solution for the collective action glitch that threw the 1800 election into the House of Representatives. The congressional debates of 1803 included extensive discussions of the need for the electoral votes of individual states to reflect the will of their majorities. The issue of whether electors should be chosen by districts was also discussed, another reform that continued to be agitated in the 1810s and 1820s (as Alexander Keyssar demonstrates in his new book, *Why Do We Still Have the Electoral College?*). Foley's bold move in elaborating this larger rationale for electoral reform is thus a significant achievement.

But the Twelfth Amendment did not pursue these further measures. Whatever tacit consensus existed in 1803 was not constitutionally entrenched. In practice, most presidential elections have satisfied the Jeffersonian norm because the two-party system is naturally conducive to that result. It is precisely because that outcome occurs so regularly that we have forgotten the historical origins of the Jeffersonian norm that Foley has recovered. Yet absent explicit constitutional language that would implement this norm, state legislatures were left free to manipulate electoral rules as they wished. As Foley concedes, after 1828 "plurality winner-take-all elections became the overwhelmingly dominant method that states used to appoint their presidential electors" (p. 50). This became the true electoral equilibrium that has operated ever since, the nominal Jeffersonian consensus notwithstanding.

Plurality winners will only occur when there are third-party candidates. The existence and potential persistence of these candidates are the true source of Foley's concern. Rather than ask why we should maintain a system in which a victorious candidate can gain three million fewer votes nationally than his rival but win on the basis of narrow victories in three states, Foley prefers using ranked-choice voting, the one mechanism he likes best to avoid the Jacksonian-era resort to winner-take-all plurality victories. Yet it is one thing to argue that ranked-choice voting would work well for, say, reducing a city council pool of 15 candidates to a final cluster of 5 or 7 winners; it is another to ask why it should apply to the sole office in which the entire "executive power" of the United States is vested in a single person. Why should one privilege the second- or third- or nth-choices of voters who consciously want to make symbolic electoral statements in favor of noncompetitive candidates, rather than an effectual choice between the actual contenders? Perhaps Ralph Nader's Florida voters might have preferred Al Gore to George Bush in 2000, or perhaps they would have just abstained from voting. The one thing we know is that Nader-style *nudniks* consciously decided not to vote for the obvious contenders in a battleground state that everyone knew was closely contested. Their initial choice should be respected as the free exercise of the suffrage by informed citizens, but it was also a decision to avoid a vote that would have affected the actual outcome of the election.

Finally, one other silent supposition of Foley's normative scheme deserves consideration: his default commitment to the federal character of presidential elections. That was also part of the putative Jeffersonian consensus, and it remains the default condition that we seemingly cannot escape. Yet why, on principled democratic grounds, should one prefer a state-based presidential election system in which a federal "compound majority-of-majorities" could be gained without a majority of the national popular vote? Why should the modern norm of one person, one vote, not prevail in presidential elections today? A commitment to a federal scheme of presidential voting assumes that each state has some coherent interest that a majority of its voters are qualified to determine. But Americans do not determine their presidential preferences by asking which candidate will best serve the interest of their state. They do so instead on the basis of all those individual preferences that shape their individual political affiliations. The states are only the arbitrary geographic divisions that determine how these individual preferences are distributed across the nation. Optimal electoral reform should make the national expression of those accumulated preferences its proper goal through a popular vote in that single national constituency, the United States of America.

Campus Diversity: The Hidden Consensus. By John M. Carey, Katherine Clayton, and Yusaku Horiuchi. New York City: Cambridge University Press, 2020. 274p. $99.44 cloth, $29.99 paper.
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— Scott E. Page, *University of Michigan*
spage@umich.edu

Scan the administrative ranks of US colleges and universities, and more likely than not you will find a Chief Diversity Officer charged with building a diverse, equitable, and inclusive campus community. University commitments to these initiatives can include scores of administrative positions and upward of ten million dollars in financial support. Many universities (including my own) now place diversity, equity, and inclusion (DEI) on equal footing with knowledge production, education, and service to society as a core mission. In light of this undeniable trend, one might reasonably ask whether these initiatives align with widely held social values or whether they are the result of administrative capture by a liberal elite forcing their ideology on a polarized society.

Enter John M. Carey, Katherine Clayton, and Yusaku Horiuchi. In this exemplary book, they undertake an earnest, innovative approach to add to our understanding of student and faculty attitudes toward campus composition. They ask a relatively straightforward set of questions: Who do students want in their cohorts, who do students want as faculty, and who do faculty want as colleagues? And, is there a consensus for diversity, or do

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campuses resemble our patchwork of polarized red states and blue states?

The book's impressive contribution owes much to the authors' limited scope. They measure preferences, and they measure them well. They leave to others to delve into the psychological, sociological, and historical contributions of those preferences. They touch on the economic, political, and legal implications of the policies that universities have adopted, but recognize that those broader questions already spawn enough PowerPoint decks, STATA analyses, legal briefs, tweets, and takes to fill your Gmail inbox quota thrice over. So, why not get some facts?

Take a moment and think about their central question: *Are preferences for diverse campuses widely held?* This is not so easy to answer; that is, unless you possess the capacious hubris of say, David Brooks, enabling you to grasp the zeitgeist of college campuses through osmosis while popping by on a book tour. The old school approach, a survey, suffers from likely larger-than-normal sample bias and social desirability bias. And there is no natural experiment to be found, a cause for concern or, dare I say, a concern for causality.

The authors, wisely, apply conjoint analysis, a tool more familiar to marketing than political science. Conjoint analysis offers subjects multiple alternatives described as collections of attributes, and subjects then choose their preferred alternative. Here, the alternatives consist of two potential admits to a college, and the attributes include the applicant's grade, SAT score, gender, race, family income, and extracurriculars. In brief, everything an admissions officer might see except the essay and the letters of recommendation.

By varying which levels of attributes people see—for example, top 5% SAT, top 2% SAT—conjoint analysis can infer how much weight people attach to changes in SAT scores. The method produces an *average marginal component effect* (AMCE), which is the average effect of an attribute on the probability of choosing a student. An AMCE of 0.3 for high SAT scores implies that, given two applicants who are identical across all other attributes (averaged across all possible combinations of attributes)—the one with the higher SAT scores will be chosen 30% more often than the baseline level; in this case, an SAT score in the bottom 25%. These inferences are possible, because conjoint analysis presents combinations of alternative values in uncorrelated bundles.

AMCE values are defined relative to the mean of all attribute values (the centroid to be precise). They make sense in a specific case, say for a female applicant from a high-income family, provided there are no interaction effects. Although interaction effects do exist (see intersectionality), their form and magnitude are such that we can restrict attention to marginal effects.

As a methodological exercise, the book surpasses expectation. Methods instructors might well consider including it as an exemplar. The scale and scope of the empirical project merit accolades as well. The experiments sample thousands of students and faculty from a half-dozen schools. Although not a random sample, the schools—which include the University of Nevada–Reno, Dartmouth, and the University of North Carolina—are broadly representative.

The analysis shows that scholarly achievement matters most. Getting an SAT score in the top 2% has an AMCE of 40% relative to being in the bottom 25%. Graduating in the top 1% of one's high school class has an AMCE of 25% relative to being in the bottom 60%. Race matters: African Americans and Latinx get nearly 10% bumps. So, too, does social class: if your parents make a cool half-million, you get an AMCE of minus 10%.

Turning to the point of the study, the data also reveals, as given away by the book's title, a broad consensus in favor of diversity. The authors characterize the degree of consensus by comparing AMCEs (the marginal value of attributes) across subpopulations. *Strong consensus* means that two group's AMCE values have significant signs in same direction and the statistical differences in those values are insignificant. White students give African American students about an 8% bump, and African American students give a 10% bump. Strong consensus.

Consensus can also be w*eak*; that is, the AMCEs can be significant in the same direction but significantly differ in their values. As might be expected, some groups prefer larger benefits for themselves. Finally, the authors define *polarization* as groups having significantly different signs; for example, one group favors legacies, and the other does not.

Although conjoint analysis can construct groups endogenously, the authors fixed their groups ahead of time based on school, cohort (freshman through senior), race, gender, family income, and political ideology. This was an appropriate choice given their objectives. The analyses find nonexistent school and cohort effects. Dartmouth students weight diversity, income, and legacy status pretty much the same as students at Nevada–Reno. Seniors think like freshmen, a finding that undermines claims of liberal indoctrination.

They also find that people of all races, genders, and income level agree on direction, although many of the AMCE values differ significantly (weak consensus). As we might expect, underrepresented minorities give more weight to race than whites, and lower-income students discriminate against wealth more than wealthy students.

Variations in magnitude notwithstanding, the signs almost always align. Even students who oppose affirmative action give a slight nod to minorities and lower-income applicants. The only groups that do not weigh such factors differently for underrepresented minorities or lower-income applicants are Republicans and people with high racial resentment. Both groups show favoritism to legacies

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and first-generation students, while discriminating against people of nonbinary gender. This last finding represents the lone instance of polarization. Democrats give nonbinary gender applicants a leg up.

In sum, there is a good deal of consensus all around. We generally all get along. Well, with two additional caveats. First, the authors assumed their groupings. Within groups of Republicans and Democrats there may exist coherent subgroups who disagree. This could be discovered by allowing for endogenous groups. Second, agreement on directional effects (weak consensus) need not imply agreement on actual admission decisions where applicants have correlated attributes. The analysis suggests that African American students would be far more likely than white students to admit a lower-income, African American applicant than a rich, white applicant with slightly higher SAT scores. Similarly, nonwhite students would advocate for more nonwhite faculty than would white students, female students for more women faculty, and so on, and so on.

Thus, even though the study reveals almost universal consensus, we can still look forward to lively campus debates about admissions criteria, with no shortage of people lining up on opposite sides of admissions and hiring decisions. Even so, how wonderful to know that though we may differ in the strength of our advocacy for diversity and inclusion, we believe in a common direction—forward.

Lighting the Way: Federal Courts, Civil Rights, and Public Policy. By Douglas Rice. Charlottesville: University of Virginia Press, 2020. 176p. $39.50 cloth.
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— Laura P. Moyer, *University of Louisville*
laura.moyer@louisville.edu

Legal scholars and social scientists alike have long debated the question of whether courts can generate social change. This debate has always been intertangled with normative concerns related to the counter-majoritarian difficulty; the academic debate also has important real-world implications for social movement strategy. In *Lighting the Way: Federal Courts, Civil Rights, and Public Policy*, Douglas Rice takes on the narrower, logically prior question of issue attention, which he describes this way: "Where do the fires start? Once started, how and when do they spread?" (p. 35). Do courts hang back and wait for Congress or the president to act, simply acting as implementers of enacted policy? Or, by leading the way, can they put pressure on the coequal branches to address an area of public policy?

In posing these questions, Rice brings together several strands of literature, including work on policy agendas by scholars like Frank Baumgartner (*Agendas and Instability in American Politics*, 1993) and Jack Kingdon (*Agendas, Alternatives, and Public Policies*, 2003) and debates within sociolegal scholarship about the extent to which courts can be a catalyst for policy change (e.g., Gerald Rosenberg, *The Hollow Hope*, 1991). This well-written book provides an excellent synthesis of the competing perspectives on courts' ability to "light the fire."

The central argument of the book is that federal courts can be a leader in influencing issue attention across institutions, but only when two conditions are present. First, the policy must have a viable political constituency that would benefit or be harmed by it. Here, Rice draws heavily on the work of Michael McCann on dispute-centered framing (*Rights at Work*, 1994) and Charles Epp on support structures (*The Rights Revolution*, 1998). Second, courts must have unique power in that policy area: "for courts to systematically lead the attention of other institutions within a particular policy area without in turn being systematically influenced by other institutions, the courts must have constitutionally based policymaking power within that policy area" (p. 3). If a policy area simply has a political constituency, but the Supreme Court lacks constitutional power or typically engages in statutory interpretation, rather than constitutional interpretation, then Rice argues that courts will only be involved in reciprocal issue attention relationships with other branches. They will not, however, be the initiator.

Although the policy agendas typology that Rice adopts here allows for comparability across institutions, it has its limitations when applied to the judicial context, making the analyses less illuminating than a more refined scheme would yield. For example, it would be useful to include more detail about the types of legal claims that fall under each policy area, particularly those with labels as broad as "social welfare."

More justification could also be provided in support of whether each broad policy area is designated as exemplifying the condition of "unique constitutional power." The book does not lay out how frequent statutory decision making versus constitutional decision making is used in each issue area to defend its categorization, and there are reasons to question the characterization of some policy areas. For instance, in the area of civil rights, federal courts routinely engage in statutory interpretation of laws like Title VII, the ADA, and the Equal Pay Act. In addition, for two of the policy areas designated as satisfying the "unique constitutional power" condition—economic activity and civil rights—the Constitution also specifies that Congress has power in each area. Under economic activity, the Commerce Clause grants expansive power to Congress to regulate interstate commerce, and under civil rights, the Enforcement Clauses of the Thirteenth, Fourteenth, and Fifteenth Amendments state that "Congress shall have the power to enforce this article by appropriate legislation." A little more explication about how the policy areas map onto the courts' typical activity might clarify these issues.