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Political Discrimination in the Law Review Selection Process

Adam Chilton, Jonathan Masur, & Kyle Rozema*

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The career trajectories of law professors and the dissemination of knowledge depend on the publication decisions of law review editors. However, 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 do so, we match data on the political ideology of student editors from 15 top law reviews over a twenty-year period to data on the political ideology of the authors of accepted articles. We find that editors accept articles in part because of shared political ideology with authors. That is, conservative editors are more likely to accept articles written by conservative authors, and liberal editors are more likely to accept articles written by liberal authors. We then investigate potential explanations for this ideological discrimination. One possibility is that student editors simply have a preference for publishing articles that promote their political ideology. Another possibility is that student editors are objectively better at assessing the contribution of articles written by authors with shared ideology. We find evidence that the ideological discrimination is driven by student editors' superior ability to ascertain the quality of articles that match their own ideology.

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

Keywords: Academia, Publication Process, Bias, Statistical Discrimination, Political Ideology, Law Professor, Law Review

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1 Introduction

Social scientists have long been interested in both documenting discrimination and investigating the underlying causes of it. Discrimination has been studied in many contexts, including the selection process for academic articles. This article investigates a new form of discrimination in the selection process for academic articles: discrimination on the basis of political ideology. A large social science literature finds that political ideology plays an important role in decision-making (e.g., Martin et al., 2004). Yet although there has been some research documenting the political diversity of the academic profession (e.g. Gross, 2013; Bonica et al., 2017) 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), we are unaware of any research that studies whether political ideology influences the article selection process. Any such evidence would be important because 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 by which research is disseminated, so the journal in which an article is published affects the article's reach and influence. The existence of ideological discrimination in the selection process for academic articles would thus have ramifications for both career trajectories and the dissemination of knowledge.

We study political discrimination in the selection process for law review articles. Law reviews are journals published by law schools and run by groups of law students and are the main outlets for legal scholarship. Student editors review submitted manuscripts and make acceptance decisions, generally without seeking expert review. Most research on discrimination in the article selection process investigates discrimination against an identifiable group of authors (known as one-sided discrimination, e.g., Ayres and Vars, 2000; Hengel,

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2016). However, some recent research investigates whether discrimination varies by shared attributes of authors and either editors or referees (known as two-sided discrimination).[1] We investigate the possibility of ideological two-sided discrimination by asking whether editors accept articles in part because of shared ideology with authors.

To study the role that political ideology plays in the article process, we obtain the identities of over 2,500 editors on the boards of 15 of the top law reviews over a twenty-year period using yearly mastheads of the editorial boards and information on the board positions that have voting rights on article offers. Next, we match the data on editors to the authors of the articles they accepted. Finally, we match editors and authors to a measure of their political ideology based on their political donations. Both editors and authors are politically active, with 51 percent of editors and 57 percent of authors having made political donations between 1979 and 2016.

We find strong evidence that the article selection process is driven in part by the relationship between the authors' and editors' political ideologies. Our estimates suggest that a 1 percentage point increase in the percent of conservative editors on a board increases the percent of articles published by conservative authors by 0.34 percent. To interpret the magnitude of this effect, 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 76 percentage points more conservatives than the most liberal board. Therefore, our estimates suggest that, on average, a law review would accept approximately 29 percent more articles written by conservative authors in a year with their most conservative editorial board compared to a year with their most liberal editorial board.

[1] We are aware of three articles on two-sided discrimination in the article selection process. Abrevaya and Hamermesh (2012) study discrimination by peer-review referees and find no evidence of discrimination from shared referee and author gender. Colussi (2017) studies discrimination by editors of peer-review journals and finds that an author's social connections to the editor improves publication outcomes. Yoon (2013) studies discrimination in the article selection process of student-edited law review articles and finds 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.

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Next, we investigate the extent to which this relationship is explained by either bias (taste-based discrimination) or statistical discrimination (information-based discrimination) (e.g., Levitt, 2004). By "bias" we mean editors having a taste for accepting or rejecting articles on the basis of ideology (Becker, 1957). This could stem from favoritism toward authors with shared ideology or from a desire to publish articles that promote their preferred political agenda. By "statistical discrimination" we mean editors being better able to screen the quality of articles promoting their political ideology (Arrow, 1973). This form of discrimination can be driven by higher levels of relevant knowledge or expertise. For example, conservative editors could have more expertise in the legal methodology of originalism and therefore have a greater ability to distinguish the quality of articles that utilize this predominantly conservative approach (and that are almost exclusively written by conservative authors) than articles that utilize a more typically liberal approach.[2]

Our investigation of bias and statistical discrimination in the law review selection process proceeds in two steps. First, we construct simple models of article selection under separate conditions of bias and statistical discrimination. Although both models predict that editors will accept more articles written by authors who share their ideology, the models have some conflicting predictions for the quality of different types of articles—in particular, articles written by authors with shared ideology and articles written by authors without shared ideology. The model of bias predicts average article quality to be lower for articles written by authors with shared ideology and higher for articles written by authors without shared ideology. The model of statistical discrimination predicts average article quality to be higher for articles written by authors with shared ideology and lower for articles written by authors without shared ideology. Second, we test these predictions using citations as a

[2]Prior studies have demonstrated a similar type of statistical discrimination in other contexts, such as how discrimination in health care can result in part from physicians having a diminished capacity to understand the symptoms of minority patients (Balsa and McGuire, 2001) and how evaluators of NIH grants are better able to distinguish quality of grant proposals in their particular field of medical expertise from informational advantages (Li, 2017).

measure of article quality. In particular, we assess whether a relationship exists between article quality and the ideological distance between authors and editors. We find that the quality of articles from liberal authors is decreasing in the conservativeness of the board and that the quality of articles from conservative authors is increasing in the conservativeness of the board. Both findings are consistent with statistical discrimination and inconsistent with bias as the causal mechanism for editors selecting more articles written by authors of similar ideology.

This article proceeds as follows. Section 2 discusses the institutional setting and describes why it is particularly well-suited to study political discrimination in the article selection process. Section 3 describes the data and reports descriptive statistics. Section 4 presents the identification strategy and reports the results. Section 5 investigates mechanisms. Section 6 describes a set of robustness checks and reports the results. Section 7 first discusses limitations and policy implications and then concludes.

2 Institutional Setting

Discrimination in the article selection process has been studied across a wide range of fields from biology (Borsuk et al., 2009) to computer science (Tomkins et al., 2017) to ecology (Budden et al., 2008) to economics (Blank, 1991) to medicine (Gilbert et al., 1994) to psychology (Lloyd, 1990). Most of this evidence concerns referee discrimination, but some addresses editor discrimination (e.g. Laband and Piette, 1994). Although most studies typically find no referee discrimination, there are exceptions. Notably, Hengel (2016) finds some evidence of discrimination against women and Tomkins et al. (2017) finds discrimination in favor of famous authors.

Building on a small but growing literature on two-sided discrimination (e.g., Dillingham et al., 1994; Price and Wolfers, 2010; Parsons et al., 2011), this article inves-

tigates two-sided political discrimination in the publication process for law review articles. There are several features of the institutional setting that make it well-suited for studying this question. First, the same pool of articles are considered by each law review board. Most academic journals 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 and different reviewers. This is not true of law reviews. Twice per year (February and August), authors that submit to one top law review almost exhaustively submit to all top law reviews. This simultaneous submission setting overcomes concerns of selection into journals and referees on the basis of ideology.

Second, there are not selection concerns based on social connections (Colussi, 2017). In peer-reviewed social science journals, the editors and potential authors share many 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. In our setting, student editors have very few social ties with law professors, particularly law professors from other schools, so there is less concern of selection due to social connections.

Third, the law review setting allows us to overcome the data limitations of studying political discrimination in the publication process of peer review articles. 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, but 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 identities of editors of law reviews are available on mastheads for each volume.

There are a few more features of the institution that provide a nice setting for

identification of discrimination more generally. First, there is a lot of editor turnover. Unlike editors of peer reviewed journal, law review boards turn over each year. This means there is year-to-year variation in the individuals involved in the article review and selection process. Second, we are able to compare the outcomes of multiple articles for the same editors. 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 discrimination in the law review selection process. One identification problem when studying discrimination in peer review journals is that there could be both editorial and referee discrimination acting simultaneously, making it difficult to disentangle their effects. For example, there could be three-way interactions between authors, editors, and referees. Unlike peer review articles that have both editors and referees, editors of law reviews both review the articles for merit and make the acceptance decisions.[3]

3 Data and Descriptive Statistics

We built a dataset that contains an estimate of the ideologies of both law review editors and the authors of the articles they accept, as well as the citations of each article. Our sample includes the following 15 law reviews: California Law Review, Columbia Law Review, Cornell Law Review, Duke Law Journal, Georgetown Law Journal, Michigan Law Review, Northwestern University Law Review, Stanford Law Review, Texas Law Review, UCLA Law Review, University of Chicago Law Review, University of Pennsylvania Law Review, Vanderbilt Law Review, Virginia Law Review, and Yale Law Journal. Our goal was to use a sample of articles from 15 of the top law reviews over a twenty-year period, and the law reviews at Harvard and New York University are not included in the sample because of an inability to link voting boards to the articles they accepted. We use the time period of

[3]Only recently and only in a few law reviews (e.g., Harvard, Yale) have articles been sent out to expert faculty referees. During our sample period (1990 to 2010), we are aware of no evidence of expert reviews in law review article selection.

1990 to 2010. We use 2010 as the end year because most recent law school graduates do not make political donations for a number of years after graduation.

Voting Members on Law Review. Each law review allows different board members (with different titles) to vote on which articles to accept. Although both the name and voting rights of board positions differ between law reviews and even within a law review over time, most law reviews have a select group of board positions that have voting rights. We surveyed members of past boards in top law reviews to determine the voting members and the articles selected by the board. The survey was based on a convenient sample (in most cases, individuals whom we knew personally who served on the law review boards). 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 rejected and which are put up for a vote. We therefore define the board as including high ranking members even if they do not ultimately vote, but the results are consistent if we define the board as only voting members.

Student Editor Identities from Masthead. Each year, each law review selects a new board. Each volume of a law review contains a masthead page which identifies the identities and position of each member of the law review. For example, Figure 1 shows the masthead of the University of Pennsylvania Law Review for Volume 139. We obtained the mastheads for each volume of 15 of the top law reviews from 1990 to 2010. From the mastheads, we hand-coded the name of each voting member and his or her position. For law reviews where the high-ranking positions do not have voting rights, we also hand-coded their names and

positions. In particular, because 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.[4] In total, we coded the identities of 2,745 editors.

Published Articles and Their Citations. Heinonline is a website that contains information about law review publications. We scraped information about each publication between 1990 and 2010 in the 15 law reviews in our sample. This includes the title, the identity of each author, the volume, the issue, and the number of citations of that article. Each volume of a law review usually contains multiple non-article publications. The law review page indicates whether a given publication is a comment, student note, or book review, none of which is selected through the same mechanism as typical articles. We use this information to exclude non-article publications. This leaves us with 9,055 articles.

Measure of Political Ideology. Our data on political ideology is drawn from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2014, 2016). DIME was constructed from a database of all political contributions—whether in local, state, or federal elections—made from 1979 to 2016 and disclosed to the Federal Election Commission. This comprises more than 250 million donations made by more than 20 million unique donors. DIME then translates political contributions into ideology scores, which are called "Campaign Finance scores" ("CFscores"), by linking contributions with the candidates who received them. First, candidates are assigned unidimensional ideological scores based upon 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.[5] For instance, Bernie Sanders has a

[4]For non-voting members, we exclude editors in charge of comments and book reviews even if they are at or above the lowest voting member on the masthead.

[5]This measure has been validated and employed in prior research (Bonica, 2014; Bonica and Sen, 2017; Thomsen, 2014; Chilton and Posner, 2015; Wood and Spencer, 2016). For example, Bonica (2017) validates CFscores against "policy preferences for a wide range of issues and successfully discriminate within party."

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 for whom they gave. For instance, if an individual's only political contribution was to Mitt Romney, her CFscore would match Romney's: 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 ($1,000/$3,000) and 2/3 of Sanders' score ($2,000/$3,000), or -1.65. In most of our regressions, we simply classify individual donors as "conservative" if their CFscore places them at or above the average American donor (CFscore$\geq$0) and "liberal" if their CFscore places them below the average American donor (CFscore$<$0).

The measure of ideology is thus available for editors and authors who donated at at least once from 1979 to 2016. One concern with this measure of ideology is that it is based on lifetime donations. For the editors, few donated before law school, so we construct the measure of ideology based on donations over the course of the editor's life. This approach does not likely raise identification concerns in our context for two reasons. First, ideology of adults has been shown to be stable (e.g., Bonica, 2014), and research suggested that typically only extreme life changes change ideology, e.g., divorce and religious conversion (Green et al., 2004). 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 our estimates. The main concern for identification would be if the process of reviewing articles caused editors' ideologies to change, but this is unlikely to be the case. We doubt that the act of editing an article is the type of intellectual engagement that is likely to change their ideology. Therefore, to the extent that editor ideology has changed since law school, any changes are likely orthogonal to the editor's experience on the law review and

therefore likely only a source of measurement error.

To match editors and authors to their CFscores, we make use of two previously established datasets. First, we match editors to their CFscores from the dataset in Bonica and Sen (2017).[6] We perform a fuzzy match based on the editor's first and last name and the law school each editor graduated from. Second, we match authors of law review articles to a dataset of law professor ideology from Bonica et al. (2017). 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.

Descriptive Statistics. Table 1 provides descriptive statistics. Panel A describes the full sample of editors and Panel B describes the full sample of authors. Over the sample of as a whole, 51 percent of editors and 57 percent of law professor authors made donations. These are high rates compared to the US population as a whole (5 percent) and relative to lawyers in general (41 percent) (Bonica et al., 2016). Figure 2 plots the percent of editors and authors who made donations over time.[7] These donations rates are consistent with previous research showing that both elite lawyers and law professors are politically active and tend to make donations at high rates (see, e.g., Bonica et al., 2017).

Both editors and authors are also quite liberal. Defining an individual as conservative based on the average donating American (CFscore$\geq$0), 22 percent of editors and 15 percent of authors are conservative. The ideology of editors and authors is consistent with previous research (see, e.g., Bonica et al., 2016, 2017).

Panel C provides descriptive statistics of the final sample used in the empirical

[6] 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 (Bonica, 2016).

[7] Note that a significant number of editor and author identities were not matched to an identity in the two datasets previously described. The donation rate is that of the matched sample.

analysis. The final sample is restricted to articles where at least one author donated and where at least one editor donated. For coauthored articles, we define an article as conservative if at least one author is conservative.[8] This approach follows that in Colussi (2017), who defines an article that is 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. The final sample contains 2,852 articles. The average number of articles per law review-volume in the final sample is 10, with 10 percent of volume-law reviews having more than 15 articles.

Our measure of board ideology is at the board-year level. We use the percent of editors that are conservative. The average board has 21 percent conservative editors, and 16 percent of articles have at least one conservative author. Figure 3 provides the distributions of the percent of conservative authors and editors per volume.

4 Primary Results

This Section investigates whether the law review article selection process is driven in part by the relationship between the authors' and editors' political ideologies. Figure 4 provides a graphical illustration of the relationship between author and editor political ideology. The figure reports a binscatter at the law review-year level. The x-axis indicates the percent of donating editors that are conservative and the y-axis indicates the percent of articles with donating authors that has at least one conservative author. The figure shows a positive relationship where a 1 percentage point increase in the percent of conservative editors is associated with a 0.3 percentage point increase in the percent of conservative authors ($p < 0.01$).

To more formally test the relationship between editor and author political ideology,

[8]The results are consistent to alternative ways of classifying co-authored articles, see Section 6.2.

we estimate Equation 1:

$$a_{ijt} = \alpha + \beta e_{jt} + \phi_t + \eta_j + \zeta_{jt} + \epsilon_{ijt} \tag{1}$$

for article i published in law review j in year t. The dependent variable a_{ijt} is an indicator variable for whether there is at least one conservative author on article i (defined as CFscore$\geq$ 0). The independent variable of interest e_{jt} is the percent of donating editors that are conservative. This means that the ideology of authors vary within a law review-year but editor ideology is constant within the law review-year. In the preferred specification, we add year fixed effects ϕ_t, law review fixed effects η_j, and law review-specific time trends ζ_{jt}.

Table 2 reports the results of estimating Equation 1. Column 1 has no controls, Column 2 adds year fixed effects, Column 3 adds law review fixed effects, and Column 4 adds law review-specific time trends. It shows that the positive relationship holds after controlling for year and law review fixed effects as well as law review-specific time trends. The results in Column 4 indicate that a 1 percentage point increase in the percent of conservative editors on a board increases the percent of articles published by conservative authors by 0.34 percent (0.0006 percentage points from a baseline of 0.156). To interpret the magnitude of this effect, consider the ideological differences between law reviews' least and most conservative boards. On average, a law review's least conservative board has 76 percent fewer conservatives than the law review's most conservative board. Therefore, our estimate suggests that, on average, a law review would accept approximately 29 percent more articles with conservative authors in a year with their most conservative editors compared to a year with their most liberal editors.

5 Mechanisms: Bias and Statistical Discrimination

This Section investigates the mechanisms driving the relationship between editor and author political ideology. In general, discrimination can take one (or both) of two possible forms: taste-based discrimination (bias) or information-based discrimination (statistical discrimination). Bias occurs if editors have a taste for accepting or rejecting articles on the basis of ideology (Becker, 1957). Statistical discrimination can occur if editors are risk averse and cannot observe the true quality of articles. Statistical discrimination occurs if editors, with no discriminatory motives, attempt to choose the highest-quality articles but are better able to screen the quality of articles promoting their political ideology (Aigner and Cain, 1977; Arrow, 1973; Phelps, 1972).

This Section proceeds in two parts. In Section 5.1, we derive predictions about the quality of articles selected under simple models of bias and statistical discrimination.[9] In Section 5.2, we test these predictions using citations of articles as a measure of quality.

5.1 Deriving Testable Predictions of Bias and Statistical Discrimination from Citations

Consider a model of article selection for a single law review volume. Editors receive I article submissions from which to accept $N < I$. Articles are denoted by $i = 1, ..., i = I$. Drawing on the two-party political system in the United States, suppose that the relationship between editor and author ideology is discrete and symmetrical such that editors and authors either share an ideology or do not share an ideology. Then, we distinguish between articles selected "with type" (where the editors and authors share an ideology) and articles selected "against type" (where the editors and authors are different ideologies). For example, an

[9] We use a simple static model to derive predictions, but acknowledge that articles are selected in equilibrium where articles selected by one journal influences the articles accepted by another journal. Among other things, a general equilibrium model would account for the fact that more conservative articles selected by one of the top law reviews could lead to fewer conservative articles being selected by lower ranked journals.

article is selected with type if (1) both the editors and the authors are conservative, or (2) both the editors and the authors are liberal; an article is selected against type if (3) the editors are conservative and the authors are liberal, or (4) the editors are liberal and the authors are conservative.

Let the q_i be the quality of each article. Suppose the objective function of selecting articles is to maximize article quality. That is, the editors are attempting to select the highest quality articles possible to accept for their law review. (Based upon our conversations with current and former editors involved in article selection, this is what law review editors claim to be doing.) In a world without bias or statistical discrimination and article quality is observable, editors sort articles from highest quality to lowest quality and choose the quality-maximizing set of articles. Assume that the quality maximizing set contains more than one article selected with type and more than one article against type. To inform a statistical test for the main mechanism at work using data on citations as a measure of quality, we will now form theoretical predictions about how the quality of with and against type articles changes under a model of bias and a model of statistical discrimination.

Model with Bias. Assume quality is observable to the editors, and suppose there is bias in favor of articles with type. To assess how quality of articles with and against type will change with bias, take the simplest case where only the marginal article against type that is in the quality maximizing set is not selected and that the marginal article with type that is not in the quality maximizing set is now selected.

> ***Prediction 1. Bias: Article Composition.*** *If editors are biased for articles with type or biased against articles against type, there will be more articles with type accepted and less articles against type accepted than if the editors do not have political bias.*

Editors choose the set of with-type articles to maximize the quality of with-type articles, and separately choose the set of against-type articles to maximize the quality of

against-type articles. The only difference between selection is that an additional with-type article is chosen and one fewer against-type article is chosen.

For the articles selected against type, bias here simply removes the lowest quality element from the against-type set. This, by construction, implies that the average quality of the against-type articles will be higher than the average quality of the against-type articles selected in a model without bias.

> ***Prediction 2. Bias: Quality of Articles Against Type.*** *If editors have political bias, average quality of articles against type will be higher than if editors do not have political bias.*

For the articles selected with type, bias here simply adds an element to the with-type set that is lower quality than the lowest quality element of the quality maximizing set. This, by construction, implies that the average quality of the with-type articles will be lower than the average quality of the with-type articles selected in a model without bias.

> ***Prediction 3: Bias: Quality of Articles With Type.*** *If editors have political bias, average quality of articles with type will be lower than if editors do not have political bias.*

Model with Statistical Discrimination. Suppose there is no bias, but that the quality of articles is not directly observed. Instead, editors get a noisy signal of quality

$$y_i = q_i + \varepsilon_i$$

$$\varepsilon_i = \theta_i \eta$$

where η is a mean zero random variable and $\theta_i = \theta_0$ for against type articles and $\theta_i = \theta_1$ for with type articles. Assume $\theta_0 = \theta_1 + \delta$, where $\delta \geq 0$ and $1 \geq \theta_1 \geq \theta_0 \geq 0$. This setting implies that there is noise for all parameter values but that the extent of statistical discrimination, as reflected in the magnitude of δ, can vary between articles with type and

articles against type. If $\delta = 0$, no statistical discrimination would exist but there still exists noise for each type of article.

Donate $\mathcal{A}_{\theta_0,\theta_1}$ as a set of accepted articles when the signals have noise parameters θ_0, θ_1. Note that because this set inherits the randomness in the quality signals, this set is itself a random variable. Donate $\alpha \in [0,1]$ as the fraction of the accepted articles that are with type (i.e., α captures the composition of with and against type articles). Finally, denote $\mu_0(\mathcal{A}_{\theta_0,\theta_1})$ as the average true quality of the articles selected with type and denote $\mu_1(\mathcal{A}_{\theta_0,\theta_1})$ as the average true quality of the articles selected against type.

Then, for $\theta_0' > \theta_0$ and holding θ_1 constant, if editors are risk averse we have

$$\mathbb{E}[\alpha(\mathcal{A}_{(\theta_0,\theta_1)})] > \mathbb{E}[\alpha(\mathcal{A}_{(\theta_0',\theta_1)})]$$

where $\alpha(\mathcal{A})$ gives the fraction of accepted articles that is with type. In words, as the noisiness of the against type signals increase, risk aversion implies that articles with type look more attractive relative to articles against type, which causes editors to select more articles with type.

> ***Prediction 4: Statistical Discrimination: Article Composition*** *If editors cannot observe the true quality of articles and the quality signals of articles against type are noisier than the quality signals of articles with type, there will be more articles with type accepted and fewer articles against type accepted than if the quality signals of articles against type are the same as the quality signals of articles with type.*

Next, given a particular value α and $(\theta_0' = \theta_1 + \delta') > (\theta_0 = \theta_1 + \delta)$, we must have

$$\mathbb{E}\big[\mu_0\big(\mathcal{A}_{(\theta_0',\theta_1)}\big)|\alpha\big] < \mathbb{E}\big[\mu_0\big(\mathcal{A}_{(\theta_0,\theta_1)}\big)|\alpha\big]$$

> ***Prediction 5: Statistical Discrimination: Quality of Articles Against Type.*** *If editors cannot observe the true quality of articles, and holding constant*

the number of articles selected against type, expected average quality of articles against type will be lower as a result of statistical discrimination.

Finally, given a particular value α and $\theta_1' < \theta_1$, we must have

$$\mathbb{E}\big[\mu_1\big(\mathcal{A}_{(\theta_0,\theta_1')}\big)|\alpha\big] > \mathbb{E}\big[\mu_1\big(\mathcal{A}_{(\theta_0,\theta_1)}\big)|\alpha\big]$$

Prediction 6. Statistical Discrimination: Quality of Articles With Type. *If editors cannot observe the true quality of articles, and holding constant the number of articles selected with type, expected average quality of articles with type will be higher as a result of statistical discrimination.*

Some Conflicting Quality Predictions from Bias and Statistical Discrimination. Models of bias and statistical discrimination change the composition of the articles selected in the same direction. Predictions 1 and 3 suggest that bias and statistical discrimination will cause editors to select more articles with type. This implies that the relationship between editor and author ideology could be explained by either bias or statistical discrimination, or both. However, it might be possible to disentangle bias and statistical discrimination by assessing differences in quality of accepted articles with type and accepted articles against type.

If the dominant type of discrimination is taste-based, Prediction 2 suggests that the quality of articles against type will be higher because fewer lower quality articles against type are accepted, and Prediction 3 suggests that the quality of articles with type will be lower because more lower quality articles with type are accepted. If the dominant type of discrimination is information-based, Prediction 5 suggests that the quality of some articles against type will decrease, and Prediction 6 suggests that the quality of some articles with type will increase. However, because Prediction 4 suggests that statistical discrimination will lead to more lower quality articles with type to be accepted and fewer lower quality articles against type to be accepted, the equilibrium quality of accepted articles with type and

articles against type can therefore change in either direction. This highlights the narrowness of Predictions 5 and 6 (the predictions are conditional on a constant composition). Therefore, it is not necessarily the case that we can distinguish bias from statistical discrimination by assessing differences in article quality between articles with type and articles against type.

However, there is one set of outcomes in which we can distinguish bias and statistical discrimination. If we observe that accepted articles with type are on average higher quality than we would expect, this would suggest that (1) the increase in quality of accepted articles with type resulting from greater ability to screen the quality of articles with type first order dominates (2) any decrease in quality of accepted articles with type resulting from more articles with type being selected. Moreover, if we observe that accepted articles against type are on average lower quality than we would expect, this would suggest that (3) the decrease in quality of accepted articles against type from worse ability to screen the quality of articles against type first order dominates (4) the increase in quality of accepted articles against type resulting from fewer articles with type being accepted. These findings would provide evidence that is consistent with the model of statistical discrimination and inconsistent with the model of bias.

5.2 Results: Citations as a Test for Bias or Statistical Discrimination

Using data on article citations, we test these predictions about whether bias or statistical discrimination is driving the relationship between editor and author ideology by estimating Equation 2.

$$\ln(\text{citations}_{jt}) = \alpha + \beta e_{jt} + \phi_t + \eta_j + \mu_{jt} + \epsilon_{ijt} \tag{2}$$

where $\ln(\text{citations}_{jt})$ is the natural log of the average article citations published in law review j in year t. The variable e_{jt} is again the percent of donating editors that are conservative.

ϕ_t are year fixed effects. η_j are law review fixed effects. μ_{jt} are law review-specific time trends.[10] The coefficient β on the variable e_{jt} estimates the relationship between board conservativeness and citations. It indicates the percent increase in citations for a 1 percentage point increase in the donating editors that are conservative. The goal here is to assess differences in quality between articles with type and articles against type within a board. We therefore restrict the sample to law review-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. This means that the sample size is lower than in Table 2.

Table 3 presents the results. Column 1 is restricted to the sample of articles written by liberal authors and Column 2 is restricted to the sample of articles written by conservative authors. We have already observed that the composition of articles by liberal and conservative authors depends on the ideology of the editors, implying subsetting the data by author ideology creates classical sample selection bias (Heckman, 1979). However, our goal here is not to estimate a causal relationship but rather to test model predictions using equilibrium citations, so the results even with sample selection bias is informative by design.

Column 1 addresses the conflict between Predictions 2 and 5 by estimating the relationship between editor conservativeness and citations for articles written by liberal authors. We find that a 10 percentage point increase in the percent of conservative editors decreases average citations of liberal authors by 1.6 percent. This is inconsistent with Prediction 2 from the model of bias but consistent with Prediction 5 from the model of statistical discrimination: as the ideological distance between editors and liberal authors increases (an increase in the percent conservative editors), the average quality of accepted articles decreases.

Column 2 addresses the conflict between Predictions 3 and 6 by estimating the

[10]The size and significance of the estimates do not materially change if law review-specific time trends are not included.

relationship between editor conservativeness and citations for articles written by conservative authors. We find that a 10 percentage point increase in the percent of conservative editors increases average citations of conservative authors by 2.1 percent. (The point estimate falls just short of statistical significant but the size and precision of the point estimates are stable without law review-specific time trends.) This is inconsistent with Prediction 2 from the model of bias but consistent with Prediction 5 from the model of statistical discrimination: as the ideological distance between editors and liberal authors increases (an increase in the percent conservative editors), the average quality of accepted articles decreases.

The results in Columns 1 and 2 are based on subsetting the data by conservative authors and liberal authors. To more formally test whether more conservative boards are worse at selecting liberal articles and better at selecting conservative articles, we generate a dataset where the unique observation is the law review-year-author ideology. That is, for each law review-year, there are two observations: the average number of citations for liberal authors and the average number of citations for conservative authors. We then estimate a regression similar to that in Equation 2 but with the addition of an indicator variable for conservative author and an interaction term for conservative author and the percent of conservative editors. The interaction term captures the differential effect of the conservativeness of the editorial board on citations of articles written by conservative authors. The results are reported in Column 3 and 4. Column 3 includes only the main effect on percent conservative editors and Column 4 adds the interaction term. We find a very similar pattern to that in Columns 1 and 2. (The size and significance of the point estimates are stable without law review-specific time trends.)

Combined with the results in the previous section, the evidence suggests that even with selecting more conservative articles, conservative editors nonetheless select on average higher quality conservative articles. Moreover, conservative editors select fewer liberal articles, but even those fewer liberal articles are lower quality than we would expect.

6 Robustness Checks

This Section assesses the robustness of the results in four ways. Section 6.1 assesses the relationship between author and editor political ideology under alternative definitions of ideology. Section 6.2 assesses the relationship between author and editor political ideology under alternative ways of handing co-authored articles. Section 6.3 assesses how missing editor ideologies influences the relationship between between author and editor political ideology. Section 6.4 assesses the use of article citations as a measure of article quality.

6.1 Alternative Definitions of Ideology

Above, we defined each donating editor and author as either liberal or conservative based on a standard measure of political ideology: whether their average political donation was given to a candidate that was more conservative than the average donating American (CFscore$\geq$0). Measuring the political ideology of any individual in this discrete fashion allowed us to construct an intuitive measure of board ideology (the percent of conservative editors) and estimate an intuitive relationship between editor and author ideology (the relationship between a percentage point change in conservative editors and the percentage point change in conservative authors).

Table 4 reports the main results using alternative definitions for author and editor ideology. We replicate the specifications in Table 2 three times. In Panel A, we use the same outcome—whether at least one of the authors is conservative—but use the mean CFscore of editors as the independent variable. In Panel B, we use the mean CFscore of the authors but use the original independent variable—the percent conservative editors. In Panel C, we use the mean CFscore of the authors and the editors. As can be seen, the results are consistent across each of the definitions. In Panel A, for example, we find in Column 4 that a 1 standard deviation shift in political ideology of the average editor with respect to the US

population is associated with a 15 percent increase in the percent of conservative authors (0.023 from a baseline of 0.156). Although we lose significance with law review fixed effects in most models, the size of the point estimates are fairly stable and in all models positive.

6.2 Alternative Ways of Handing Co-Authored Articles

Above, we defined an article with multiple authors as conservative if at least one author is conservative. This definition was motivated by Colussi (2017), who defines an article that is authored by more than one author as having a social connection with the editor if at least one author is has a social connection to an editor. Table 5 replicates the specifications in Table 2 using alternative ways of handling co-authored articles. Panel A reports the results where the unit of observation is the donating author rather than the article. Panel B reports the results defining an article as conservative only if there is at least one conservative author and no liberal authors. As can be seen, the results are not sensitive to the way of handling co-authored articles.

6.3 Missing Ideology

Our measure of editor and author ideology is based on political donations. In our sample, 51 percent of editors and 57 percent of law professor authors made donations. Editors and authors that have not made a donation have missing ideology. 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 changes the interpretation of the results but does not create any identification concerns. Our focus here deals with potential concerns related to missing ideology of editors. Above, we define a board's conservativeness based on the ideology of the donating editors. Editors that donate may differ in unobservable ways from the editors that do not donate. If missing editor ideology is random, this introduces classic measurement error on the independent variable. If missing editor ideology is not

random, it could introduce bias.

Table 6 assesses the extent that missing ideology of editors influences the results. Each robustness check replicates the specifications in Table 2 with a different sample or redefines the way we calculate the percent conservative editors. First, Panel A drops boards where only a few editors have made donations (in particular, we require at least 3 editors to have donated, but the results tell the same story using different thresholds). Second, Panel B exploits the fact that editors in earlier years have made more donations (see Figure 2) by restricting the sample to years when there were relatively more editors donating (1990 to 2000). 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. In both panels, 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 main point estimates in Table 2.

In a final approach, instead of restricting the sample to assess whether the missing editor ideology is consistent with measurement error, we actually introduce more measurement error into the measure of editor ideology to assess the extent that the number of missing editor ideologies can change the results. Using data on the percentage of conservative graduates from each law school-year from Bonica et al. (2016), Panel C assumes that editors with missing ideology are ideologically represented by the alumni of their law school in the five years around when they graduated. We then 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. As expected, the standard errors increase (more than doubling in some specifications). Although the estimates are not statistically significant in any specification, the point estimates remain positive in each of the specifications. This provides some evidence that observing the missing editor ideologies would not change the direction of the point estimates.

6.4 Citation as a Measure of Quality

Above, we used citations as a measure of quality. We implicitly assumed that citations are exogenous to political ideology. One concern is the possibility that law reviews have reputations as being liberal or conservative. If so, law reviews could have different ideological compositions of readers, which could influence citations. To assess this concern, we test for differences in citations between articles by liberal and conservative authors at the same law review. We find no evidence of differences in citations between articles by liberal and conservative authors. At the law review-year level, articles by liberal authors are cited on average 82.9 times and articles by conservative authors are cited on average 81.2 times. This difference in means does not come close to reaching statistical significance at conventional levels ($p < 0.73$).

7 Conclusion

This article studied political discrimination in the selection process for articles in student edited law reviews. We matched the identities of the editors of law reviews and authors of accepted articles to a measure of political ideology based on political donations. We found strong evidence that the selection process for law review articles is driven in part because of the shared political ideology of authors and editors. This finding contributes to the literature on discrimination in the article selection process (Blank, 1991; Abrevaya and Hamermesh, 2012; Hengel, 2016; Colussi, 2017) by examining both a new setting (the legal academy) and a new dimension along which discrimination can occur (political ideology). To investigate whether this relationship was driven by taste-based or information-based discrimination, we then used article citations as a measure of article quality and assessed whether articles whose authors and editors share an ideology differ in quality from articles whose authors and editors have different ideologies. We found evidence consistent with statisti-

cal discrimination and inconsistent with bias as the causal mechanism for editors selecting more articles written by authors of similar ideology. This second finding contributes to the literature exploring the underlying causes of discrimination (Levitt, 2004; Antonovics and Knight, 2009; Ewens et al., 2014) by again examining both a new setting (law review article selection) and a new dimension along which discrimination can occur (political ideology).

The results also shed light on important debates in the legal academy. First, the paper highlights a potential mechanism driving ideological diversity in the legal academy, particularly on the faculties of the very best law schools. On the one hand, because law review editors at elite law schools tend to be liberal, the fact that author ideology matters in the selection process could contribute to conservatives publishing in lower ranked journals. On the other hand, because law review editors at elite law schools are relatively more conservative than law professors, the fact that author ideology matters in the selection process could contribute to conservatives publishing in higher ranked journals. Either way, the fact that author ideology matters can influence outcomes on the entry-level and lateral legal academic markets.

Second, the results contribute to the debate within academia on the relative merits of selecting articles via peer review or through a student-run process. Our finding that discrimination is information-based lends support to the "common criticism . . . that law students lack the experience or training to effectively evaluate legal scholarship" (Posner, 1995). It could be the case that political discrimination in the article selection process is unique to law reviews because they are edited by law students and often legal issues do not have a single correct answer. Indeed, one might expect statistical discrimination to be less severe for peer reviewers because they have greater expertise in the academic discipline. However, one might also expect editors and referees in peer reviewed journals to be more likely to know the politics of authors and to be better able to discern whether the research promotes their preferred policy agenda. Our findings motivate further inquiry into what role

politics play in the article selection process in peer reviewed journals and across disciplines.

Finally, the results have potentially important welfare implications. Because legal scholarship is often cited and used by judges in deciding cases and by policy-makers to inform policy debates, the relationship between editor and author ideology can affect the dissemination of knowledge and influence cases and policies. If articles published in higher-ranking law reviews are more likely to influence judges or policy-makers, discrimination in the article selection process at the top law reviews could skew judicial and policy outcomes. This issue may warrant further study as well.

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Figures and Tables

Figure 1: Example of a law review masthead

University of Pennsylvania Law Review

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Formerly American Law Register

BOARD OF OFFICERS
VOLUME 139

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DANIEL L. SUSSMAN
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DAVID S. WACHEN

* On leave, 1990-91

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ELLEN T.I. CHUNG, *Office Staff*
PHAN LAM, *Office Staff*
KIRA LEWIS, *Office Staff*
DENISE RUBIN, *Office Staff*
KEVIN WILIS, *Office Staff*

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Table 1: Descriptive statistics

A. Editor Information	
Number of Editors	2745
Donation Rate (%)	30
Percent Conservative (%)	22
B. Author Information	
Number of Authors	6443
Donation Rate (%)	57
Percent Conservative (%)	15
C. Sample	
Number of Articles	2852
Mean Percent Conservative Board (%)	21
At Least One Conservative Author (%)	16

Figure 2: Editors and authors with political donations over time

Figure 3: Distribution of author and editor ideology

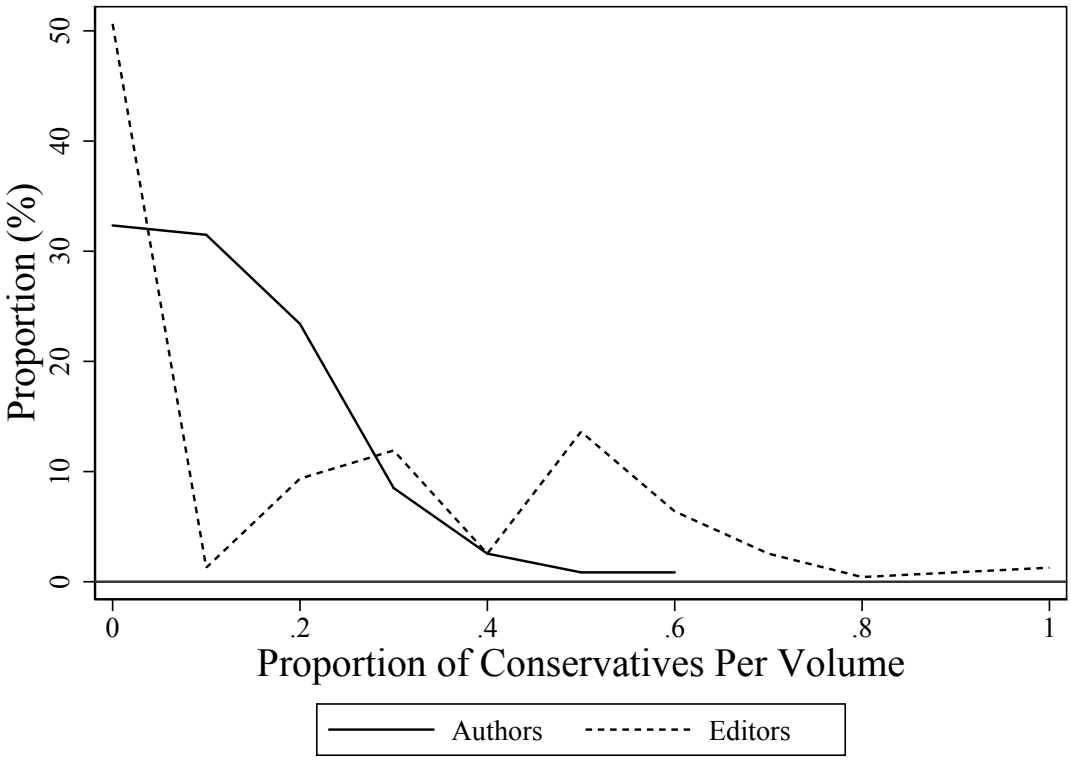

Figure 4: Relationship between author and editor political ideology

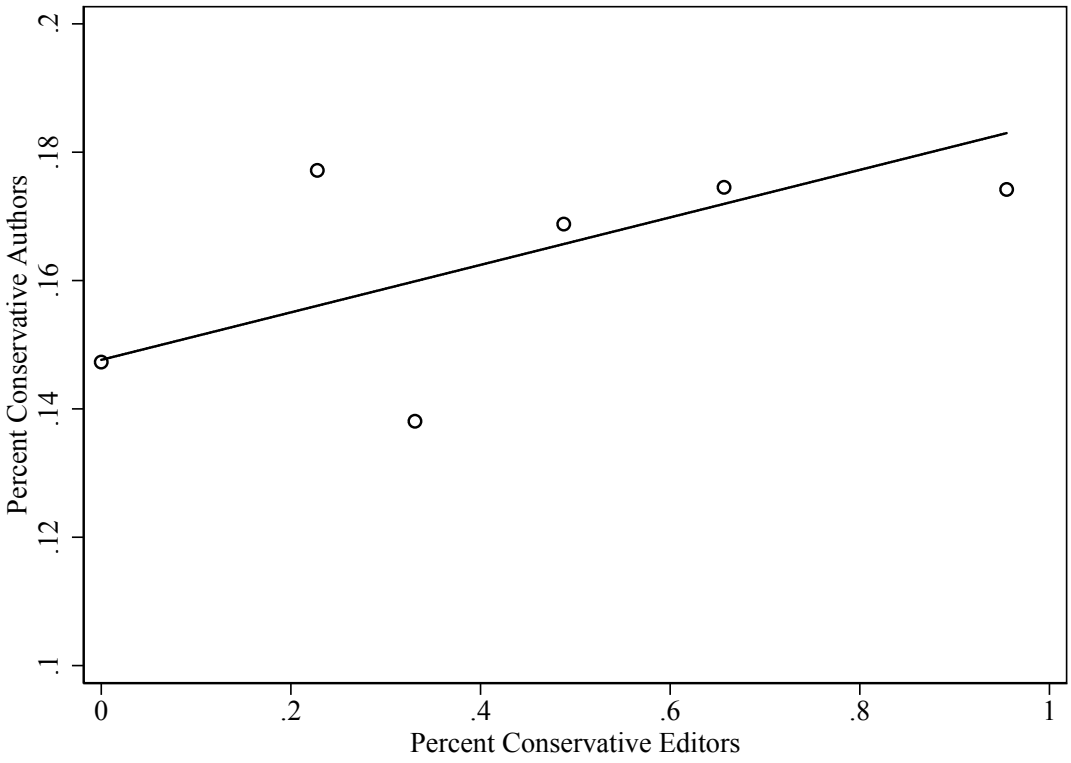

Table 2: Influence of editor ideology on author ideology

	Conservative Author			
	(1)	(2)	(3)	(4)
Percent Conservative Editors	0.048**	0.063***	0.054**	0.060**
	(0.023)	(0.024)	(0.027)	(0.028)
Covariates				
Year FE	No	Yes	Yes	Yes
Journal FE	No	No	Yes	Yes
Journal Time Trends	No	No	No	Yes
N	2,852	2,852	2,852	2,852
Dep Var Mean	0.156	0.156	0.156	0.156

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Relationship between editor-author ideology and citations

	ln(Citations)			
	(1)	(2)	(3)	(4)
	Liberal Authors	Conservative Authors	All Authors	All Authors
Percent Conservative Editors	-0.156***	0.212	-0.109***	-0.149***
	(0.035)	(0.142)	(0.029)	(0.043)
Percent Conservative Editors × Conservative Author				0.299***
				(0.081)
Covariates				
Year FE	Yes	Yes	Yes	Yes
Journal FE	Yes	Yes	Yes	Yes
Journal Time Trends	Yes	Yes	Yes	Yes
N	1,911	445	2,356	2,356
Dep Var Mean	4.221	4.063	4.235	4.191

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Regressions are weighted by the number of articles. Main effect on "At Least one Conservative Author" included in Column 4 but not reported.

Table 4: Alternative definitions for ideology

	(1)	(2)	(3)	(4)
A. Conservative Author & Average Editor CFscore				
Average Editor CFscore	0.021**	0.027**	0.019	0.023*
	(0.010)	(0.011)	(0.013)	(0.013)
Dep Var Mean	0.156	0.156	0.156	0.156
B. Author CFscore & Percent Conservative Editors				
Percent Conservative Editors	0.104**	0.136**	0.093	0.099
	(0.052)	(0.055)	(0.062)	(0.063)
Dep Var Mean	-0.889	-0.889	-0.889	-0.889
C. Author CFscore & Average Editor CFscore				
Average Editor CFscore	0.050**	0.062**	0.031	0.037
	(0.023)	(0.025)	(0.028)	(0.029)
Dep Var Mean	-0.889	-0.889	-0.889	-0.889
Covariates				
Year FE	No	Yes	Yes	Yes
Journal FE	No	No	Yes	Yes
Journal Time Trends	No	No	No	Yes
N	2,852	2,852	2,852	2,852

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Alternative ways of handling co-authored articles

	Conservative Author			
	(1)	(2)	(3)	(4)
A. Unit of Observation is Author				
Percent Conservative Editors	0.048**	0.063***	0.051**	0.056**
	(0.021)	(0.022)	(0.025)	(0.026)
N	3,118	3,118	3,118	3,118
Dep Var Mean	0.146	0.146	0.146	0.146
B. Article Conservative if no Liberal Authors				
Percent Conservative Editors	0.047**	0.059**	0.046*	0.053**
	(0.022)	(0.023)	(0.026)	(0.026)
N	2,852	2,852	2,852	2,852
Dep Var Mean	0.135	0.135	0.135	0.135
Covariates				
Year FE	No	Yes	Yes	Yes
Journal FE	No	No	Yes	Yes
Journal Time Trends	No	No	No	Yes

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 6: Missing editor ideologies

	Conservative Author			
	(1)	(2)	(3)	(4)
A. Boards with at least 3 Donating Editors				
Percent Conservative Editors	0.074**	0.095**	0.100**	0.127**
	(0.036)	(0.039)	(0.046)	(0.049)
N	1,463	1,463	1,463	1,463
Dep Var Mean	0.148	0.148	0.148	0.148
B. Restrict Sample to 1990 to 2000				
Percent Conservative Editors	0.052	0.078**	0.068*	0.086**
	(0.033)	(0.035)	(0.039)	(0.041)
N	1,653	1,653	1,653	1,653
Dep Var Mean	0.150	0.150	0.150	0.150
C. Fill in Missing Editors with Law School Cohort				
Percent Conservative Editors	0.051	0.063	0.036	0.052
	(0.047)	(0.048)	(0.068)	(0.071)
N	3,488	3,488	3,488	3,488
Dep Var Mean	0.154	0.154	0.154	0.154
Covariates				
Year FE	No	Yes	Yes	Yes
Journal FE	No	No	Yes	Yes
Journal Time Trends	No	No	No	Yes

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.