 1992, Republicans made significant southern gains in the House. The gains were concentrated in districts adjacent to newly created majority-minority ones, partly as formerly Democratic white voters now found themselves surrounded by Republicans but also as their own behavior changed in response to the altered context. The critical account focuses on “redrawn voters” in the 1992 vote. As Seth McKee concentrates on incumbents seeking reelection, he probably understates the total redistricting dynamic. The argument is nicely complemented by the obvious counterfactual, redistricting between 2000 and 2002, which did not exhibit the pattern of the earlier event.

The rest of the book is an account of consolidation of the Republican advantage, some thoughts about the setbacks of 2006 and 2008, and speculation on the post-2008 possibilities. One emphasis is on the contributions of the South to overall party polarization; for McKee, the South is practically the whole story. Consistent with this position is evidence that although the South was not immune to the pro-Democratic trends of 2006 and 2008, it resisted them more than other regions did.

As for the future, McKee tries to stay on the fence but seems optimistic about the permanence of the modest Democratic turnaround. Republican domination of the South is predicated on the party’s strength among non-Hispanic whites, still a solid majority but whose preponderance is dwindling.

This is a formidable body of claims, or it would be if the book actually delivered the goods. The most persuasive causal story is the account of Republican gains between 1990 and 1992. The combination of redistricting and the majority-minority mandate supplies the requisite exogenous shock. The momentary mobilization of strong candidates supplies a causal pathway, as does the evidence for the historically specific impact of “redrawn” voters. The difficulty is that the 1990–92 shifts still left the Republicans a weak minority in the region, and not only was the 1992–94 swing massive but it was so in practically every region in the country. To the extent that any account of the 1992–94 change is ventured, the focus is on the changing nature of Republican candidacies. But where 1992 induced entry by high-quality Republicans, 1994 produced the opposite profile. McKee makes much of this but to no clear effect. Here, one suspects, he is anatomizing an epiphenomenon, not supplying a causal mechanism.

The causal story for 1994 lies elsewhere. And it is not peculiar to the South; only the Northeast resisted the Republican tide of that year, and the southern seat gain was not an outlier. The new southern distinctiveness is a product of the years after 1994.

The author is right, then, to spend time on this aftermath. Part of his story is the growing Republican strength at the state level, with all that that implies for redistricting; however the Republicans ascended the southern high ground, their possession of it is vital to their future in the region. But McKee largely glosses over the comparisons with other regions that would seem to be critical to the account. And he presents as his trump card for understanding recent and incipient change a distinction dating back to V. O. Key, between the Deep South and the Peripheral South. Exactly how this distinction underpins a causal analysis is unclear, however, and it seems odd in our time to assign the designation “Peripheral” to states that comprise nearly three-fourths of the region’s population.

Also unsatisfactory is the account of events before 1990. Much of it is stitched together through paraphrase of work by others. For instance, there is an extended account of debates over the party identification as the unmoved mover. McKee seems eager to position himself between the hard line exemplified by Donald Green, Bradley Palmquist, and Eric Schickler’s (2004) *Partisan Hearts and Minds: Political Parties and the Social Identities of Voters* and the advocates of “macropartisanship” (Michael D. Mackuen, Robert S. Erikson, and James A. Stimson, “Macropartisanship,” *American Political Science Review* 83 [December 1989]: 1125–42). But neither side disagrees about the probability of change in the underlying party distribution in the face of policy shifts so momentous and enduring as those in the southern system. There might be a difference in expectation over the relative contribution of conversion and generational replacement, but this hardly seems central to the search for ultimate causes. To that, the timing of distributional shifts might be critical. But for McKee’s story, that timing is awkward: His Figure 2.5 shows that, depending on the exact comparison, one-fourth to one-third of the net 1950–90 shift in the southern distribution of party identification occurred *before* 1964.

In all, the book is a disappointment. As original scholarship, it is most successful for one smallish part of the 1994 realignment. Elsewhere, the analysis is quite aimless, not closely focused on causal mechanisms. I would worry that as an introduction to scholarship by others, the exegesis is too preoccupied with debates implicated only indirectly in the great question of southern transformation and that concern factors appearing late in the explanatory chain. Where he does focus on ultimate causes, his review is very partial, giving short shrift to real divisions of scholarly opinion.