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Political Ideology and the Law Review Selection Process

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We investigate the role that political ideology plays in the selection process for articles in law reviews. To do so, we match data on the political ideology of student editors from 15 top law reviews from 1990 to 2005 to data on the political ideology of the authors of accepted articles. We find that law reviews with a higher share of conservative editors accept a higher share of articles written by conservative authors. We then investigate potential explanations for this pattern. One possibility is that editors have a preference for publishing articles written by authors that share their ideology. Another possibility is that editors are objectively better at assessing the contribution of articles written by authors that share their ideology. We find evidence that the relationship between editor and author ideology is driven by editors’ greater ability to screen articles written by authors that share their ideology.

*JEL: I23, J15, J70, J71, K0, M51

Keywords: Academia, Publication Process, Political Ideology, Law Professor, Law Review

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1 Introduction

Law reviews are the main outlet for legal scholarship. They are journals run by groups of law students who select which articles to publish. These publication decisions are shrouded in mystery, and little is known about the factors that affect them. In this article, we investigate one potentially important factor: political ideology. To study the role that political ideology plays in the law review selection process, we collect data on articles published at 15 of the top law reviews from 1990 to 2005. Next, we obtain the identities of editors from yearly mastheads of the editorial boards. Finally, we match editors and authors to a measure of their political ideology based on their political donations.

Using this data, we find that the number of accepted articles written by conservative authors is increasing in the conservativeness of a law review’s editorial board. Our estimates suggest that an editorial board with 1 percentage point more conservative editors accept 0.6 percent more articles written by conservative authors. To interpret the magnitude of this estimate, consider the ideological differences between law reviews’ least and most conservative boards. On average across the 15 law reviews in our dataset, a law review’s most conservative board has 72 percentage points more conservatives than their most liberal board. Therefore, our estimates suggest that a law review would accept 43 percent more articles written by conservative authors in a year with their most conservative board compared to a year with their most liberal board.

Next, we investigate two potential explanations for this pattern. One possible explanation is that the relationship stems from editor favoritism toward authors with shared ideology or from a desire to publish articles that promote editors’ preferred political agendas. Another possible explanation is that the relationship stems from editors being better able to screen articles with shared ideology, which can be driven by higher levels of relevant knowledge or expertise. For example, if conservative editors have more expertise in the
legal methodology of originalism, they might have a greater ability than liberal editors to
distinguish articles that will be widely cited that utilize this predominantly conservative
approach. Studies have demonstrated that a similar explanation drives disparities in other
contexts. For example, physicians have a diminished capacity to understand the symptoms
of minority patients (Balsa and McGuire, 2001) and evaluators of grant proposals are better
able to distinguish the quality of grant proposals in their particular field of expertise (Li,
2017).

Although both explanations would predict that editors would accept more articles
written by authors who share their ideology, the explanations generate conflicting predictions
about the future citations of articles written by authors with and without shared ideology.
On the one hand, if editors accept articles on the basis of ideology because of a preference
to promote their ideology, a standard result is that the average article citations would be
lower for articles written by authors with shared ideology and higher for articles written by
authors without shared ideology (Becker, 1957; Knowles et al., 2001). On the other hand, if
editors accept articles on the basis of ideology because they are better at screening articles
written by authors with shared ideology, it is possible that the average citations would be
higher for articles written by authors with shared ideology and lower for articles written
by authors without shared ideology (Aigner and Cain, 1977). We test these predictions
and find that articles written by authors with shared ideology are cited more often than
articles written by authors without shared ideology. These findings are inconsistent with the
preference explanation and consistent with the screening explanation as the mechanism for
editors selecting more articles written by authors with shared ideology.

The results shed light on important debates in the legal academy. First, the
results highlight a potential mechanism influencing the ideological diversity of law professors.
Because the number of articles published by conservative authors depends on both the supply
of articles from conservative authors and the demand for articles by editors, the relationship
between editor and author ideology implies that changes in the ideology of student editors can change the career opportunities available to conservative law professors. However, the direction of the effects for academics during our time period is ambiguous because student editors at elite law schools tend to be liberal but are nonetheless relatively more conservative than law professors. Second, the results are relevant to the “common criticism . . . that law students lack the experience or training to effectively evaluate legal scholarship” (Posner, 1995). Although the results do not allow us to directly comment on the debate over whether selecting articles through a peer-review process would be superior to a student-run process (see, e.g., Friedman, 2018), they suggest that students may be relatively better at selecting articles over which they have some prior knowledge or expertise.

The results also have potentially important welfare implications. If articles published in higher-ranked law reviews are more likely to influence judges or policy-makers, the role of ideology in the article selection process could skew judicial and policy outcomes. In addition, the fact that author ideology matters in the article selection process can influence hiring decisions on the entry-level and lateral legal academic markets. And given the conventional wisdom among law professors that past article placements influence student editors’ publication decisions, any disparities created by shared ideologies between editors and authors could compound over time.

This article proceeds as follows. Section 2 discusses the institutional setting. Section 3 describes the data and reports descriptive statistics. Sections 4 presents the identification strategy and report the primary results. Section 5 explores two potential explanations for the pattern. Section 6 explores whether limitations of our ideology data could influence the results. Section 7 concludes.
2 Institutional Setting

A large social science literature finds that political ideology plays an important role in decision-making (e.g., Martin et al., 2004). Although there has been some research documenting the political diversity of the academic profession (e.g., Gross, 2013; Bonica et al., 2018) and the role of ideology in the production of academic research (e.g., Jelveh and Kogut, 2014; Chilton and Posner, 2015; Jelveh et al., 2017), there is no research investigating whether political ideology influences the article selection process.

However, there is a literature on the influence of other factors on the article selection process across a wide range of fields, including biology (Borsuk et al., 2009), computer science (Tomkins et al., 2017), ecology (Budden et al., 2008), economics (Blank, 1991; Smart and Waldfogel, 1996), medicine (Gilbert et al., 1994), and psychology (Lloyd, 1990). Most of this literature studies whether referees give more favorable recommendations to some authors than others, but some addresses whether journal editors favor some authors (e.g., Laband and Piette, 1994). These studies usually find no evidence of referee favoritism, but there are exceptions. Notably, Hengel (2016) finds evidence that women authors are held to higher writing standards than men, and Tomkins et al. (2017) finds evidence that articles written by famous authors are more likely to be published in a single-blind review than in a double-blind review.

Most of this research assesses differences in outcomes by the characteristics of an identifiable group of authors without regard to the characteristics of the editors or referees (e.g., Ayres and Vars, 2000). However, three articles investigate whether publication outcomes vary by shared attributes of authors and either editors or referees. Abrevaya and Hamermesh (2012) find no evidence that referee’s recommendations differ if the referee and author share a gender. Colussi (2018) finds evidence that an author’s social connections to the editor improves publication outcomes. Yoon (2013) finds evidence that law professors
are more likely to publish in the law review of their home law school and that those in-home articles are cited less frequently than publications by outside faculty in the same law review.

This paper investigates the influence of political ideology in the selection process of law review articles. Student editors review submitted manuscripts and make acceptance decisions, generally without seeking expert review.\(^1\) Student editors have long been criticized for a lack of expertise (see, e.g., Friedman, 2018). Scholars have suggested that student editors rely on a set of proxies when choosing which articles to publish. Quantitative and qualitative evidence suggests that factors unrelated to article quality influence student’s acceptance decisions: the author’s credentials and reputation, the author’s previous publication record, the author’s connections to the publishing institution, the subject matter (with a preference for hot topics), and the author’s race and gender.\(^2\) Although there have been some suggestions of an effect of ideology in law review selection decisions (including an offhanded comment in Posner, 1995),\(^3\) the question has not been studied empirically.

There are six features of the law review publication process that make the institutional setting well-suited for studying the relationship between ideology and article selection. First, unlike editors of peer-reviewed journals, law review boards turn over each year. This means there is year-to-year variation in the individuals involved in the article selection process. Second, we are able to compare the outcomes of multiple articles for the same editors.

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\(^1\)A few law reviews (e.g., Harvard Law Review, Yale Law Journal) have recently begun to send some articles out to expert faculty referees. During our sample period (1990 to 2005), expert reviews in law review article selection were very uncommon. If we exclude the few journals that might have sent articles out for review during our sample period, the results are largely unchanged in both an economic and statistical sense.

\(^2\)See, e.g., Ellman (1983); Leibman and White (1989); Merritt (1998); Christensen and Oseid (2007); Nance and Steinberg (2009); Higdon (2016). Recent lawsuits have been filed challenging the use of race and gender in law review article selection decisions (Van Voris, 2018).

\(^3\)See Posner (1995) at 1133 (“The change in the character of legal scholarship has been accompanied by a collapse of political consensus among legal scholars and by a vast expansion in constitutional law, which is the most political field of law as a consequence of the nature of the issues it addresses, the remotesness of the governing text, and the field’s domination by a court (the Supreme Court) from which there is no possibility of appeal to a still higher court to keep the judges in line. Legal scholarship became more political at the same time that it was becoming more centrifugal. These developments beached not only a number of doctrinal scholars but also most student editors. They were now dealing with a scholarly enterprise vast reaches of which they could barely comprehend, and they were being tempted by the increasing politicization of the enterprise to employ political criteria in their editorial decisions.”).
In the peer review process, this may not be possible because rarely will the same editor and referee be observed.

Third, there are not multiple sources of influence in the law review selection process. One identification problem when studying influences in peer review journals is that there could be both editorial and referee influences acting simultaneously, making it difficult to disentangle their effects. For example, there could be three-way interactions between authors, editors, and referees. With law reviews, that is generally not the case.

Fourth, the same pool of articles are considered by each law review board. Most academic journals outside of law reviews restrict authors from submitting to other journals simultaneously, which might lead authors to self-select into submitting to different journals. The result is that different journals may have dramatically different pools of articles to select from. This is not true of law reviews. Twice per year (February and August), authors who submit to one top law review almost exhaustively submit to all top law reviews. This simultaneous submission setting overcomes concerns of selection into journals on the basis of ideology.

Fifth, student editors have very few social ties with law professors, particularly law professors from other schools. In many peer-reviewed journals, the editors and potential authors can share professional connections. The result is that conservative (liberal) editors may be more likely to accept articles from conservative (liberal) authors simply because they have more professional and personal ties (Colussi, 2018).

Finally, the law review setting allows us to match student editors to a measure of their ideology. Linking individuals to common measures of political ideology based on political donations requires enough information on the individual to distinguish between individuals with the same name in the United States. However, data used in most studies of the peer review process only contains the first name of the reviewer (e.g., Abrevaya and Hamermesh, 2012). Unlike the single or double blind process in peer review journals, the
first and last names of student editors are available on mastheads for each volume.4

3 Data and Descriptive Statistics


Voting Members on Law Review. Each law review allows different board positions to vote on which articles to accept, but the board positions with voting rights differ between law reviews and over time. To determine the positions with voting rights, we surveyed the editor-in-chief of every volume in our sample. Appendix A provides details about the survey.

4It is worth emphasizing that because law students make publication decisions, the article selection process for law reviews is probably unlike other article selection processes. Any findings of a relationship between political ideology and article selection for laws reviews thus might not be generalizable to the article selection process in other disciplines, and any findings of a relationship between political ideology and article selection in other disciplines might not be generalizable to the article selection process in law reviews.

5We do not include the Harvard Law Review because the masthead does not distinguish between editorial positions and it allows the full body of editors to vote on the selection of articles. As a result, we are unable to identify the people who participated in the selection of any given article and link them to political donations. Moreover, even if we were able to link their editors to their donations, the large number of editors would ensure that there would be almost no year-to-year variation in the ideology of the board. We do not include New York University Law Review because there is not a one-to-one correspondence between boards and volumes. As a result, we cannot identify precisely which boards chose which articles. Nonetheless, we reran the analysis with New York University in the sample while coding students as accepting articles with a publication date in the year they have a board position on the masthead, and the results are consistent.

6An earlier version of this article used more years of data. At the advice of a referee, we use 2005 as the end year because the percent of editors making political donations noticeably decreased in the later years of the sample.
Although technically only the voting members of a law review ultimately vote on whether to accept an article, it is likely that high-ranking members without voting rights also influence which articles are accepted. This may be through control of the agenda: most articles are rejected before the board holds a vote, and the editor-in-chief and other high-ranking members of a law review may have the ability to influence which articles are put up for a vote. We therefore include high ranking members even if they do not ultimately vote, but the results are consistent if we define the voting board to include only voting members.

Mapping Editorial Boards to Selected Articles. The editorial board at the time an article is published may not have initially selected the article. For example, an article published in Volume 100 of a law review may have been selected for publication by the articles committee of Volume 99. We thus needed to link articles to the board that actually selected them. To do so, the survey asked editor-in-chiefs what volumes published the articles they accepted. We then used this information, and additional information from follow-up correspondence, to map published articles to the board members that selected them.

Student Editor Identities from Masthead. Each volume of a law review contains a masthead page which lists each editor and their position. For example, Figure 1 shows the masthead of the University of Pennsylvania Law Review for Volume 139. We obtained the mastheads for each volume in our sample. From the mastheads, we hand-coded the name of each voting member. In total, we coded the identities of 1,988 editors. The mean and median number of relevant members per journal in our sample is 8.©

©Mastheads are usually structured with the highest ranking position on the top (editor-in-chief) and positions listed in decreasing rank down the page of the masthead. We hand-coded all positions that are at or near the same level as the lowest ranking title that has voting rights. We exclude editors in charge of comments and book reviews as non-voting even if they are at or above the lowest voting member on the masthead.

©Appendix B reports the distribution of the overall number of members per journal-year and the number of donating members per journal-year. Of the members who have made donations, the mean and median number per journal-year in our sample is 3, with 75 percent having at least 2 members.
Estimating Gender and Race. We use editors’ names to recover an estimate for their gender and race. To estimate gender, we use the website “Genderize.”\textsuperscript{9} To estimate race, we use the python package “ethnicolr.”\textsuperscript{10} Ethnicolr “exploit[s] the US census data, the Florida voting registration data, and the Wikipedia data collected by Skiena and colleagues, to predict race and ethnicity based on first and last name or just the last name.” We code someone as non-white if they are predicted to be non-white in any of the datasets.

Published Articles and Their Citations. Heinonline is a website that contains information about law review publications. We gathered information from Heinonline about each publication between 1990 and 2005 in the 15 law reviews in our sample. This includes the title, identity of each author, volume, issue, and number of citations of that article. We make three sample restrictions. First, each volume of a law review usually contains multiple pieces not selected through the same mechanism as typical articles, including comments, notes, and book reviews. We exclude these non-article publications. Second, articles that are published as part of a symposium are typically solicited and do not go through the same selection process. We exclude symposium articles, but the results are similar if we include them. Third, there is evidence that student editors do not evaluate all authors in the same way, with law reviews publishing more articles written by faculty at their own institution (Yoon, 2013). We exclude home school authors because one might expect ideology to play a different role during article selection for them,\textsuperscript{11} but the results are consistent if we include them.\textsuperscript{12} After these restrictions, the final sample is 1,573 articles.

\textsuperscript{9}https://genderize.io/
\textsuperscript{10}https://github.com/appeler/ethnicolr
\textsuperscript{11}An earlier version of this article included home school authors in the main sample. We excluded home school authors from the main sample at the advice of a referee.
\textsuperscript{12}Nance and Steinberg (2008) find that editors make decisions in part because of the prestige of the author or their institution. As a robustness check, we estimated the relationship between editor on author ideology after excluding authors from the top 15 law schools, and the results are consistent.
**Measure of Political Ideology.** Our measure of political ideology is based on the political donations of the student editors and authors. The specific measure we use is called the Campaign Finance score (“CFscore”) and is drawn from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2014). A growing literature uses and validates CFscores (Bonica, 2014; Bonica and Sen, 2017; Thomsen, 2014; Chilton and Posner, 2015; Bonica and Rosenthal, 2016; Wood and Spencer, 2016; Bonica et al., 2019). DIME is a database of campaign contributions made from 1979 to 2016. This comprises more than 250 million donations made by more than 20 million unique donors. Appendix C describes the process of matching our sample to DIME.

To calculate the CFscores from raw donations data, DIME first assigns political candidates unidimensional ideological scores based on their common donors. The scale is normalized to the population of U.S. donors such that the mean is zero and the standard deviation is one. For instance, Bernie Sanders has a CFscore of -1.89, Barak Obama has a CFscore of -1.16, Mitt Romney has a CFscore of 0.90, and Donald Trump has a CFscore of 1.29. Individual donors are then assigned ideological scores on the same unidimensional scale based upon a weighted average of the amount of donations they have made and the CFscore of the candidate to whom they gave. For instance, if an individual’s only political contribution was to Mitt Romney, her CFscore would match Romney’s CFscore (0.90). If an individual donated a total of $1,000 to Barack Obama and $2,000 to Bernie Sanders, her CFscore would be the sum of 1/3 of Obama’s score and 2/3 of Sanders’ score (-1.65).

A number of concerns arise with the use of the CFscore for measuring editor and author ideology. For instance, CFscores are based on lifetime donations, and not all people donate to campaigns and thus do not have a CFscore. In Section 6, we explore the extent to which the drawbacks of the CFscore are likely to affect the results.

13For example, Bonica (2019) validates CFscores against a battery of policy items and finds that they are “powerful predictors of policy preferences for a wide range of issues and successfully discriminate between donors from the same party.”
Descriptive Statistics. Table 1 provides descriptive statistics. Panel A describes the full sample of editors and Panel B describes the full sample of authors. For editors, our measure of board ideology is at the journal-year level. For authors, we define an article as conservative if at least one author is conservative. This approach is motivated by Colussi (2018), who defines an article authored by more than one author as having a social connection with the editor if at least one author is has a social connection with an editor. However, the results are consistent when using alternative ways of classifying co-authored articles.14

Figure 2 plots the percent of editors and authors with political donations over time.15 Over the sample as a whole, we matched 34 percent of editors and 58 percent of authors to donations. We classify individual donors as “conservative” if their CFscore places them at or above the average American donor (CFscore ≥ 0) and “liberal” if their CFscore places them below the average American donor (CFscore < 0). Using this definition, 21 percent of editors and 15 percent of authors are conservative. Figure 3 provides the distributions of the percent of conservative editors and authors per volume.16 The ideology of editors and authors is consistent with previous research (see, e.g., Bonica et al., 2016, 2018).

Panel C provides descriptive statistics of the final sample used in the empirical analysis. The main analysis is at the journal-year level. The average number of articles per journal-year in the final sample is 7, with 10 percent having more than 10 articles.

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14The results are consistent under four alternative ways of handling co-authored articles: (1) making the unit of observation the donating author rather than the article; (2) only defining an article as conservative only if there is at least one conservative author and no liberal authors; (3) using the most liberal or conservative author’s CFscore rather than the mean of the author’s CFscores; and (4) dropping all coauthored articles.

15Figure 2 reports the mean donation rates across articles rather than at the individual level. The fact that the same author publishes multiple articles explains why the percent of authors with donations averaged over articles is roughly the same as the percent of authors who have had a donations in the matched sample.

16Table D1 in the Appendix reports the mean and standard deviation of the percent conservative editors by journal, and shows that there is considerable variation of editor ideology within a journal over time. Across the journals, the average and standard deviation percent conservative editors is of 20 and 24, respectively.
4 Relationship Between Editor and Author Ideology

4.1 Research Design

Suppose article \( i \) has a set of attributes including quality \( q_i \), political ideology as proxied by the author’s ideology \( a_i \), author gender \( g_i \), and author race \( r_i \). Suppose the editors of law review \( j \) in year \( t \) have a set of attributes including political ideology \( e_{jt} \), gender \( g_{jt} \), and race \( r_{jt} \). Further suppose the editors have three preferences over articles. First, editors prefer articles with higher quality \( q_i \). Second, editors prefer to select articles from authors with shared gender and race denoted \( G_{ijt} \) and \( R_{ijt} \). Third, editors prefer to select articles with an ideology close to their own ideology. In particular, let \( P_{ijt} \) be the ideological distance between editors \( j \) and article \( i \).

Let \( y_{ijt} \) be an indicator variable for whether editors from law review \( j \) accept article \( i \) in year \( t \). To estimate the relationship between editor and article ideology, we would ideally estimate:

\[
y_{ijt} = \hat{\alpha} + \hat{\gamma}q_{ijt} + \hat{\beta}P_{ijt} + \hat{\sigma}G_{ijt} + \hat{\theta}R_{ijt} + \varepsilon_{it}
\]

where \( \hat{\beta} \) estimates the relationship between editor and article ideology. The problem is that estimating the above equation requires the full set of articles considered by the editors—that is, both the articles they published and the articles they did not publish. We do not observe the full set of articles submitted to editors. Rather, we only observe the set of accepted articles. Given the set of accepted articles, one potential option is simply to regress author ideology on editor ideology. The problem with such an approach is that, because gender and race are correlated with ideology, any preferences for shared gender or race would create omitted variables bias.\(^{18}\) To account for the possibility of gender and racial preferences, we

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\(^{17}\)Chilton and Posner (2015) find that the substance of articles partly reflects the ideology of their authors.\(^{18}\)For discussions of race and omitted variable bias, see Clarke and Rothenberg (2018); Liscow and Woolston (2018); Miller (2019).
control for editor gender and race. We estimate Equation 1 at the journal-year level.

\[ a_{jt} = \alpha + \beta e_{jt} + \gamma g_{jt} + \theta r_{jt} + \phi_t + \eta_j + \epsilon_{jt} \] (1)

The primary dependent variable \( a_{jt} \) is the percent of articles with at least one conservative author. One advantage of this measure is that it lends itself to easy substantive interpretation. It is also possible that the students selecting articles are aware of whether an author is conservative or liberal but unaware of the degree to which an author is conservative or liberal. One disadvantage of this measure is that it discards information that is contained within the continuous measure of ideology. Therefore, we also use the mean CFscore of the authors as a second dependent variable.

The independent variable of interest \( e_{jt} \) is either the percent of donating editors who are conservative or the mean CFscore of the donating editors. In the preferred specification, we include year fixed effects \( \phi_t \), journal fixed effects \( \eta_j \), and controls for editor gender and race \( (g_{jt} \text{ and } r_{jt}) \). For the gender and race controls, we use the percent of female editors and the percent of non-white editors. The coefficient of interest is \( \beta \). It asks, for a given mix of editors in terms of gender and race, does within-journal variation in the ideology of the editors associate with a different likelihood of publishing articles written by conservative authors?

The articles selected by one law review in a given year are not independent of the articles selected at other journals in the year, which can create mutual dependence in the error terms within a year. In the main results, we cluster standard errors at the year level. Below, we find that the size of the standard errors are similar using different levels of clustering.
4.2 Results

Panel A of Table 2 reports the results of regressing the percent of conservative authors on the percent of conservative editors. Column 1 includes year fixed effects, Column 2 adds law review fixed effects, Column 3 adds controls for gender and race, and Column 4 adds journal time trends. Column 3 reports the results from estimating Equation 1 and is our preferred specification. The estimate indicates that a board with 1 percentage point more conservative editors accept 0.6 percent more articles written by conservative authors (0.0093 percentage points from a baseline of 0.148).\(^{19}\) To interpret the magnitude of the effect, first consider that the within-journal standard deviation of editor ideology is 24 percent of conservative editors on average across the journals. Therefore, the estimate suggests that a 1 standard deviation conservative shift in a board’s ideology is associated with a 14 percent increase in the number of articles published by conservative authors. As another way to interpret the magnitude of the effect, consider that across the law reviews, on average a law review’s least conservative board has 72 percent fewer conservatives than the law reviews most conservative board. Therefore, the estimate suggests that a law review would accept 43 percent more articles with conservative authors in a year with their most conservative editors compared to a year with their most liberal editors.\(^{20}\)

Panel B reports the results of regressing the mean CFscore of authors on the mean CFscore of editors. Although the results are not statistically significant at conventional levels in all specifications, they are consistent with the estimates in Panel A. The results in

\(^{19}\)In Panel A of Table 2, the dependent variable takes the value of 0 if there are no conservative authors and the value of 1 if 100 percent of the authors are conservative, and the independent variable takes the value of 0 if there are no conservative editors and the value of 1 if 100 percent of the editors are conservative. As such, the point estimate of 0.093 indicates that moving from a board with 0 percent conservatives to 100 percent conservatives increases the probability of selecting a conservative author by 9.3 percentage points. To interpret the coefficient as a 1 percentage point increase in the percent conservative editors, the point estimate is divided by 100.

\(^{20}\)Imagine if student editors matched the ideology of the public. In our sample, 21 percent of editors are conservative, but 50 percent of the donating public are conservative. If student editors had matched the ideology of the public, the estimate suggests that the 15 law reviews would have accepted 17 percent more conservative articles.
Column 3 indicate that a 1 CFscore point more conservative board accepts articles written by authors that are 0.083 CFscore points more conservative.

Figure 4 assesses the sensitivity of the results to alternative modeling choices while using the same specifications as in Table 2. First, we assess the sensitivity of the results to different levels of analysis. The main specification was at the journal-year level, but we also estimate these same specifications at the article level, as indicated on the left hand side of the figure. This means that the ideology of authors vary within a journal-year but the ideology of editors is constant within the journal-year. The dependent variable is either an indicator variable for whether there is at least one conservative author on the article or the author CFscore (if there are multiple authors, the mean CFscore). Second, we assess the possibility that the specification did not properly account for trends by estimating a first-difference specification, as indicated on the left hand side of the figure. The dependent variable is the change in the percent conservative authors (mean author CFscore) from one year to the next, and the independent variable is the change in the percent conservative editors (mean editor CFscore) from one year to the next. Finally, we assess the sensitivity of the results to different levels of clustering. For each combination of controls and level of analysis, we estimate the specification without a clustering adjustment (as indicated by “No”), clustering by journal (as indicated by “Journal”), and clustering by year (as indicated by “Year”).

Figure 4 reports the point estimates and 90 percent confidence intervals for these regressions. These results show that the size of the point estimates and the standard errors are not highly sensitive to the level of analysis, the way trends are accounted for, and the level of clustering. Overall, although the estimates in some specifications do not quite reach statistical significance, the results are consistent across the specifications.
5 Mechanisms

There are two main potential explanations for the positive relationship between editor and author political ideology. First, editors could have a preference for accepting or rejecting articles on the basis of ideology (Becker, 1957). Second, editors could attempt to choose the articles that will be most cited but observe noisy signals of quality and be better at screening articles written by authors with shared ideology (Aigner and Cain, 1977; Arrow, 1973; Phelps, 1972). Although both of these explanations predict that editors will accept more articles written by authors with shared ideology (which we refer to as “likeminded articles”), the explanations have conflicting predictions about the citations of likeminded articles and non-likeminded articles. If likeminded articles are selected more often because of a preference, the likeminded articles will have fewer citations. This is a standard prediction from Becker (1957). Therefore, if likeminded articles have more citations, it cannot be the case that a taste for selecting likeminded articles is driving the relationship between editor and author political ideology.

To test between these explanations, we generate a dataset at the journal-year-author ideology level. That is, for each journal-year, there are two observations: the average number of citations for liberal authors and the average number of citations for conservative authors. Let $c_{jt}$ be an indicator variable for conservative author. We estimate Equation 2.

$$\ln(\text{citations}_{jtc}) = \alpha + \beta p_{jt} \times c_{jt} + \sigma c_{jt} + \phi_{jt} + \epsilon_{jt}$$  \hspace{1cm} (2)

where $\ln(\text{citations}_{jtc})$ is the natural log of the average article citations published in journal $j$.

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21 See Mungan (2018) for a review of this literature.
22 Screening could lead to more likeminded articles being accepted, and the additional articles could be less likely to be cited on the whole. As a result, it is not necessarily the case that differential screening ability would lead to higher citations for the average likeminded article. Nonetheless, if we observe that likeminded articles are more highly cited than we would expect, this would suggest that the increase in citations of likeminded articles resulting from greater screening ability first order dominates any decrease in citations from the additional likeminded articles being published.
in year $t$ of author of ideology $c_{jt}$, $p_{jt}$ is the percent of donating editors that are conservative, and $\phi_{jt}$ are journal-year fixed effects. The coefficient $\beta$ on the interaction term captures the differential effect of the conservativeness of the editorial board on citations of articles written by conservative authors.

Equation 2 produces an unbiased estimate for $\beta$ under two assumptions. The first assumption is that student editor feedback in the editing process changes citations to a likeminded article the same as a non-likeminded article. Although we have no support for the assumption, we are skeptical that differential effort that students put into editing articles because of ideology can be material in changing citations to the article. The second assumption is that the additional likeminded articles that are accepted are not more likely to be drawn from highly cited fields. This assumption would be violated if, for example, a board decides to publish one more likeminded article from a high citation field (e.g., constitutional law) instead of a non-likeminded article from a low citation field (e.g., tax law).

In Table 3, Column 1 includes the main effect on percent conservative editors, the main effect on conservative author, year fixed effects, and journal fixed effects. Column 2 adds the interaction term, Column 3 adds the controls for gender and race, and Column 4 adds journal time trends. Column 5 estimates Equation 2 by replacing the journal time trends with journal-year fixed effects, which allows for a comparison of the citations of articles written by liberal and conservative authors within a journal-year. Because there is no variation in editor ideology within a journal-year, the main effect on percent conservative editors and the controls for race and gender drop out of the regression with journal-year fixed effects.

---

23With journal-year fixed effects, the regression is estimated using journal-years in which at least one published article was written by a conservative author and at least one published article was written by a liberal author. For the specifications without journal-year fixed effects, we restrict the sample to the same journal-years in which at least one published article was written by a conservative author and at least one published article was written by a liberal author in order to assess differences in impact between author ideology within an editorial board. This means that the sample size is not double the sample size in Table 2.

24We have been unable to obtain data on the subject matter of articles. If we had data on the subject matter of articles, we would include subject matter fixed effects.
In Columns 2 to 4, the main effect on percent conservative editors is negative, suggesting that more conservative editors accept liberal articles that are cited less. The estimate on the interaction term is positive, statistically significant, and stable across the specifications. The point estimates for Columns 2 to 5 indicate that moving from 0 percent conservative editors to 100 percent conservative editors is associated with a 55 percent increase in citations for articles written by conservative authors. To interpret the estimate, note again that the within-journal standard deviation of editor ideology is 24 percent of conservative editors on average across the journals. This means that a 1 standard deviation shift in a board’s ideology is associated with a 13 percent change in citations for articles written by conservative authors. Table 3 thus provides no evidence that the relationship between editor and author ideology is driven by a preference for selecting articles on the basis of ideology.

To further investigate the relationship, Figure 5 reports a binned scatterplot and a line of best fit of the relationship of interest. The y-axis is the difference between the natural log of the mean citations of articles written by conservative authors in a journal-year and the natural log of the mean citations of articles written by liberal authors in the same journal-year. A positive number indicates that the articles written by conservative authors were cited more on average than the articles written by liberal authors. The x-axis is the percent of donating editors that are conservative. The positive and statistically significant relationship indicates that more conservative boards accept articles written by conservative authors that are cited more than the articles written by liberal authors that they accept.

6 Using Political Donations to Measure Ideology

Measures of ideology based on political donations are increasingly used in social science research (e.g., Bonica et al., 2017a), but there are potentially important limi-
tions with the approach.\textsuperscript{25} We assess whether two limitations of using CFscores to measure ideology—that they are based on lifetime donations and that they are not available for all authors and editors—are likely to bias the results.

\subsection{Lifetime Measure of Ideology}

One concern with using CFscores as a measure of ideology is that they are based on lifetime donations. There are three reasons to believe that this approach does not raise identification concerns for the analysis. First, ideology of adults has been shown to be stable (e.g., Bonica, 2014), and research suggests that typically only major life changes are likely to alter ideology (Green et al., 2004). To put the stability of ideology in context, some evidence suggests that Americans are more likely to change their religion than their political party.\textsuperscript{26} Although many of the editors may have gone through these kind of major life changes, we think it is unlikely that this would occur in a systematic way that would drive correlations between average board ideologies and the articles that are accepted. Second, if editors’ ideology changes after law school and before subsequent political donations are made, this approach would likely introduce measurement error rather than create any bias in the estimates. Finally, the main concern for identification would be if the process of reviewing articles caused editors’ ideologies to change (i.e., reverse causation). For the results to be driven by reverse causation, editing one and a half more conservative articles would have to lead to a one standard deviation change in ideology of the entire editorial board. That is, for the combined group of editors, editing the article would need to have an effect on each editor’s ideology dramatically bigger than the demonstrated effect from profound life events, such as large changes in wealth (Bonica and Rosenthal, 2016). Although the students who

\textsuperscript{25}For a general discussion of uncertainty when measuring ideology, see Bailey and Spitzer (2018).

\textsuperscript{26}As discussed in The Economist (2018), “datasets do not line up in a way that makes the conjecture possible to prove, but it is a fair bet that, at least among those most engaged in politics, Americans are more likely to change their religion than to change their party.”
spend the most time editing the additional conservative article may have had a deep enough level of intellectual engagement to change their ideology, such a change is unlikely to occur for the board overall in a given year.

### 6.2 Missing Ideology Data

Our measure of ideology is based on political donations, and editors and authors that have not made a donation thus have missing ideology. For authors, missing ideology does not create any identification concerns. Because our sample of articles is restricted to those with at least one donating author, missing author ideologies changes the sample of articles. It therefore only changes the interpretation of the results.

For editors, however, missing ideology creates potential identification concerns. Because we define a board’s conservativeness based on the ideology of the donating editors, missing editor ideology may bias the results. We investigate whether missing data may be driving the results under three different assumptions. First, it is possible that editors’ propensity to donate is uncorrelated with their ideology. If so, missing editor ideologies would be drawn from the same ideological distribution as the editors in our sample who have donated, implying that editors are effectively missing at random. If so, missing ideology would introduce only classical measurement error, which would attenuate the estimated coefficients towards zero but would not bias the estimated coefficients.

If missing editor ideologies are random and introduce measurement error, restricting the sample to one in which we observe relatively more editor ideologies would decrease measurement error, leading to larger point estimates and more precision. We tested this possibility by restricting our sample to boards with at least 3 editors with donations and donations.

---

27See Lee (2017) for an example of law and economics research confronting classical measurement error.

28This discussion and analysis follows Bonica et al. (2019). These results are omitted for brevity, but they are available in the working paper version of this article.
to years when there were relatively more editors donating (1990 to 2000). We find that that the size of the point estimates increase, suggesting that measurement error for missing editor ideology was driving down the size of the estimates in Table 2.

Second, it is possible that missing editors do not have similar ideologies to the other members of their editorial board but have similar ideologies to their fellow law school classmates. To test how this possibility would change the results, we use data on the percentage of conservative graduates from each law school-year from Bonica et al. (2016). We then assume that editors with missing ideology are ideologically represented by the alumni of their law school in the five years around when they graduated and calculate the percent conservative editors for all the board—editors that made and have not made donations—after filling in the missing editor ideologies with this average conservativeness. Unlike the last tests that reduced measurement error by restricting the sample, this approach introduces measurement error into our measure of editor ideology. As expected, we find that the standard errors increase (more than doubling in some specifications) when using this approach. Although the estimates are not statistically significant in most specifications, the point estimates remain positive in each of them. This provides some evidence that observing the missing editor ideologies would not change the direction of the point estimates.

Third, it is possible that donating editors have systematically different ideologies from non-donating editors. If donors have different ideological preferences than non-donors, it could systematically bias the estimates. We investigate how the results would change if missing editors have less intense ideological preferences than donating editors. To do so, we

---

\(^{29}\)The results tell the same story using different thresholds.  
\(^{30}\)We are unaware of any reason to think that the ideologies of non-donors are meaningfully different from the ideologies of donors. Prior research using surveys and using CFscores has produced consistent estimates of the ideologies of lawyers. For instance, Peppers and Zorn (2008) surveyed Supreme Court clerks and found that 75 percent were democrats and 25 percent were republicans; when analyzing the same population using the CFscore, Bonica et al. (2017b) found the exact same breakdown to the percent. Similarly, Lindgren (2016) surveyed law professors and found 80 percent to be Democrats and 13 percent to be Republicans (the remaining 7 percent were independents): based on donating law professors’ CFscores, Bonica et al. (2018) found that 85 percent were liberal and 15 percent were conservative. We thus do not have reason to believe missing ideology data is actually biasing the results.
follow Bonica et al. (2019) and use a back-of-the-envelope adjustment to correct for missing data. Assume that in a journal-year the mean ideology of missing editors is equal to $\rho$ times the mean ideology of the non-missing editors:

$$e_{jt}^M = \rho e_{jt}$$

where $e_{jt}^M$ is the ideology of missing editors and $\rho < 1$ corresponds to the case in which editors with more intense ideological preferences are more likely to donate. In this case, the true ideology of editors, $e_{jt}^*$, is:

$$e_{jt}^* = (1 - \mu)e_{jt} + \mu e_{jt}^M$$

where $\mu$ is the proportion of missing editors. In this setting, Bonica et al. (2019) show that obtaining the true effect of ideology requires scaling the coefficient estimated from the sample of donors by a factor of $\frac{1}{1-\mu(1-\rho)}$. Given that we observe 34 percent of editor donations ($\mu = 0.66$), the point estimate in Table 2, Panel B, Column 3 of $\hat{\beta} = 0.093$ implies that the true coefficient is $\beta = \frac{0.093}{0.34 + 0.66\rho}$. Because $\rho$ is between 0 and 1, the true value of $\beta$ under these assumptions is between 0.093 and 0.273. As the extent to which donors have stronger ideological views relative to non-donors increases, the true coefficient increases. Because it is unlikely to be the case that editors with missing ideology actually have more intense preferences than those whose ideology we observe, the estimates likely provide a lower bound for the true effect.

Taken together, these results suggest that missing editor ideology may be moderately biasing the size of the estimated coefficients toward zero, but they do not suggest that missing editor ideology is driving our finding of a non-zero effect.

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31 Appendix E provides the derivation.
7 Conclusion

This article studied the role that political ideology plays in the selection process for law review articles. We matched the identities of student editors and authors of accepted articles to a measure of political ideology based on political donations. We find that the number of accepted articles written by conservative authors is increasing in the conservativeness of a law review’s editorial board. This finding contributes to the literature on disparate outcomes in the article selection process (Blank, 1991; Abrevaya and Hamermesh, 2012; Hengel, 2016; Colussi, 2018) by examining both a new setting (the legal academy) and a new dimension along which disparate outcomes can occur (political ideology).

We then investigated whether this relationship was driven by student editors having a preference for publishing articles that promote their political ideology or by student editors being objectively better at assessing the contribution of articles written by authors with shared ideology. To do so, we assessed whether articles whose authors and editors share an ideology are cited more than from articles whose authors and editors have different ideologies. We found evidence inconsistent with the preference explanation and consistent with the screening explanation. This second finding contributes to the literature exploring the underlying causes of disparate outcomes (Levitt, 2004; Antonovics and Knight, 2009; Ewens et al., 2014).

The results shed light on important debates in the legal academy. Academic careers are based on publishing (Frey and Rost, 2010), and the journal in which a scholar publishes exerts a strong influence on that individual’s job opportunities (Diamond, 1986). Moreover, academic articles published in journals remain the primary mechanism for disseminating research, so the journal in which an article is published affects the article’s reach and influence. The fact that ideology plays a role the selection process for law review articles thus has ramifications for both career trajectories and the dissemination of knowledge.
References


Figures and Tables

Figure 1: Example of a law review masthead

University of Pennsylvania
Law Review
FOUNDED 1852
Formerly American Law Register

BOARD OF OFFICERS
VOLUME 139

Managing Editor
EDWARD J. WEISS
Research & Writing Editor
KERIN SUE BISCHOFF
Articles Editors
STEVEN M. BUNKIN
EDWARD HERNSTADT
JEFFREY A. RACKOW
JOHN PETER SCAZER
GREGORY LAWRENCE WEINBERGER

Editor-in-Chief
ALEXANDER C. GAVIS

Executive Editors
KAREN J. BROTHERS
IRWIN PANITCH
BARRY L. REFSIN

Comment Editors
SU SUN BAI
CYNTHIA HAMILTON GURNEE
BENJAMIN G. ROBBINS
LARGON VAN NORDEN JR.

Editors
GUSTAVO ARNAVAT
HEIDI A. BECK
NEIL STUART BROMBERG
JAMES W. BUCKING
RUDOLPH CONTRERAS
MARTIN J. DOYLE
HOWARD M. EISENBERG
LISA E. FACTOR
MARC L. FROHMAN

JACQUELINE L. GLASSMAN
DAVID M. GOLDENBERG
ROBERT ANDREW KAPLAN
AARON RICHARD KRAUSS
LB KREGENOW
MICHAEL CARY LEVINE
MICHAEL LIEBERMAN
DORRETA MASSARDO
McGNNIS

DEBRA L. MOSES
Special Projects Editor
RICHARD J. PRATT
Special Projects Editor
J. DUANE PUGH JR.

Associate Editors
LYNN A. ADDINGTON
ANDREW D. AFRICK
BENJAMIN M. ALEXANDER
NAZAN ANVARAI*
MARTIN F. ARIAS
ALISON J. ARNOLD
KATHY M. BAUST
LILLIAN E. BENEDICT
STEVEN M. BERG
WILLIAM S. BIEL
GRAF J. BROWN
JEFFREY O. COOPER
JON B. DUBROW
DANIEL J. EPSTEIN
LOUIS E. FELDMAN
TERI L. FIRMES
LAWRENCE M. FRANKEL

JOEL E. FRIEDLANDER
MARCEL C. GARAU
TAMARA R. GELBOIN
JESSICA GOLDMAN
OLGA M. GOMEZ
PHILIP J. GOODMAN
CHARLES P. GOODWIN
BRIAN H. GRAPF
DOUGLAS F. HALLJAN
DOUGLAS W. HENKIN
J. BRENT HOOKER
S. RANDALL HUMM
FEETR A. JONES
AMY Y. KIM
WENDY S. LADER
A. ALLISON LISBONNE
FELICIA B. LISTWA

ERIC W. MCCORMICK*
ENIKO N. MIKSCHE
BEATE A. ORT
STEPHANIE A. PHILPS
BLAKE M. RHODES
DANIEL L. RIKARD
LAWRENCE D. ROSENBERG
ODED SALOMY
JOHN M. SCHIPERO
JONATHAN M. SHAW
STEVEN D. SILVERMAN
ADAM C. SILVERSTEIN
AMY SINDE
CYNTHIA SOOHO
MCAR H. SUPCOFF
DANIEL L. SUSSMAN
MICHAEL R. TEIN
DAVID S. WACHEN

DEBORAH J. SHORTER, Office Manager
ELLEN T. CHUNG, Office Staff
PHAN LAM, Office Staff
KIRA LEWIS, Office Staff
DENISE RUBIN, Office Staff
KEVIN WILS, Office Staff

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# Table 1: Descriptive statistics

## A. Editor Information

<table>
<thead>
<tr>
<th>Description</th>
<th>Value</th>
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</thead>
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<tr>
<td>Percent of Editors with Donations (%)</td>
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<tr>
<td>Percent of Editors with Donations that are Conservative (%)</td>
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## B. Author Information

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<td>Percent of Authors with Donations that are Conservative (%)</td>
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## C. Sample

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</thead>
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<td>Number of Articles</td>
<td>1,573</td>
</tr>
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<tr>
<td>At Least One Conservative Author (%)</td>
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</table>
Figure 2: Editors and authors with political donations over time

Electronic copy available at: https://ssrn.com/abstract=3119903
Figure 3: Distribution of author and editor ideology
Table 2: Relationship between editor ideology on author ideology

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<td><strong>A. Outcome: Percent Conservative Author</strong></td>
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<tr>
<td>Percent Conservative Editors</td>
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<td>0.096**</td>
<td>0.093**</td>
<td>0.120***</td>
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<td></td>
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<td>(0.035)</td>
<td>(0.037)</td>
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<td>0.148</td>
<td>0.148</td>
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</tr>
<tr>
<td><strong>B. Outcome: Mean Author CFscore</strong></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>Mean Editor CFscore</td>
<td>0.094*</td>
<td>0.090*</td>
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<td>Journal FE</td>
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<td>Gender and Race Controls</td>
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<tr>
<td>N</td>
<td>234</td>
<td>234</td>
<td>234</td>
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</table>

*Note: The regressions are at the journal-year level. Standard errors clustered by year in parentheses. * p<0.1, ** p<0.05, *** p<0.01.*
Figure 4: Sensitivity tests to controls and clustering

Note: The figure reports the point estimate and 90 percent confidence interval for a series of regressions. The left panel reports the results of regressing the percent of conservative authors on the percent of conservative editors. The right panel reports the results of regressing the mean CFscore of authors on the mean CFscore of editors. As indicated on the left hand side of the figure, the top section estimates the specifications at the article level, the middle section estimates the specifications at the journal-year level, and the bottom section estimates a first-difference specification at the journal-year level. Within a section, the controls correspond to the specifications in Table 2. For each combination of controls and level of analysis, the figure reports regressions without a clustering adjustment (as indicated by “No”), clustering by journal (as indicated by “Journal”), and clustering by year (as indicated by “Year”).
Table 3: Relationship between editor-author ideology and citations

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</thead>
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<td>ln(Citations)</td>
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<td>Percent Conservative Editors</td>
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<td>(0.18)</td>
<td>(0.15)</td>
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<tr>
<td>Percent Conservative Editors \times At Least one Conservative Author</td>
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<td>0.55**</td>
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<td>296</td>
<td>296</td>
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</table>

Note: The regressions are at the journal-year-ideology level. Standard errors clustered by year in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is restricted to journal-years in which at least one published article was written by a conservative author and at least one published article was written by a liberal author, so the sample size is lower than in Table 2. See text for details. Regressions are weighted by the number of articles. Main effect on “Conservative Author” is included in the specifications but is not reported.
Figure 5: Relationship between editor political ideology and difference in citations to articles from liberal and conservative authors

Note: The figure reports a binned scatterplot and a line of best fit of the relationship of interest. The y-axis is the difference between the natural log of the mean citations of articles written by conservative authors in a journal-year and the natural log of the mean citations of articles written by liberal authors in the same journal-year, where a positive number indicates that the articles written by conservative authors were cited more on average than the articles written by liberal authors. The x-axis is the percent of donating editors who are conservative.
Online Appendix

This online appendix provides additional information on five topics. Appendix A describes the survey we sent to former editors-in-chiefs to identify the board positions that had voting rights and the articles that were selected for their volume. Appendix B presents the distribution of voting editors by law review. Appendix C explains the process we used to match our sample to DIME. Appendix D reports summary statistics on ideology by law review. Appendix E provides technical details of the derivation of the adjustment for missing editor ideology.
A Appendix: Survey

For the survey, we had research assistants search the internet for email addresses of the editors-in-chief, and we emailed each of the editors whose email addresses we could locate. Figure A1 provides the text of the email. We automated the emails to contain a personalized note that included the names of people on the board who could have had a voting position based on the masthead of the law review in the given year.\textsuperscript{32}

Figure A2 reports the responses to the survey. Each line shows the years in the sample for a given law review, and each point on the line reports the outcome of the survey in that year. The different dots indicate whether (1) we could not locate an email address, (2) we did not receive a survey response, (3) we received a response consistent with the majority of responses for a journal, or (4) we received a response inconsistent with the other responses. The responses within a journal were overall very consistent, and any differences in the positions reported almost always differed only by one voting position. Of the 240 editors-in-chief in our sample, we were able to locate 205 email addresses. Of the 205 emails, we received 94 meaningful responses (some responded that they could not remember). Of the 94 responses, only 19 indicated voting positions that did not match the other responses.

Based on the survey, Table A1 reports the breakdown of the positions that vote across law reviews in our sample.

\textsuperscript{32}The idea for using names was that an editor might not remember the voting position but might remember the person who voted.
Dear [insert firstname],

I hope you won’t mind me contacting you out of the blue, with a random question. I teach law at UChicago, and I’m working on a project that studies how different top law reviews are structured, and how they make decisions about which articles to publish. As part of this project, I’m reaching out to EICs of top law reviews from past years, which is what led me to you. I have just two quick questions for you, and I’m very much hoping that you can help me out.

1. When you were EIC of the law review at [insert name of law school], which board members had a vote in deciding which articles to publish? Was it the EIC and the articles editors, or were there others? For example, did the following editors vote: [one high ranking title on the law review] (e.g., [name of one of the people with the title from the masthead]), [another high ranking title on the law review] (e.g., [name of one of the people with the title from the masthead]), or others I’m missing?

2. I see that you were the EIC of Volume [insert volume]. Do you happen to recall if all of the articles that your board selected were published in Volume [insert volume], or were there some published in Volumes [insert volume prior] or [insert volume after]?

Again, my apologies for contacting you out of the blue like this. I would be tremendously grateful for any information you can provide.

Many thanks,

Jonathan

Note: Figure A1 provides the exact wording of the email that we sent to the EICs in our sample. The emails were automated to include the editor’s first name, the names of people on the board whom we believed had a voting position based on the masthead of the law review in the given year, and the volume numbers for question two.
Figure A2: Survey responses

- Berkeley
- Chicago
- Columbia
- Cornell
- Duke
- Georgetown
- Michigan
- Northwestern
- Penn
- Stanford
- Texas
- UCLA
- Vanderbilt
- Virginia
- Yale

- Responded, Consistent
- Did Not Respond
- Could Not Locate
- Responded, Inconsistent
Table A1: Editorial positions with decision authority by law review

<table>
<thead>
<tr>
<th>Law Review</th>
<th>Editorial Positions with Decision Authority</th>
</tr>
</thead>
<tbody>
<tr>
<td>Berkeley</td>
<td>Senior Articles Editor, Articles Editors</td>
</tr>
<tr>
<td>Chicago</td>
<td>Editor-in-Chief, Executive Editor, Articles Editors</td>
</tr>
<tr>
<td>Columbia</td>
<td>Editor-in-Chief, Articles Editors</td>
</tr>
<tr>
<td>Cornell</td>
<td>Editor-in-Chief, Senior Articles Editor, Articles Editors</td>
</tr>
<tr>
<td>Duke</td>
<td>Editor-in-Chief, Managing Editor, Articles Editors, Executive Editors</td>
</tr>
<tr>
<td>Georgetown</td>
<td>Senior Articles Editor, Executive Articles Editors, Articles Editors</td>
</tr>
<tr>
<td>Michigan</td>
<td>Editor-in-Chief, Executive Articles Editor, Articles Editors</td>
</tr>
<tr>
<td>Northwestern</td>
<td>Editor-in-Chief, Articles Editors</td>
</tr>
<tr>
<td>Penn</td>
<td>Articles Editors</td>
</tr>
<tr>
<td>Stanford</td>
<td>Articles Editors</td>
</tr>
<tr>
<td>Texas</td>
<td>Editor-in-Chief, Articles Editors</td>
</tr>
<tr>
<td>UCLA</td>
<td>Editor-in-Chief, Chief Articles Editor, Articles Editors</td>
</tr>
<tr>
<td>Vanderbilt</td>
<td>Editor-in-Chief, Senior Articles Editor, Articles Editors</td>
</tr>
<tr>
<td>Virginia</td>
<td>Editor-in-Chief, Articles Development Editors, Articles Editors</td>
</tr>
<tr>
<td>Yale</td>
<td>Editor-in-Chief, Executive Editor, Articles Editors</td>
</tr>
</tbody>
</table>

Note: Based on survey of former student editors.
Appendix: Distribution of Voting Members

Figure B1: Distribution of number of voting members on law reviews

Electronic copy available at: https://ssrn.com/abstract=3119903
C Appendix: Matching

To match editors and authors to DIME, we make use of two previously established datasets. First, we match editors to their CFscores from the dataset in Bonica and Sen (2017). The dataset of lawyer CFscores in Bonica and Sen (2017) contains the law school each one attended and the year of graduation. In our dataset, we know the editors’ law schools and are able to use the volume in which the editor served on the law review to identify the year of graduation. To perform the match, we make use of the law school in which an editor graduated and the year of graduation. In particular, we require both the law school and the year of graduation to match perfectly, and then perform the fuzzy match based on names alone. This means that the match process looks for similar names of editor identities from the mastheads and lawyers identities from Bonica and Sen (2017) within a given law school and graduating year, which significantly improves match quality compared to matching off name and law school alone. Through this process, we were able to match 34 percent of editor identities to donation records.

Second, we match authors of law review articles to a dataset of law professor ideology from Bonica et al. (2018). That dataset contains the names of all law professors recorded in the 2012 AALS Directory of Law Teachers and the ideologies of donating professors. This means that we only use author ideology for authors who were law professors in 2012. We were able to match 58 percent of author identities to donation records.

To put these numbers in comparison, (Bonica et al., 2017b) matched 38 percent of district court clerks, 47 percent of circuit court clerks, and 66 percent of Supreme Court clerks to donation records. Similarly, Bonica et al. (2018) matched 64 percent of law professors to donation records, and Chilton and Posner (2015) matched 63 percent of law professors to donation records (notably, Bonica et al. (2018) matched professors through the DIME database and Chilton and Posner (2015) searched for individual law professors’ donation records on OpenSecrets.com, but both found nearly identical match rates). That said, it is important to note that our matching statistics are not perfectly comparable to prior work because we are matching to datasets that have already been matched to political donations (Bonica and Sen, 2017; Bonica et al., 2018). This means matching errors can occur with our match to the prior datasets and that matching errors to donations are already present in the dataset we match to. Of the editors and authors we were able to match to these prior datasets, the donation rates were 51 percent for editors and 58 for authors.

The data is available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/FQ6EPR. We recover lawyer names by matching the the “dime_cid” variable to DIME.
# Appendix: Ideology by Law Review

Table D1: Mean and standard deviation of political ideology by law review

<table>
<thead>
<tr>
<th>Percent Conservative Editors</th>
<th>Mean</th>
<th>St. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Berkeley</td>
<td>6</td>
<td>13</td>
</tr>
<tr>
<td>Chicago</td>
<td>41</td>
<td>34</td>
</tr>
<tr>
<td>Columbia</td>
<td>16</td>
<td>22</td>
</tr>
<tr>
<td>Cornell</td>
<td>12</td>
<td>20</td>
</tr>
<tr>
<td>Duke</td>
<td>19</td>
<td>31</td>
</tr>
<tr>
<td>Georgetown</td>
<td>16</td>
<td>22</td>
</tr>
<tr>
<td>Michigan</td>
<td>7</td>
<td>13</td>
</tr>
<tr>
<td>Northwestern</td>
<td>29</td>
<td>37</td>
</tr>
<tr>
<td>Penn</td>
<td>14</td>
<td>21</td>
</tr>
<tr>
<td>Stanford</td>
<td>7</td>
<td>16</td>
</tr>
<tr>
<td>Texas</td>
<td>41</td>
<td>25</td>
</tr>
<tr>
<td>UCLA</td>
<td>6</td>
<td>16</td>
</tr>
<tr>
<td>Vanderbilt</td>
<td>41</td>
<td>29</td>
</tr>
<tr>
<td>Virginia</td>
<td>21</td>
<td>36</td>
</tr>
<tr>
<td>Yale</td>
<td>27</td>
<td>30</td>
</tr>
</tbody>
</table>
This derivation of the adjustment for missing editor ideology closely follows a similar derivation in Bonica et al. (2019), who study the influence of Supreme Court clerk ideology on justice voting. Comparing this derivation to the clerk context, editors are the clerks and law reviews are the justices. Assume that in a journal-year the mean ideology of missing editors is equal to $\rho$ times the mean ideology of the non-missing editors.

$$e_{jt}^M = \rho e_{jt}$$

Here $e_{jt}^M$ represents the ideology of missing editors and $\rho < 1$ corresponds to the case in which editors with more intense ideological preferences are more likely to donate. In this case, the true ideology of editors, $e_{jt}^*$, would be:

$$e_{jt}^* = (1 - \mu)e_{jt} + \mu e_{jt}^M$$

where $\mu$ is the proportion of missing editors. Solving for $e_{jt}$:

$$e_{jt} = \frac{e_{jt}^*}{1 - \mu + \rho \mu}$$

Editors decision to accept articles in a journal-year is a function of the true editor ideology, $a_{jt} = \beta e_{jt}^* + \varepsilon_{jt}$. Plugging in $e_{jt}^*$ from above:

$$a_{jt} = \beta (1 - \mu + \rho \mu) e_{jt} + \varepsilon_{jt}$$

Given observed editor ideology ($e_{jt}$), regressing acceptance decisions ($a_{jt}$) on it yields an estimated coefficient of $\hat{\beta} = \beta (1 - \mu + \rho \mu)$. To obtain the true effect of editor ideology on acceptance decisions, the estimated coefficient is scaled according to:

$$\beta = \frac{\hat{\beta}}{1 - \mu + \rho \mu}$$