

(p. 88), while linking her bipartisan work with former Senator Claire McCaskill on sexual assault to her experiences both in college and in the military. Smith notes that Ernst, though “rated the second most conservative member of the Senate in 2019” (p. 112), proved willing to break with her party on issues of military readiness and well-being, as she did when she came out against the Trump administration’s ban on trans service members.

The biographies of these four Congresswomen are eminently readable and informative, as is an additional chapter on the so-called “badasses” of the 116th Congress: three military veterans, Mikie Sherrill, Elaine Luria, and Chrissy Houlahan, and two former CIA agents, Elissa Slotkin and Abigail Spanberger. Smith evaluates all the women on their own terms—asking how they viewed their service in the military and Congress and how it fit within a larger tradition of “service above self.” The biographies are well-researched and will be of general interest to those interested in veterans, women, or women veterans as candidates and legislators. Smith addresses these women’s childhoods, military service, role models, entrances into politics, and congressional service, including the ways in which their military service impacts their legislative approaches and priorities. McSally’s class action lawsuit against the Pentagon for its policy requiring women serving in Saudi Arabia to wear the abaya is well known. Smith puts this lawsuit into the context of earlier suits filed by military women that expanded women’s career opportunities in the military, and McSally’s own congressional efforts to further gender equality in the U.S. military.

Smith’s handling of the Congresswomen, while humanizing, is perhaps too diplomatic at times. Notably, Smith repeatedly highlights Gabbard’s stated commitment to treat all Americans with respect (p. 150), stresses the “internal consistency in Gabbard’s ideology and principled approach to foreign policy” (p. 155), and emphasizes her commitment to the principle of “service above self”—Gabbard’s campaign motto (p. 151). Yet the evidence Smith presents, including Gabbard’s reference to fellow Representative Adam Schiff as a “domestic terrorist” and her inconsistent takes on foreign policy—if not Smith’s assessment of it—undermine this claim. Regardless, even when Smith pulls her punches in the analysis, she doesn’t shy away from presenting the candidates’ foibles, from Gabbard’s gaffes to McSally’s reversal on Trump to win her Arizona primary.

The thread that binds this book together is the concept of a tradition of women who serve their country through both military or quasi-military service *and* congressional service. Smith presents the veteran Congresswomen of the post-Gulf War era as a continuation of a trend begun much earlier. U.S. women’s military and quasi-military service goes back to the Revolutionary War, in which Margaret Corbin and Mary Hayes took the places of their fallen husbands while many others served in support roles.

Smith traces the roots of the current generation to veterans of the World Wars and subsequent eras, noting that women created the first veterans’ organization for women in 1921—codifying their identity as “servicewomen.” Smith identifies two cohorts of women. Ruth Bryan Owen, Edith Nourse Rogers, and Helen Douglas Mankin served in medical roles in the first World War before playing pivotal roles as Congresswomen in opening official military service to women. Smith’s second cohort consists of Margaret Chase Smith, who, while not serving in any quasi-military capacity, was nevertheless critical in expanding women’s military service while in Congress; and Mary Catherine Small Long, who served in the Women Accepted for Volunteer Military Service (WAVES) in World War II, but did not join Congress until 1985. Smith’s biographies of these earlier service-women in Congress are particularly valuable for the archival research that underpins them.

This book is the first to attempt to understand what motivates the veteran women in Congress and how they got there—though it doesn’t address those whose campaigns fell short. Smith’s exploratory analysis builds on work that separately assesses women in Congress and veterans in Congress, notably Jeremy Teigen’s (2018) *Why Veterans Run: Military Service in American Presidential Elections, 1789–2016*, and Peter Feaver and Richard Kohn’s (2001) edited volume *Soldiers and Citizens: The Civil-Military Gap and American National Security*. Smith is breaking new ground here, and this book has a place on the shelf of anyone studying this new wave of veteran Congresswomen.

Persuading the Supreme Court: The Significance of Briefs in Judicial Decision Making. By Morgan L. W. Hazelton and Rachael K. Hinkle. Lawrence, KS: University of Kansas Press, 2022. 275p. \$32.95 paper.
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Morgan Hazelton and Rachael Hinkle’s new book, *Persuading the Supreme Court: The Significance of Briefs in Judicial Decision Making*, examines one of the remaining parts of the Supreme Court’s judicial process yet to receive a book-length treatment in political science: briefs on the merits. This is a significant gap in the literature given that these briefs are the primary opportunity for attorneys to inform and persuade the justices without interruption. Hazelton and Hinkle’s goal is to provide a thorough understanding of how the written information received by the justices influences their decision making, which the authors successfully do by the end of their book. To accomplish this mammoth feat, Hazelton and Hinkle collected more than 32,000 briefs from litigants (merits briefs) and outside interests (amicus briefs) on all cases

heard between 1970 and 2015. They then use this impressive and novel dataset to understand who produces these briefs and, importantly, what makes them successful both individually and together in influencing case outcomes and opinion content.

Although their data are impressive, the richness of the book comes from the mixed-method approach used by Hazelton and Hinkle: they combine their statistical analyses of the brief data with interviews of Supreme Court attorneys and former Supreme Court law clerks. Direct quotations and important firsthand experience provide descriptive information from the viewpoints of both those writing the briefs and those on the receiving end who help the justices comb through the immense number of pages of information received for each case. These interviews are intricately woven in each chapter through Hazelton and Hinkle's review of existing literature, which helps build their theory and hypotheses. These interviews will surely be a phenomenal source of new research questions, theory building, and unique anecdotes.

The first major question the authors tackle is, What characteristics make a quality brief, and are they the same for all briefs? Here, they break down an important theory of party capability, also known as "the haves and have-nots," to distinguish between filers and attorneys. These terms are often conflated and used interchangeably by scholars in the judicial politics literature because an attorney typically represents a litigant/filer/party. Yet, Hazelton and Hinkle point out that filers and attorneys are not necessarily one and the same. Thus, they examine separately the capability of litigants and their attorneys on the merits and extend this to amicus filers and attorneys hired to write amicus briefs. They use various measures of information, citations, and language to operationalize "quality." Their results support this important distinction. The number of attorneys on a brief and attorneys' experience matter overall for the quality of briefs on the merits, whereas filer factors are not as significant. In contrast, amicus brief quality is influenced by both attorney quantity and experience and filer quantity and experience.

The next question Hazelton and Hinkle attempt to answer is complicated by their copious interview information: Do parties and amici coordinate the information in their briefs, and if so, do they divide and conquer the information they include in their briefs or do they highlight the same important legal arguments? This question is important for political scientists who are interested in how parties and outside interests influence case outcomes and content. It also has significant real-world implications for determining the best way for litigants and outside interests to use their time and resources to achieve their desired outcomes. Although many experts recognize the coordination between parties and amici, responses were mixed on whether having more information or repetitive information was an effective strategy. Using a cosine similarity score, which

estimates the similarity between two documents based on word choice but not order, the authors find that having more experienced attorneys increases the likelihood of repetitive information. But when interest groups are the amicus filers, the likelihood of similarity decreases.

In the end, these important factors—such as brief similarity, the number of attorneys on a brief, and attorney experience—recognized by Hazelton and Hinkle only matter if a party gets their desired outcome in the case. The authors first approach this in the most straightforward way by examining how brief language, quality, and attorney and filer resources influence individual justices' vote choice and case outcomes. Overall, they find that having more words and using a more expansive vocabulary in their brief increase the likelihood of a justice's vote in their favor, whereas having more citations decreases this likelihood.

The authors also use their cosine similarity score to understand how different types of briefs influence the content of opinions. In judicial politics research, scholars often rely on vote choice or case outcome measures (like those previously discussed) because that is all that is available. Case outcomes are important for the litigants directly involved, but the policy created by the Court in the majority opinion affects the rest of the country and outside interests. Using their cosine similarity score, Hazelton and Hinkle examine how merits and amicus briefs influence majority opinions and therefore the policy adopted by the Court. Importantly, the authors find that novelty, unique words, and citations decrease the likelihood of an opinion incorporating information from either parties' or amici's briefs, but shared words and citations among these documents significantly increase the likelihood that the Court's majority opinion reflects both the litigants' and outside interests' briefs. Put differently, litigants and friends of the Court are more likely to influence Supreme Court policy outcomes if they repeat and share information.

The unique insights that Hazelton and Hinkle's book provides to political science, law, and legal practitioners goes well beyond the word limit available to me for this review. This book is a necessary addition to the shelf of anyone who views themselves as a scholar of US judicial politics. It is the first to provide descriptive information on briefs at the US Supreme Court; and the first to examine how these briefs work in combination to influence justices' votes and policy outcomes. It will fundamentally alter the way researchers think about litigants, attorneys, and their distinct roles and resources.

Although this book is the first to examine the influence of briefs on the Supreme Court, it certainly will not be the last. The data collected by Hazelton and Hinkle will be used to inform Supreme Court scholars for years to come. For example, are certain types of interest groups responsible for the finding that interest groups contribute unique information? The cosine similarity score the authors adapted will surely also be useful in answering

the following questions: How similar are amicus briefs across cases? How similar are justices' oral argument questions to the briefs? Is the oral advocate with a greater cosine similarity score to the briefs less likely to win? These questions only scratch the surface of what can be done with the data and measures introduced by Hazelton and Hinkle. Nor does it even begin to account for the

in-depth expert knowledge provided by their interviews. In addition to the book's main text, appendix A is rich with additional data on the different goals of parties, briefs at the agenda stage, and so much more that the authors label as "Avenues for Future Research" (p. 219). Any number of such avenues have been opened by this rich, rewarding, and important book.

COMPARATIVE POLITICS

Would Democratic Socialism Be Better? By Lane Kenworthy. New York: Oxford University Press, 2022. 240p. \$99.00 cloth, \$27.95 paper. doi:10.1017/S1537592723000221

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This is an interesting contribution to a very important debate, though ultimately, I find Lane Kenworthy's thesis somewhat too static and conservative. In a nutshell, the conclusion of *Would Democratic Socialism Be Better?* is that we do not need to spend time thinking about new forms of democratic socialism. All we need to do according to the author is to follow the best-practice institutions of social-democratic capitalism, especially those of Denmark, Norway and Sweden, which have been very successful in the past and should serve as our guide for the future.

Kenworthy defines social democratic capitalism as a regime featuring "a capitalist economy, a democratic political system, good elementary and secondary schooling, a big welfare state, employment-conducive public services (childcare, job training and others) and moderate regulation of product and labor markets" (p. 2). The most interesting part of the book is when the author documents the successes of Europe's social democratic societies in achieving higher welfare, life expectancy, and life satisfaction than all alternative existing systems, and in particular higher incomes for the lowest socio-economic groups than the US. This conclusion also applies to countries like Germany or France, but Kenworthy is particularly interested in the case of Denmark, Norway, and Sweden, where the mobilization of public resources for social transfers and social investment has been particularly strong, especially in health and education, resulting in total tax revenues around 50% of national income, a public employment share around 30%, and substantially higher welfare and incomes for lower socio-economic groups than pretty much everywhere else (see, e.g., the figures on pp.3, 8, 42, etc.). The readers of Kenworthy will recognize the author's characteristic sharp style throughout the exposition, as well as the richness and clarity of the comparative empirical material embodied in the extended set of figures.

In my opinion, the main limitation of Kenworthy's book is that he tends to treat social-democratic societies (which he calls "social democratic capitalism") as a quasi-finished product, almost as a static and frozen system. In my view, this approach is wrong-headed and has contributed to weakening the social-democratic agenda in recent decades. Social democracy is not a finished product—quite the contrary: it is still in the making. In particular, there exists a continuum of potential regimes and future trajectories that are yet to be explored between currently existing social democracy and various forms of democratic socialism. Here we come to the central issue: Kenworthy does not like the idea of a continuum between social democracy and democratic socialism. He is particularly upset at people like Bernie Sanders, who pretend to support democratic socialism and who in fact have in mind something closer to Nordic social democracy (p.14). He should also be upset at the many political movements who use the terms "social democracy" and "democratic socialism" in an interchangeable manner, like the "Parti socialiste Suisse" (the French name of the party), whose name is "Sozialdemokratische Partei der Schweiz" in German, "Partito socialista svizzero" in Italian, and "Partida socialdemocrata da la Svizra" in Romansh: this is however the same party with the same platform.

Kenworthy would like to draw a sharp dividing line between social democracy and democratic socialism, but the problem is that his own definitions do not allow him to do so. He proposes the following definition for socialism: "let's stipulate that socialism refers to an economy in which two-thirds or more of employment and output (GDP) is in firms that are owned by government, citizens or workers" (p.1). The difficulty is that Kenworthy never defines what it means to be owned by "citizens or workers" rather than by "capitalists." Does this mean, for instance, that more than 50% of the company shares are owned by citizens or workers who individually own less than 1%, or does this require that 75% of the shares are owned by citizens or workers with nobody owning more than 10%, or should the definition also take into account the size of the company and the number of workers? Kenworthy never tells us. Depending on the exact thresholds and definitions, one might end up with the conclusion that democratic socialism is already in place in a number of countries.