

Do Law Clerks Influence Voting on the Supreme Court?

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We study the influence of law clerks on judicial voting by Supreme Court justices. We exploit the timing of the clerkship hiring process to link variation in clerk ideology to variation in whether a justice casts a liberal or conservative vote. To measure clerk ideology, we match clerks to their political donations disclosed to the Federal Election Commission. We document a positive and statistically significant effect of clerk ideology on judicial voting. Our results suggest that moving from moderate liberal clerks (with ideology in the 25th percentile of U.S. donors) to moderate conservative clerks (with ideology in the 75th percentile of U.S. donors) would increase a justice's conservative voting rate by about 5% each term. We find larger effects in cases that are higher profile, cases that are legally significant, and cases in which the justices are more evenly divided. We interpret our results as providing suggestive evidence that clerk influence operates through persuasion rather than delegation of decision-making responsibility.

Keywords: Judicial Behavior, Political Ideologies, Law Clerks, Judges

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Introduction

The specter of the law clerk as a legal Rasputin, exerting an important influence on the cases actually decided by the Court, may be discarded at once. ... It is unreasonable to suppose that a lawyer in middle age or older, of sufficient eminence in some walk of life to be appointed as one of nine judges of the world's most powerful court, would consciously abandon his own views as to what is right and what is wrong in the law because a stripling clerk just graduated from law school tells him to.

– William Rehnquist

The law is not made by judge alone, but by judge and company.

– Jeremy Bentham

The influence of law clerks on judicial decision-making at the U.S. Supreme Court has been a perennial topic of interest among both academics and popular commentators on the Court. The inner workings of Supreme Court justices' chambers are notoriously secretive, but interviews and writings by former clerks suggest that clerks play an important role in the judicial opinion writing process—researching the facts of the case and the relevant legal precedent, serving as emissaries to other chambers, and even writing drafts of the opinions themselves. What remains hotly debated, however, is whether clerks, who are often only a few years out of law school, influence the actual votes cast by Supreme Court justices on the cases they hear.

Understanding the influence of clerks at the Supreme Court is crucial for understanding the development of law and the political economy of the American judiciary, but the secretive nature of the institution makes the topic difficult to study empirically. Interviews with former clerks certainly suggest that clerks exert a significant degree of influence over their justices in specific cases ([Woodward and Armstrong, 2005](#); [Ward and Weiden, 2006](#)). However, this view may be colored by clerks' exaggerated sense of their own importance in the process ([Toobin, 2007](#); [Kozinski and Bernstein, 1998](#)) or may represent aberrations from the norm.

In addition, two problems have significantly stymied efforts to assess law clerks influence on judicial decision making. The first is identification. In particular, identifying the causal effect of clerks on justices' voting behavior is challenging because justices may choose to hire clerks who share their preferences over case outcomes, making it difficult to distinguish the effect of a clerk from the effect of the justice's own ideology. The second is data. In particular, little information about Supreme Court clerks' preferences over case outcomes has been systematically collected, making it difficult to assess whether clerks have exerted influence how justices vote.

To deal with the fact that the assignment of clerks to justices is not random and that justices may seek out clerks who share their ideological preferences, we exploit the timing of the Supreme Court clerk hiring process. In particular, justices tend to decide on future clerks well before the date that the clerkship begins, often early in the prior term or even during the term two years before the actual clerkship begins. In conjunction with the assumption that evolutions in judicial ideology are characterized by a random-walk process (with or without drift), this feature of the institution allows us to plausibly isolate the causal effect of clerk ideology on judicial voting behavior. Intuitively, changes in clerk ideology between two terms reflects changes in the justice's ideology during the *prior* two terms; under the random walk assumption, these will be uncorrelated with subsequent evolutions in the justice's ideology.

To overcome the lack of data on Supreme Court clerk preferences over case outcomes, we construct a novel measure of Supreme Court clerk political ideology. In particular, we match the universe of Supreme Court clerks to the universe of political donations disclosed to the Federal Election Commission and arranged on a unidimensional ideological scale by [Bonica \(2014\)](#). Although only about 5% of the general public have made political donations, over 70% of the Supreme Court clerks in our sample have done so. For those clerks who donated, these data provide a measure of ideological

preferences as revealed by their actual donation decisions.

The results of our analysis suggest an important and statistically significant effect of clerk ideology on judicial voting behavior. Under our preferred specification, a standard deviation change in clerk ideology is associated with a 1 percentage point change in a justice’s conservative vote share. Put differently, replacing a chambers made up of moderate liberal clerks (with ideology in the 25th percentile of U.S. donors) with moderate conservative clerks (with ideology in the 75th percentile) would increase a justice’s conservative votes by about 5% each term. We document substantially larger effects of clerk ideology in high-profile cases, cases that are perceived as legally significant, and in cases where the justices are more closely divided as to the correct result. We interpret these findings to provide suggestive evidence that clerk influence operates through clerks persuading their justice to follow the clerk’s preferred outcome, rather than through justices delegating decision-making to clerks.

Our results contribute to important literatures in law, economics, and political science. For example, an established literature in economics seeks to understand the sources of judicial decision-making ([Maskin and Tirole, 2004](#); [Gennaioli and Shleifer, 2007](#); [Baker and Mezzetti, 2012](#); [Lim, 2013](#); [Lim et al., 2015](#); [Yang, 2015](#)), with some of this work focused on the Supreme Court in particular (e.g., [Iaryczower and Shum, 2012](#)). Similarly extensive literatures address these questions within the fields of political science and law (for an overview, see [Epstein et al., 2013](#)).

Several recent papers have studied the role of clerks on the Supreme Court. [Peppers and Zorn \(2008\)](#) studied the effect of clerk ideology on Supreme Court voting by surveying former clerks on whether they identified as Democrats or Republicans. Similarly, [Kromphardt \(2015\)](#) proxies for clerk ideology using the ideology of the judge for whom the clerk previously worked. Although these papers report evidence for clerk influence, neither does so on the basis of an empirical strategy that is robust to changes

in judicial ideology over time. As discussed below, specifications with this limitation run the risk of conflating clerk influence with secular changes in a justice’s voting preferences over time—of the type that are commonly understood to occur (Epstein et al., 2007). We build on these results by developing and constructing a better measure of clerk ideology and by utilizing an identification strategy that is robust to changes in judicial ideology over time.¹

The remainder of the paper proceeds as follows. Section 1 provides general background about Supreme Court clerkships and describes potential mechanisms by which clerks may influence judicial voting behavior. Section 2 describes our empirical strategy. Section 3 describes our data and provides descriptive statistics. Section 4 presents our baseline results as well as additional analyses. Section 5 conducts robustness checks relating to our identifying assumption, sample selection concerns, and the stability of clerk ideology over time. Section 6 concludes.

1 Background and Mechanisms for Clerk Influence

Supreme Court clerkships are prestigious, one-year positions in which a clerk works directly for an individual Supreme Court justice.² Although justices are free to hire whomever they wish, the justices typically select recent top graduates of America’s most elite law schools. At least since the 1960s, these recent graduates have typically

¹In addition to the studies described in this paragraph, a number of recent papers have used text analysis to study the role of clerks in the writing of Supreme Court opinions by assessing changes in writing style between terms (Wahlbeck et al., 2002; Choi and Gulati, 2005; Rosenthal and Yoon, 2011; Li et al., 2013; Carlson et al., 2016).

² A great deal has been written about the role of judicial law clerks at the Supreme Court. See Peppers (2006) for an overview. Although the workings of Supreme Court chambers are largely confidential, there are at least four ways that information has been made public. First, some academics and journalists have been leaked information that has been used to write behind the scenes accounts of Supreme Court decision-making (see, e.g., Woodward and Armstrong, 2005). Second, the papers of some deceased justices—most notably Justice Blackmun—have been later publicly released (see, e.g., Greenhouse, 2006). Third, a handful of clerks have written books or articles that recount their experiences working at the Supreme Court (see, e.g., Lazarus, 2005). Finally, some justices have publicly spoken or written about the inner workings of the court.

served, or will serve, as a clerk for federal district or, more typically, federal appeals court judge (Hutchinson, 1998; Rozema and Peng, 2015). Each term, the chief justice can hire up to five clerks per term and associate justices can hire up to four clerks per term (Peppers, 2012). In part to attract the best talent, the justices normally hire clerks well before their clerkship begins, typically at the beginning of the prior judicial term or earlier.

The specific responsibilities of the clerks varies by justice and have evolved over time (Newland, 1961; Baier, 1973; Peppers, 2006). The standard tasks that are assigned to clerks include reviewing petitions for certiorari (i.e., which cases to hear), writing memos that assess the merits of pending cases, helping the justices prepare for oral arguments, and assisting in the drafting and editing of opinions. The clerks work in close quarters with the justices and have frequent professional and social interactions with the justices, ranging from formal meetings before oral arguments to lunches and after-work happy hours.

As we noted before, a running debate concerns whether clerks influence the voting of Supreme Court justices.³ There are at least two pathways for how such influence could occur: delegation and persuasion.⁴ First, justices may delegate a number of responsibilities to clerks, such as reading the briefs submitted by the parties,

³This debate has largely been motivated by prominent anecdotes and qualitative evidence. For example, there are a number of widely known stories about clerks having swayed justices' votes while clerking. See, e.g., Tribe (1991) discussing his role in swaying the outcome of *Katz v. United States*, 389 U.S. 347 (1967), Garrow (2014) discussing the role of clerks in *Roe v. Wade*, 410 U.S. 113 (1973), and Ward and Weiden (2006) discussing the influence of clerks on the outcome of *Planned Parenthood v. Casey*, 505 U.S. 833 (1992). Additionally, the behavior of justices can also be seen as evidence that they believe that clerks may influence their decisions. For instance, the fact that Justice Kennedy has at times empowered a group of high profile conservative lawyers and judges to pre-screen clerks can be interpreted as concern that liberal clerks may sway decisions, and the fact that Justice Scalia often intentionally hired liberal "counter-clerks" may have reflected his belief that their ideological perspective was valuable.

⁴There are other pathways of influence that we do not consider here. For example, one possibility is that justices learn about the personal lives of their clerks and are influenced via empathy (Washington, 2008). This pathway, which might operate if a justice hires female or LGBT law clerks, could serve to influence the justices' votes on cases having a gender or LGBT dimension.

reading amicus briefs and lower court opinions, and conducting legal research about related cases. Justices who delegate such responsibilities may have difficulty monitoring whether clerks' presentation of those facts and arguments are distorted by political bias. For example, a clerk may—consciously or unconsciously—emphasize the facts and arguments that best support his or her preferred outcome. It is through this kind of delegation that a number of accounts of prior clerks, including [Rehnquist \(1957\)](#) and [Lazarus \(2005\)](#), have argued that clerks are able to have influence. These delegation stories of influence would suggest that clerk influence would be highest for justices who delegate more frequently, and in relatively minor or uninteresting cases where the justice was least engaged.

Second, clerks may influence Supreme Court voting by persuading justices to vote in the direction preferred by the clerk. Clerks tend to have near-constant access to the justice for whom they work, and clerks may use that access to lobby the justice to vote in their preferred direction. Along these lines, anecdotal evidence suggests that clerks can influence a justice's vote in the cases in which the justices feels to be a close call. For example, [Garrow \(2014\)](#) and [Ward and Weiden \(2006\)](#) provide detailed accounts of how clerks were able to influence justices when two important abortion rights cases—*Roe v. Wade* and *Planned Parenthood v. Casey*—were being decided. These persuasion stories of influence would suggest that clerk influence would be highest in cases in which the justices were less confident over which outcome was correct, and in cases where the clerks were the most engaged.

It is important to note that the delegation and persuasion pathways of how clerks exert influence are not mutually exclusive, nor are they the only possible pathways of clerk influence. It could be the case that the clerks both covertly shape the information that is presented to justices in ways that changes voting and also overtly persuade the justices to vote in a particular direction. Moreover, without detailed

information on the inner workings of the justices’ chambers—which is largely kept confidential—it is difficult to empirically test the relative importance of these two mechanisms. Nonetheless, in addition to testing whether clerk ideology influences voting, we also provide suggestive evidence along these lines below.

2 Empirical Framework

We model the conservativeness of a justice’s voting in a particular term ($y_{j,t}$) as a function of the justice’s ideology during that term ($d_{j,t}$) and the ideology of the clerks hired by the justice during that term ($c_{j,t}$) according to (1):

$$y_{j,t} = \beta c_{j,t} + d_{j,t} \tag{1}$$

for justice j in term t . Our goal is to estimate β , which captures the effect of clerk ideology on a justice’s voting. The challenge for identification is that $d_{j,t}$ is unobserved. In addition, $d_{j,t}$ is likely to be correlated with $c_{j,t}$ because justices may choose to hire ideologically aligned clerks or because clerks seek to work for justices whose ideologies they share.

As a benchmark, suppose that judicial ideology is time-invariant, $d_{j,t} = d_j$. In this case, one can obtain an unbiased estimate for β by estimating (1) in a model with justice fixed effects. In practice, of course, a justice’s ideology may evolve over time (Epstein et al., 2007), and if it does, this evolution can result in a biased estimate for β . For example, a justice that grows increasingly liberal over time may hire more liberal clerks at the end of her career than at the beginning, and also cast an increasing number of liberal votes over this period. Controlling for secular time trends may alleviate or remove this bias, but it is also possible that shifts in a justice’s ideology could occur

discretely, in ways that a parametric time trend does not capture.

To allow for idiosyncratic changes in judicial ideology, we model $d_{j,t}$ as a random walk:⁵

$$d_{j,t} = d_{j,t-1} + \eta_{j,t} \tag{2}$$

in which innovations in a justice’s ideology ($\eta_{j,t}$) are assumed to be uncorrelated across justices and over time:

$$\text{cov}(d_{j,t}, d_{j',t'}) = 0 \quad \forall j, j', t, t' \tag{3}$$

Even when modeling changes in judicial ideology as a random walk, a central concern with identifying the effect of clerk ideology on judicial outcomes is still that the assignment of clerks to justices is non-random—a justice chooses which clerks to employ in a given term. As a result, justices are likely to employ clerks that share their ideology (Bonica et al., 2016b). Consequently, one might worry that an observed correlation between judicial outcomes in a term and clerk ideology in that term merely reflects an omitted third variable (judicial ideology) rather than a causal relationship. Because of this concern, our empirical strategy relies on a useful institutional feature of the Supreme Court clerkship hiring process during our sample period: namely, that Supreme Court justices typically hire their law clerks one to two terms before the clerkship begins. For example, a clerk hired to work for a justice from June 2015 to June 2016 would likely be hired sometime in mid to late 2013 or 2014.⁶

It is this feature, along with our random walk assumption, that permits identification of clerk influence given the non-random assignment of clerks to justices. In particular, the random walk assumption implies that at the date a clerk is hired, justices are unable to predict how their own ideology will evolve between the date of

⁵This is similar to the assumption made in other papers on judicial ideology, e.g., Martin and Quinn (2002), who model the justices’ ideologies with a random walk prior.

⁶We discuss the robustness of our results to more conservative assumptions about the length of the lag between clerk hire and employment in Section 5.

clerk hire and the date the clerk begins work. For example, a justice who hires a law clerk in January of 2017 to begin employment (and thus start assisting on cases before the Court) in June of 2018 will do so without knowing how his or her own ideology will change during the intervening months. Because of this time delay between hiring decisions and employment, it is plausible to assume that a clerk hired to begin work during term t reflects the justice's ideology during term $t - 1$ (or term $t - 2$, depending on how long ago the clerk was hired), rather than the justice's ideology during term t .⁷

As such, changes in clerk ideology between terms $t - 1$ and t will actually reflect changes in judicial ideology between terms $t - 2$ and $t - 1$ (or between $t - 3$ and $t - 2$, again depending on how far in advance the justice hires). Hence, under these assumptions, changes in clerk ideology between two terms will not be conflated with unobserved changes in judicial ideology during the same period of time.

More formally, suppose that the ideology of the clerks selected to clerk in term t depend on the justice's ideology in $t - 1$ and an idiosyncratic error term, $\nu_{j,t}$, reflecting factors uncorrelated with judicial ideology.⁸

$$c_{j,t} = \alpha d_{j,t-1} + \nu_{j,t} \tag{4}$$

Taken together, these assumptions permit the unbiased identification of β in (1). Taking first differences of the variables in (1) between consecutive terms, and applying (2), yields:

⁷One case in which this assumption would be violated is if clerks tend to closely monitor the ideology of the hiring justice after accepting an offer for future employment, adapting his or her own ideology in response. Another possibility is that an exogenous shock could simultaneously affect the justice and the clerk ideology (for example, a significant political shock, such as an economic recession or terrorist attack). However, to the extent such events affect other justices as well, they will be picked up in the term effects included in the specifications below.

⁸Note that the analysis is similar for term t clerks hired in $t - 2$. In that case, one could write $c_{j,t} = \alpha d_{i,t-2} + \nu_{j,t}$, and the result would make use of the fact that $cov(\eta_{i,t-2}, \eta_{j,t}) = 0$.

$$\Delta y_{j,t} = \beta \Delta c_{j,t} + \eta_{j,t} \tag{5}$$

Note that estimation of (5) yields an unbiased estimate for β if $cov(\Delta c_{j,t}, \eta_{j,t}) = 0$. From (2) and (4), we have that $\Delta c_{j,t} = \alpha \eta_{j,t-1} + \Delta \nu_{j,t}$. Because judicial ideology is assumed to be a random walk, we know that $cov(\eta_{j,t-1}, \eta_{j,t}) = 0$. In addition, recall that ν represents factors uncorrelated with ideology, so it is reasonable to assume that $cov(\Delta \nu_{j,t}, \eta_{j,t}) = 0$. Hence, $cov(\Delta c_{j,t}, \eta_{j,t}) = 0$, and as a result, estimation of (5) yields an unbiased estimate of β .

Below, we implement this estimation strategy by regressing changes in a justice’s voting behavior between consecutive terms on changes in the ideology of the clerks employed by that justice over the same time period. We also implement justice fixed effects and (linear) time trends to additionally account for both justice specific effects and also for the possibility of (linear) intellectual drift (Epstein et al., 2007).

3 Data

Our empirical analysis uses data on Supreme Court justice voting and the ideology of Supreme Court clerks.

3.1 Sources of Data

Supreme Court Vote Data Our outcome data is based on individual Supreme Court justice votes on cases decided between 1960 and 2009, obtained from the Supreme Court Database (Spaeth et al., 2015). Spaeth et al. hand code each vote as liberal, conservative, or “unspecifiable.” This measure is among the most widely used in empirical analyses of judicial behavior (Epstein et al., 2013, 2005). In the present application,

we restrict our focus to cases coded as either liberal or conservative.⁹

Clerk Ideology Data To obtain data on clerk ideology, we matched the universe of Supreme Court clerks (obtained from the Supreme Court Information Office) to the universe of political donations disclosed by the Federal Election Commission and agencies from state, local, and federal elections since 1979.¹⁰ The donations data include approximately 100 million political donations. An individual’s political donations are arranged on a unidimensional ideological scale by [Bonica \(2014\)](#) from extremely liberal to extremely conservative. The scale is normalized so that it has a mean of 0 and a standard deviation of 1 with respect to the population of U.S. donors. The resulting campaign finance score (CFscore) reflects an individual’s ideology to the extent the individual tends to make political donations to support candidates that share his or her political beliefs.¹¹

There are two concerns with using this data for clerk ideology in our application. First, the measure is only available for clerks who donated at some point during the time span covered by the political donations data (1979-2014), and such clerks may differ in unobservable ways from the clerks that do not donate. We investigate issues of sample selection in Section [5.2](#).

A second concern is that of those clerks who donate, very few do so before their clerkship. As a result, we construct our measure of clerk ideology from donations over the course of the clerk’s life. This may introduce measurement error if clerk ideology changes between the time of the clerkship and the time that subsequent donations are made. We investigate this concern in Section [5.3](#).

⁹This excludes 4,848 votes coded as “unspecifiable.”

¹⁰We report details of the matching process in the Technical Appendix.

¹¹[Bonica \(2014\)](#) and [Bonica and Sen \(2016\)](#) provide evidence about the internal and external validity of using political donations as a measure of ideology.

3.2 Descriptive Statistics

Table 1 presents descriptive statistics for our sample. Column 1 presents information on the distribution of conservative votes cast. Our sample includes 66,209 votes, of which 45.8 percent are coded as conservative. The standard deviation of votes cast captures both within- and between- variation in the voting behavior of individual justices. In contrast, the mean within-justice standard deviation captures the average degree of variation in the direction of votes cast by a single justice—for instance, this number would be zero if each conservative justice always cast conservative votes and if each liberal justice always cast liberal votes.

[Insert Table 1 about here]

Column 2 of Table 1 presents justice voting behavior at the justice-term level of analysis. We observe 439 justice-term combinations in our sample. The conservative vote shares in these terms range from 5.5% (Byron White in 1961) at the liberal end of the spectrum to 77.9% (Clarence Thomas in 2003) at the conservative end of the spectrum. There is less variation in how an individual justice votes between terms than there is between how different justices vote.

Columns 3 and 4 of Table 1 present information on the distribution of clerk CFscores, overall and by justice-term. The total number of individuals clerking for the Supreme Court during our sample period is 1,599, 72.2% of whom we match to an individual record in the political donations data. The donation rate among Supreme Court clerks is quite high compared to other groups; for comparison, the mean donation rate among all Americans is 5% and among all lawyers is 40% (Bonica et al., 2015). As shown in Appendix Figure A1, most justice-terms are missing 0 to 1 clerks.¹²

¹²Section A2.1 of the Supplementary Appendix presents detailed information about the match rate by justice-term.

Figure 1 presents the mean conservative vote share among Supreme Court justices over time.¹³ The figure is consistent with the common understanding that the Court was more conservative during the 1970s and 1980s (during the Burger Court) compared to the 1960s (Warren Court). Notably, in addition to this broad trend, the figure shows substantial year-to-year fluctuations in the mean share of conservative votes.

[Insert Figure 1 about here]

Figure 2 presents information on the cases per term and clerks hired per term during our sample period. The number of cases decided by the Supreme Court each term has decreased substantially over time, from an average of 180 cases per term in the 1960's to 92 cases per term in the 2000's. Over the same time period, the average number of clerks employed by each justice has risen, from an average of 19 clerks per term during the 1960s to 38 clerks per term during the 2000s.

[Insert Figure 2 about here]

Finally, Figure 3 presents the ideological distribution of the Supreme Court clerks in our sample.¹⁴ The mean CFscore is -0.50. To put this in perspective, this is comparable to Bill Clinton's CFscore of -0.68. The figure is bimodal, and suggests that the political alignment of Supreme Court clerks is consistent with the broader two-party split in American politics.

[Insert Figure 3 about here]

¹³Section A2.2 of the Supplementary Appendix presents information about voting by justice.

¹⁴Section A2.3 of the Supplementary Appendix presents the ideological distribution of clerks by justice and the correlation between clerk and justice ideology. For additional analysis and discussion of this ideological distribution (as well as information on the ideological distribution of lower court clerks), see Bonica et al. (2016a).

4 Results

4.1 Primary Results

In this section, we implement the empirical model described in section 2. We first consider a specification with justice fixed effects:

$$y_{cjt} = \alpha + \beta \text{Clerk}_{j,t} + \mu_j + \delta_t + \zeta X_c + \varepsilon_{cjt} \quad (6)$$

In (6), y indicates a conservative vote by justice j in term t on case c , $\text{Clerk}_{j,t}$ denotes the mean CFscore for the clerks employed by justice j in term t , μ_j denotes justice fixed effects, δ_t denotes term fixed effects, X_c denotes a vector of case-level characteristics (which we implement with issue-area fixed effects), and ε_{cjt} denotes the error term.¹⁵ Standard errors are clustered at both the justice and case levels, following the two-way clustering procedure described in Cameron et al. (2011) and Cameron and Miller (2015). As discussed in Section 2, estimating (6) yields an unbiased estimate of β if within-justice variation in ideology is uncorrelated with variation in the ideology of the clerks hired by the justice.

[Insert Table 2 about here]

The results of this analysis are presented in Table 2, Column 1. The estimated coefficient on clerk ideology is positive and statistically significant. The point estimate, 0.017, is the percentage point change in the conservative vote share associated with a one standard deviation shift in the mean political ideology of the clerks hired by a justice in a term—a 3.7 percent change relative to the mean conservative vote share of 0.46.

¹⁵We consider alternative specifications using median, minimum, and maximum clerk ideology per term in Supplemental Table A4. Note that these specifications are robust to a hiring model in which justices seek to hire “counter-clerks” with ideologies opposite to their own.

Column 2 of Table 2 adds justice-specific linear time trends. As discussed in Section 2, the inclusion of such a trend may alleviate the bias associated with changes in judicial ideology over time. Including justice-specific time trends slightly attenuates the estimated coefficient on clerk ideology, but the effect remains statistically significant.

We next turn to the first-differences model, which exploits the timing of the Supreme Clerk hiring process to disentangle changes in judicial ideology from changes in clerk hiring. Figure 4 plots changes in a justice’s conservative vote share between consecutive terms against changes in the ideology of the clerks hired by the justice in those terms. The figure suggests a moderate but positive-sloping relationship, with larger increases in the degree of clerk conservativeness associated with larger increases in the justice’s conservative vote share.

[Insert Figure 4 about here]

Column 3 of Table 2 confirms this graphical evidence. The econometric specification is given by:

$$\Delta y_{j,t} = \alpha' + \beta \Delta \text{Clerk}_{j,t} + \delta'_t + \varepsilon'_{jt} \quad (7)$$

where $\Delta y_{j,t} = y_{j,t} - y_{j,t-1}$ and $\Delta \text{Clerk}_{j,t} = \text{Clerk}_{j,t} - \text{Clerk}_{j,t-1}$. Note that (7) is estimated at the justice-term, rather than the case-justice-term, level. Estimating (7) yields a point estimate of 0.008, slightly smaller than the estimated effect reported in Column 2.

Finally, Column 4 of Table 2 incorporates a justice-specific linear time trend, which we implement by adding a justice fixed effect to the first-differences model in (7). The inclusion of a linear time trend is appropriate in the first-differences model if judicial ideology is best characterized by a random walk with a justice-specific drift

term. As shown in Column 4, the point estimate increases slightly following this addition, to approximately 0.010.

The results in Table 2 suggest the presence of a non-trivial effect of clerk ideology on judicial voting behavior. Focusing on Column 4, which we will use as our baseline specification for the analyses that follow, the coefficient indicates that replacing the clerks employed by a justice in a term with clerks that are one standard deviation more conservative increases the conservative vote share of that justice by approximately 1 percentage point. Put differently, replacing a judicial chambers composed of moderate liberal clerks (with CFscores in the 25th percentile of U.S. donors) with a judicial chambers composed of moderate conservative clerks (with CFscores in the 75th percentile of U.S. donors) would result in a 4.5 percent increase in the number of conservative votes by the justice during the term.¹⁶

4.2 Additional Results and Possible Mechanisms

As discussed in Section 1, observing differences in clerk influence across case types may offer clues as to the mechanisms by which clerks influence justice voting. In particular, two possible channels through which clerks may influence justice voting are because (1) the justice effectively delegates decision-making on the case to the clerk, or (2) the clerk persuades the justice to vote for the clerk’s preferred outcome. A delegation channel suggests clerk influence should be greatest in cases where justices are least engaged (and most willing to delegate) and among justices who assign the most opinion-writing responsibility to clerks; by contrast, a persuasion channel would perhaps be the most pronounced in the most important cases, where interest among the clerks might be the highest and where they might derive the highest utility from

¹⁶Among U.S. donors, the 25th and 75th CFscore percentiles are given by -1.18 and 1.02, respectively. We compute this effect by multiplying the difference in these CFscores (2.20) by the point estimate (.0096), and scaling by the mean fraction of conservative votes (0.468).

the justices voting in line with their preferences.

Table 3 explores these possibilities by comparing clerk influence across various types of cases.¹⁷ Column 1 compares clerk influence in cases that are high profile to those that are not, motivated by the fact that justices would be less likely to delegate to clerks in cases that are relatively high profile. Following Epstein and Segal (2000), we proxy for whether a case is high profile by whether it appears on the front page of the *New York Times*.

In this analysis, there are two observations for each justice-term: one for the high profile cases, and one for the cases that are not high profile. The empirical specification is given by:

$$\Delta y_{jtu} = \alpha' + \beta \Delta \text{Clerk}_{jt} + \gamma \pi_u \Delta \text{Clerk}_{jt} + \pi_u + \delta'_t + \varepsilon'_{jt} \quad (8)$$

where π_u indicates whether the observation is composed of high profile cases.

The results in Column 1 are precisely the opposite of what a delegation theory of clerk influence would predict. The main effect on clerk influence is close to zero and statistically insignificant. In contrast, the interaction term is large and statistically significant, suggesting that clerk influence is greatest in cases that are high profile. Although inconsistent with the delegation theory of clerk influence, this finding is consistent with the persuasion model—it could be that clerks are most motivated to influence their justice in cases that are high profile.

Alternatively, a justice’s interest in a case—and willingness to delegate—may depend not on whether the case is high profile, but on whether it is legally significant. Column 2 repeats the analysis in Column 1, focusing on whether a case is classified as

¹⁷Table A3 in the Supplemental Appendix presents summary statistics on the subgroups of cases analyzed in this section.

“major” by *Congressional Quarterly* in terms of its legal significance.¹⁸ The results in Column 2 provide strong evidence that clerk influence is strongest in legally significant cases, and virtually non-existent in other cases. Like Column 1, these results are difficult to square with a delegation model of influence, but consistent with a model by which clerk influence operates through persuasion.

We next turn to the “closeness” of the case, which we measure based on how much disagreement exists among the justices as to the correct outcome. As with high profile cases and cases that are legally significant, one would expect that justices would be least willing to delegate responsibility to clerks in cases where the justices disagree with one another. Column 3 investigates how clerk influence varies between unanimous and non-unanimous cases. Here, the entire effect of clerk influence appears driven by non-unanimous cases.

Although the finding that clerk influence is driven by non-unanimous cases may provide evidence against delegation theories of clerk influence, it may also simply reflect the fact that there is a ceiling effect in unanimous cases and hence little scope for clerks to influence the outcome. Because of this, Column 4 focuses on “close” cases, which we define as cases where the outcome is decided by a vote of 5-4 or 6-3. Here too, the results suggest that clerk influence is entirely driven by close cases. Overall, we interpret the results in Column 3 as being more consistent with persuasion models of influence than with delegation models of influence.

Table 4 reports the results of our primary specification by issue area. Although the smaller number of cases increases the variability of the results, the results suggest that the largest impact of clerk ideology occurs in first amendment and civil

¹⁸While the *New York Times* measure captures the attention a case received when it was handed down, the “*CQ*” measure is based on experts’ retroactive assessment of whether a case was a landmark decision. Both measures have previously been used by scholars as proxies for important Supreme Court cases (Epstein and Segal, 2000; Fowler et al., 2007).

rights cases. In these areas, a one standard deviation increase in the conservativeness of a justice's clerks in a term results in a 3.4 and 2.5 percentage point increase in the conservative vote share on these issues. There are several possible interpretations of these results. For instance, it may be the case that first amendment and civil rights are the two issue areas that clerks are most passionate about. Alternatively, it may be the case that these are subjects about which justices look for the most help from their clerks, or where the legal doctrines are the most vague and thus provide the greatest opportunity for persuasion.

[Insert Table 4 about here]

Finally, Table 5 turns from characteristics of the case to characteristics of the justice in determining the factors that shape clerk influence. Column 1 investigates whether clerks are more influential when they work for justices who have the clerk author first drafts of their opinions. For this specification, we rely on [Peppers and Zorn \(2008\)](#)'s coding of the job duties that each justice gives their clerks. Although the outcomes of the decisions are typically decided by the point in time at which opinions are drafted,¹⁹ this variable may proxy the degree of responsibility the justice assigns to clerks generally, or, less charitably, the justice's own disengagement from the process. The results of the analysis, reported in Column 1, are consistent with the hypothesis that clerks exert more influence when working for justices who involve clerks more heavily in the opinion-writing process.²⁰ However, we do not wish to make

¹⁹It is worth noting that justices do occasionally change their mind after the opinions have been assigned to be written. For example, in one prominent recent case, it has been reported that Chief Justice John Roberts changed his vote the case deciding the constitutionality of the Affordable Care Act: *National Federation of Independent Business v. Sebelius*, 567 U.S. __ (2012) (see [Barnett, 2012](#); [Crawford, 2012](#)). In another example, future-judge Richard Posner apparently swayed the outcome of *Sanders v. U.S.*, 373 U.S. 1 (1963) while clerking for Justice Brennan by misunderstanding Brennan's instructions and producing a draft opinion that supported the opposite outcome as what the justices had agreed to in conference. However, Brennan and the other justices were sufficiently convinced by the draft to change their votes.

²⁰The estimated coefficient on the interaction term for clerk ideology and workload reported in

too much of this analysis, as all recent Supreme Court justices delegate the writing of opinion drafts to their clerks, so this effect is identified solely off of the four justices in our sample period who did not follow this practice.

[Insert Table 5 about here]

Column 2 of Table 5 investigates differences in clerk influence across liberal and conservative justices, as measured by whether the justice’s conservative vote share is below the sample median. Interestingly, the results suggest that the effect of clerk influence on judicial voting is quite similar across justices on both sides of the ideological divide.

Finally, Column 3 of Table 5 investigates the effect of clerk influence by years of experience on the bench. Because judicial experience is perfectly colinear with the justice fixed effects in this model, we do not include a main effect for experience in this analysis. The results provide no evidence that clerk influence tends to be higher or lower over the course of a Supreme Court justice’s career. This suggests to us that law clerk influence does not increase along with the justices’ age, perhaps easing the concern that older justices rely on the assistance of their clerks to a greater amount.

5 Robustness Checks

5.1 Investigating Identifying Assumptions

As discussed in Section 2, a positive correlation between clerk ideology and judicial voting may simply reflect unobserved variation in judicial ideology. Our identification strategy addresses this concern by exploiting the timing of the Supreme Court

Table 5 (corresponding to the specification in Column 4 of Table 2) is positive, but not statistically significant. The estimated coefficients (not reported) from the specifications corresponding to Columns 1 to 3 of Table 2 are similar in magnitude and statistically significant.

clerk hiring process to disentangle the effect of the clerks from unobserved changes in judicial ideology. This subsection investigates whether the assumptions underlying this approach are likely to hold in our data.

Timing of Influence Our first check compares the timing of a change in voting behavior to the time that a given set of clerks works for a justice. If changes in clerk ideology and changes in voting behavior were both driven by changes in judicial ideology, we would expect changes in voting behavior to appear prior to the clerk’s arrival, and persist in the years following the clerks’ departure. That is, a justice who was more liberal in term t would also tend to be more liberal in the years before and after term t . In contrast, if the change in voting behavior in term t was primarily driven by the clerks employed by the justice in term t , we would expect the change in voting behavior to be largely confined to term t itself.

Figure 5 presents the association between clerk ideology and judicial voting in each term during the five terms before and after the clerks are employed, using our preferred specification (Column 4 in Table 2). The results suggest that the association between clerk ideology and judicial voting is largely confined to the term in which the clerks are employed; the estimated coefficient for every other term is smaller in magnitude and statistically indistinguishable from zero. The figure thus provides evidence for the source of the observed effect on voting behavior being the clerks themselves rather than a change in judicial ideology.

[Insert Figure 5 about here]

Timing of Hiring Our second check investigates the robustness of our results to a partial failure of the hiring timing assumption underlying our identification strategy. Specifically, we have assumed that the clerks employed in term t are hired based on

the justice’s ideology in or before term $t - 1$. In practice, however, cases are decided gradually over the course of a term, and changes in judicial ideology may occur gradually over the course of a term as well. If a substantial number of term $t - 1$ cases are decided before the term t clerk is hired (which can occur when clerks are hired less than one year prior to their clerkship start date or when some term $t - 1$ cases are actually decided in term $t - 2$), changes in clerk ideology between $t - 1$ and t may be correlated with changes in judicial ideology over the same period.

Figure 6 illustrates the concern described in the prior paragraph. Panel A presents the case in which clerks for a term are hired before the prior term’s cases are decided. Any change in judicial ideology between the hiring of the clerks for term $t - 1$ and term t occurs between the times labeled C_{t-1} and C_t in the figure. In turn, any change in judicial ideology between the deciding of the cases for term $t - 1$ and term t occurs between the times labeled D_{t-1} and D_t in the figure. Because C_{t-1} to C_t occurs entirely before D_{t-1} to D_t , the two time periods do not contain any common changes in judicial ideology.²¹

[Insert Figure 6 about here]

Panel B of Figure 6 turns to the case in which the clerks for a term are hired after a portion of the prior term’s cases have already been decided. In this case, the time period between the hiring of the clerks for terms $t - 1$ and t (C_{t-1} to C_t) overlaps with the time period between the deciding of the cases for terms $t - 1$ and t (D_{t-1} to D_t). Hence, changes in judicial ideology that occur during the period of overlap may affect both of these differences, driving a spurious correlation.

To address this concern, we consider a restriction of the analysis to cases that are decided relatively late in the term, after the subsequent year’s clerks are very

²¹In addition, the assumption that judicial ideology follows a random walk guarantees that changes in judicial ideology occurring between C_{t-1} and C_t are uncorrelated with changes occurring between D_{t-1} and D_t .

likely to have been hired. Panel C of Figure 6 illustrates this restriction. When only cases in the later part of the term are considered, the time period between the hiring of the term $t - 1$ and term t clerks does not overlap with the time period between the deciding of the term $t - 1$ and term t cases. As a result, it is reasonable to assume that any correlations between the two differences will not be driven by common changes in judicial ideology.

Panel A of Table 6 implements this robustness check by replicating Table 2 while restricting the sample to cases argued in January through June, after the subsequent year’s clerk will almost always have been hired.²² Although the standard errors increase slightly, the point estimates remain similar in magnitude and for the most part remain statistically significant, suggesting that violations of our assumptions about the clerk hiring process are not driving the results.

Finally, anecdotal evidence suggests that clerk hiring has moved earlier and earlier over time, so that even if our timing assumptions do not hold for the early years of the sample, they are likely to hold by the end of the sample. Thus any bias is likely restricted to the early part of the sample. Panel B Table 6 restricts the analysis to the second half of our sample. The results provide evidence that the result is not driven by a bias in the early years.

[Insert Table 6 about here]

5.2 Accounting for Missing Data

Because our measure of clerk ideology is based on clerks’ political donations, clerks who have never made a political donation are missing from our analysis (28 percent of clerks during our sample). This subsection investigates the possibility that

²²Recall that clerks are typically hired to work for a single year term, beginning and ending in the summer months between terms.

this sample selection biases our estimates. In general, missing clerk ideology data may bias our results by introducing measurement error into our measure of the mean ideology of the clerks employed by a justice during a given term. This measurement error may be classical, in which case it is likely to attenuate our estimated coefficients, or it may be correlated with clerk ideology, potentially resulting in other forms of bias. This section investigates the effect of the missing data under three possible assumptions.

Ideology Missing at Random As a baseline, it may be the case that clerks' propensity to donate is uncorrelated with their ideology, meaning that the missing clerks are drawn from the same ideological distribution as the clerks we observe. In this case, the missing data causes us to mis-measure clerk ideology, but because the data are effectively missing completely at random, the measurement error will be classical, biasing our estimated coefficient towards zero.

To provide a sense for the magnitude of the attenuation bias, we conduct a simulation exercise in which we back out the true treatment effect size that would generate our observed results, given the degree of missing data we observe and the assumption that the data is missing at random. Specifically, we implement the following algorithm: (1) select a hypothetical treatment effect; (2) fill in the missing clerk-level data by drawing at random without replacement from the distribution of observed clerks hired by the same justice as the missing clerk; (3) generate predicted voting data based on the hypothetical treatment effect from Step 1 and the new clerk data from Step 2; (4) randomly drop clerk-level observations so that the degree of missing data is the same as in the true data; (5) estimate our baseline first-differences specification (Column 4 of Table 2). Intuitively, this process generates data assuming a particular treatment effect, and then generates measurement error of the form we observe to estimate the likely degree of attenuation bias. We repeat this process 1,000 times for

a given assumed treatment effect and take the mean estimated coefficient from Step 5. If this mean treatment effect matches our observed treatment effect, it suggests that the hypothetical treatment effect chosen in Step 1 may characterize the true data generating process for our sample. In contrast, if the mean treatment effect that emerges from this process is too large or too small, we adjust the hypothetical treatment effect in Step 1 and begin the process again. This algorithm converges on an estimated effect size of 0.0202, consistent with the theory that the measurement error from the missing clerk data causes attenuation bias.

Ideology Missing at Random Conditional on Observables The second possibility we consider is that the ideology of the clerks who do not donate may differ systematically from the clerks that do, but that other information can be leveraged to predict the ideology of clerks that do not donate. Although we do not observe donations for the missing clerks, we do observe other information about them that can be used to predict their ideology, such as their gender and which law school they attended. Following [Bonica et al. \(2016b\)](#), we impute the ideology of the missing clerks based on their observable characteristics. The details of the imputation procedure are reported in the Technical Appendix. The results of the analysis using the imputed data are presented in Column 1 of Table 7. The point estimate and standard error are largely unchanged.

[Insert Table 7 about here]

Next, we proxy the ideology of the unobserved clerks using information about the ideology of the prior judge who employed the clerk prior to the Supreme Court justice.²³ For this analysis, we use the Judicial Common Space ideology measure of the

²³[Kromphardt \(2015\)](#) employs a similar approach to measure clerk ideology.

prior judge.²⁴ The results are presented in Column 2 of Table 7. As above, the results are largely unchanged by the addition of this additional clerk data, but the estimated coefficient increases relative to our baseline specification.

Ideology Missingness Reflect Preference Intensity The third possibility we consider is that there may be systematic ideological differences between donating and non-donating clerks, and that these differences are not fully captured by the characteristics of the clerks we observe or by the ideology of the judge for whom they previously clerked. One likely possibility along these lines is that the clerks who donate, and are thus in our data, hold more intense ideological preferences than those who do not donate. This would be, for example, the difference between a clerk who “leans Republican” versus one who is “strongly Republican.” Variation in the intensity of ideological preferences could translate both into missingness in the data as well as affect influence of the clerk on judicial decision making.

To provide intuition as to how variation in donating behavior driven by variation in ideological intensity would affect our results, we derive a back-of-the-envelope adjustment to correct for this source of missingness. In particular, suppose that the mean ideology of the clerks who donate is ρ times greater than the ideology of the clerks who do not, $c_{jt}^U = \rho c_{jt}$, where c_{jt}^U denotes the (unobserved) ideology of the clerks employed by justice j in term t , and $0 \leq \rho \leq 1$. In this case, the true ideology of the clerks hired in a justice-term, c_{jt}^* , is given by

$$c_{jt}^* = (1 - \mu) c_{jt} + \mu c_{jt}^U$$

²⁴Judicial Common Space scores are calculated from the ideology of the political actors responsible for their nomination (Epstein et al., 2007). Specifically, the judge’s JCS score reflects the ideology of the appointing President, or, if the President and the home-state Senator at the time of nomination are of the same party, then of the home-state Senator (or an average of the two home-state Senators, if both are of the same party).

where μ denotes the fraction of clerks in the sample whose ideology is unobserved. Under this assumption, it is straightforward to show that the coefficient estimated from the observed clerk data must be scaled by a factor of $\frac{1}{1-\mu(1-\rho)}$ to obtain the true effect of clerk ideology on judicial outcomes.²⁵ Setting $\mu = 0.278$ (which corresponds to the degree of missingness in our data) and applying this adjustment to our estimated coefficient from the baseline first-differences model yields an adjusted coefficient that ranges from 1 percentage point (corresponding to $\rho = 1$) to 1.4 percentage points (corresponding to $\rho = 0$). In words, the greater the degree to which the ideological intensity of the clerk that do not donate are attenuated relative to the clerks that do, the larger the true coefficient is.

Notably, since $\rho \leq 1$, it will always be the case that the adjustment is weakly greater than 1. In other words, the adjustment highlights that for our observed coefficient to be larger in magnitude than the true effect, it must be the case that the clerks who do not donate actually have *more* intense preferences, on average, than those that do donate. Thus our unadjusted results may plausibly be interpreted as a lower bound for the true effect, with the corresponding upper bound given by 0.013. Consistent with the upper bound derived through this adjustment, Column 3 of Table 7 shows that assuming an ideology of 0 for each clerk who did not donate yields an estimated effect of 1.1 percentage points.

In summary, missing data is likely to moderately bias the size of our estimated coefficients towards zero but is unlikely to be driving our finding of an effect.

5.3 Endogenous Movement in Clerk Ideology

A third potential concern with our approach stems from the fact that our measure of clerk ideology is derived from all political donations made by a clerk, not just

²⁵The Technical Appendix provides this calculation.

those in the year of the clerkship. Specifically, one might be concerned that the justice influences the evolution of the clerk’s ideology in future years, e.g., clerks who work for liberal justices might become more liberal after the clerkship. However, because our identification strategy exploits within-justice variation in clerk ideology, such an effect is unlikely to be generating our results. That is, the key assumption for us is that if clerks A and B worked for the same justice, and A is more liberal than B after the clerkship, then A would tend to have been more liberal than B during the clerkship as well. This observation is supported by the finding from the political science literature that an individual’s ideology tends to be stable over time (e.g., [Green et al., 2004](#); [Bonica, 2014](#)).

Supplemental Table [A5](#) explores the evolution of clerk ideology over time using the relatively small fraction of clerks who donate both before and after their clerkship. Because so few Supreme Court clerks fall into this category, this analysis also uses data on clerks from federal district and circuit courts, from 1995 to 2004, described in [Bonica et al. \(2016a\)](#). The results confirm that post-clerkship ideology is strongly correlated with pre-clerkship ideology, and provide no evidence that the ideology of a clerk’s justice influences his or her subsequent ideological evolution.

Finally, it is possible that clerk ideology may evolve in future years according to the manner in which the justice votes during the term in which the clerk is employed. The story here would be that a clerk employed in a term in which the justice voted liberally in a large fraction of cases would become more liberal over time compared to a different clerk who worked for the same justice but who was employed during a term in which the justice voted conservatively in more cases. This evolution might occur because of cognitive dissonance—helping to defend the justice’s conservative positions might make an impressionable clerk more conservative, and vice-versa. Though theoretically possible, this mechanism seems unlikely to be important enough to be

responsible for much (if any) of our estimated effect. That is, it would need to be the case that working on a slightly different set of cases in one term (approximately 5%) could transform clerks from one side of the political spectrum to the other (CFscores from the 25th percentile to the 75th percentile)—an effect that appears implausibly large given the observed stickiness of political ideology.

6 Conclusion

In this paper we have studied whether law clerks affect how Supreme Court justices vote. We find strong evidence that they do. For major cases, and for cases where the justices are close to evenly divided, the influence of clerks is even greater.

When interpreting our results, several factors suggest the actual effect of clerk ideology may be greater than what our point estimates suggest. First, as discussed in Section 4, the measurement error induced by the fact that some clerks are not observed to make political donations likely attenuates our estimated coefficient. Our discussion in that section suggests that accounting for that bias would inflate our estimated effect by up to 40 percent.

Second, our specifications assume that the influence of clerk ideology occurs at the justice-term level, with each clerk donating equally to the disposition of each case. This assumption is appropriate if each clerk has an equal opportunity to weigh in on each case that the justice decides, but will be violated if certain clerks influence a particular decision more than others. For example, cases are often assigned to a primary clerk to work on, and the effect of a clerk's ideology may be greatest on cases for which that clerk has been assigned. The measurement error associated with this misspecification could further attenuate our estimated coefficients.

Third, we have focused on measuring clerk influence along one particular

dimension in which clerk preferences differ (political ideology), but clerks may influence Supreme Court decision-making in other ways as well (e.g., commitment to precedent). Similarly, in addition to case outcomes clerks may influence the breadth of judicial decisions, which cases are selected for consideration, and the legal analysis employed in the opinion itself. Such questions are important topics for future research.

Finally, although our focus has been on providing a descriptive account of clerkship influence, our results speak to important normative issues as well. For example, judges have been criticized for excessively relying on clerks. There are two reasons for this. First, unlike judges, clerks are neither democratically elected or confirmed by a democratically elected body (Clark, 1959). Second, clerks, being recent law school graduates, have limited legal and practical experience, which would tend to reduce the quality of work that is delegated to them (Posner, 1983). These arguments are stronger in a world in which judges delegate their decision-making powers to clerks directly, and weaker in a world in which judges—like government officials in other parts of government—rely on staff for input but ultimately make the important decisions themselves. Thus to the extent our results support persuasion over delegation models of clerkship influence, our findings suggest the influence of clerks is less troubling than one might otherwise believe.

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Tables

Table 1: Descriptive Statistics

	<i>Conservative Vote</i>		<i>Clerk Ideology</i>	
	Overall	Term	Overall	Term
Mean	0.458	0.468	-0.502	-0.521
Min	0	0.055	-1.775	-1.775
Max	1	0.779	1.440	1.376
St. Dev.	0.498	0.163	0.976	0.788
Mean Within- Justice St. Dev.	0.475	0.071	0.795	0.588
Obs.	66209	439	1155	498

Table 2: Influence of Clerk Ideology on Justice Voting

	<i>Conservative Vote</i>		Δ <i>Conservative Votes</i>	
	(1)	(2)	(3)	(4)
Clerk Ideology	0.017*** (0.006)	0.012*** (0.003)		
Δ Clerk Ideology			0.008* (0.004)	0.010** (0.004)
<i>Covariates</i>				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	No	Yes
Justice Time Trends	No	Yes	No	No
N	66,209	66,209	404	404
R ²	0.129	0.134	0.500	0.535
Mean Conservative Votes	0.458	0.458	0.468	0.468

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Columns (1) and (2): standard errors clustered by justice and case. Specifications include issue area fixed effects. Columns (3) and (4): standard errors clustered by justice.

Table 3: Heterogeneous Effects: Case Attributes

	<i>Major Case</i>		<i>Controversial</i>	
	<i>NYT</i> (1)	<i>CQ</i> (2)	<i>Unanimous</i> (3)	<i>Close</i> (4)
Δ Clerk Ideology	0.002 (0.007)	-0.003 (0.012)	0.021*** (0.007)	-0.005 (0.006)
Major Case	-0.004 (0.003)	-0.004 (0.004)		
Major Case $\times \Delta$ Clerk Ideology	0.039*** (0.011)	0.049** (0.021)		
Unanimous Case			0.009*** (0.003)	
Unanimous Case $\times \Delta$ Clerk Ideology			-0.028*** (0.009)	
Close Case				-0.005 (0.004)
Close Case $\times \Delta$ Clerk Ideology				0.037*** (0.012)
N	807	807	808	806
R ²	0.261	0.286	0.37	0.249
Mean Conservative Votes	0.447	0.437	0.461	0.471

Note: Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Column (1): NYT indicates a case appeared on the front page of the *New York Times*. Column (2): CQ indicates a case is classified as “major” by *Congressional Quarterly* in terms of its legal significance. Column (3): Unanimous indicates whether the decision was unanimous. Column (4): Close Case indicates the vote on the decision was 5-4 or 6-3.

Table 4: Heterogeneous Effects: Issue Areas

	Issue Area					
	Civil Rights	Criminal Procedure	Economic Activity	First Amendment	Judicial Power	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Clerk Ideology	0.025*** (0.008)	0.009 (0.007)	0.011 (0.010)	0.034*** (0.011)	-0.007 (0.010)	-0.002 (0.010)
N	403	404	404	403	404	404
R ²	0.448	0.489	0.576	0.490	0.562	0.589
Mean Conservative Votes	0.448	0.514	0.434	0.449	0.599	0.382

Note: Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Each column reports separate results for models ran by issue area, as specified in the heading.

Table 5: Heterogeneous Effects: Justice Attributes

	<i>Workload</i>	<i>Ideology</i>	<i>Experience</i>
	(1)	(2)	(3)
Δ Clerk Ideology	-0.006 (0.011)	0.010** (0.004)	0.015** (0.007)
Workload × Δ Clerk Ideology	0.018 (0.011)		
Liberal Justice × Δ Clerk Ideology		-0.001 (0.009)	
Experience × Δ Clerk Ideology			-0.000 (0.001)
N	404	404	404
R ²	0.518	0.515	0.516
Mean Conservative Votes	0.468	0.468	0.468

Note: Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Workload indicates whether clerks author the first drafts of a justice's opinions. Liberal Justice indicates the justice's conservative vote share is below the sample median. Experience indicates the number of years the justice has been on the bench.

Table 6: Timing Assumptions

	<i>Conservative Vote</i>		Δ <i>Conservative Votes</i>	
	(1)	(2)	(3)	(4)
A. Cases After December				
Clerk Ideology	0.017*** (0.006)	0.010* (0.006)		
Δ Clerk Ideology			0.008 (0.005)	0.010* (0.005)
N	32,206	32,206	403	403
R ²	0.127	0.132	0.456	0.463
Mean Conservative Votes	0.461	0.461	0.469	0.469
B. Cases in Last 25 Years				
Clerk Ideology	0.014*** (0.005)	0.009 (0.008)		
Δ Clerk Ideology			0.011*** (0.004)	0.011** (0.004)
N	26,243	26,243	204	204
R ²	0.112	0.114	0.545	0.562
Mean Conservative Votes	0.507	0.507	0.511	0.511
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	No	Yes
Justice Time Trends	No	Yes	No	No
<i>Note:</i> Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Columns (1) and (2): standard errors clustered by justice and case. Specifications include issue area fixed effects. Columns (3) and (4): standard errors clustered by justice. Panel A restricts the sample to cases argued in January through June, after the subsequent year's clerk will almost always have been hired. See Figure 6 and text description. Panel B restricts to the last 25 years of the sample, during a time in which clerks are usually hired at least on term before the clerkship begins.				

Table 7: Investigating Selection

	<i>Fill in Missing With</i>		
	<i>Imputed</i> (1)	<i>JCS Score</i> (2)	<i>Zeros</i> (3)
Δ Clerk Ideology	0.009* (0.005)	0.013** (0.005)	0.011* (0.006)
N	404	404	404
R ²	0.512	0.529	0.513
Mean Conservative Votes	0.468	0.468	0.468

Note: Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Column (1) reports the results for imputed clerk ideology of the missing clerks, based on their observable characteristics. See the Technical Appendix for details of the imputation procedure. Column (2) reports the results using as a proxy for unobserved clerk ideology the the ideology of the prior judge who employed the clerk prior to the Supreme Court justice. Column (3) reports the results using a CFscore of 0 for unobserved clerks.

Figures

Figure 1: Conservative Votes by Term

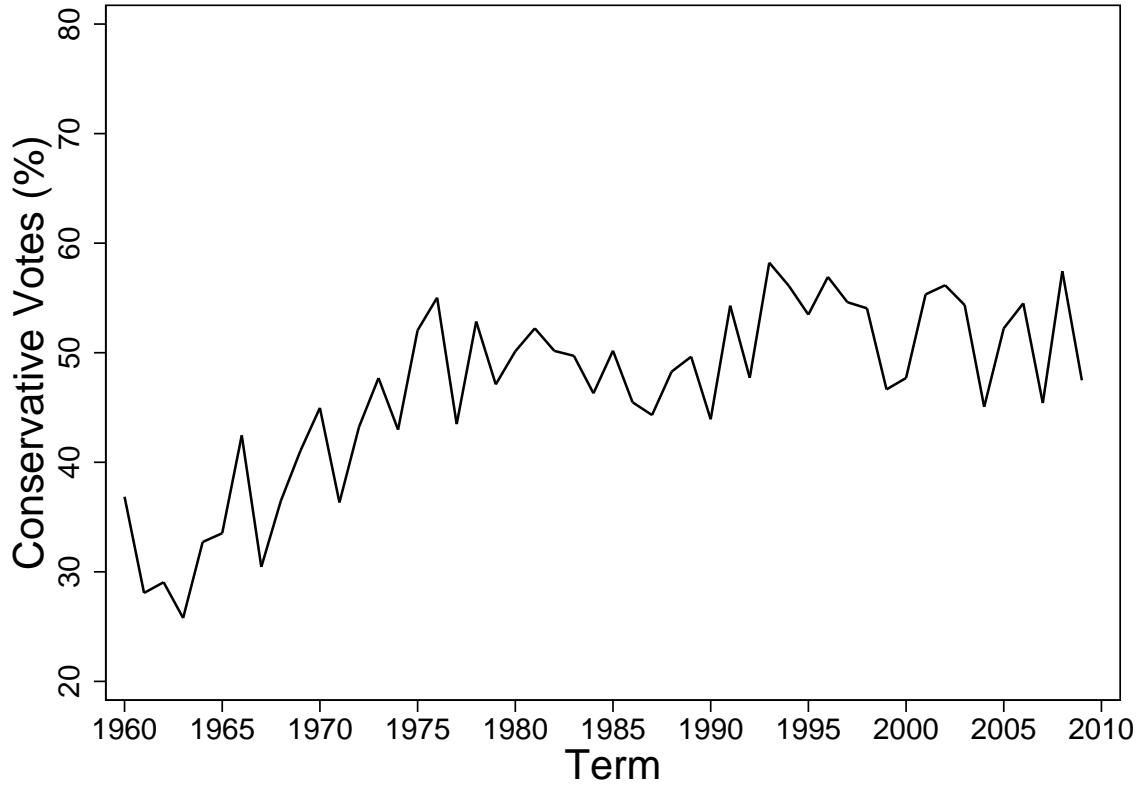


Figure 2: Number of Cases and Clerks By Term

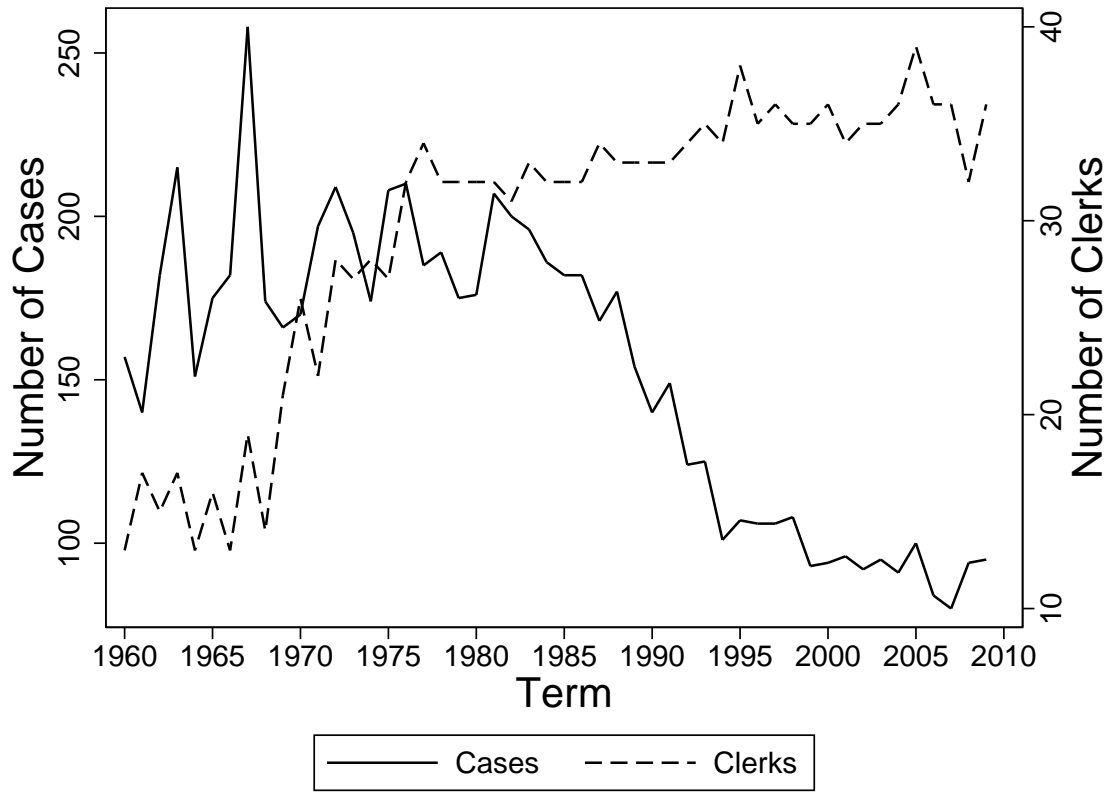


Figure 3: Supreme Court Clerk Ideology

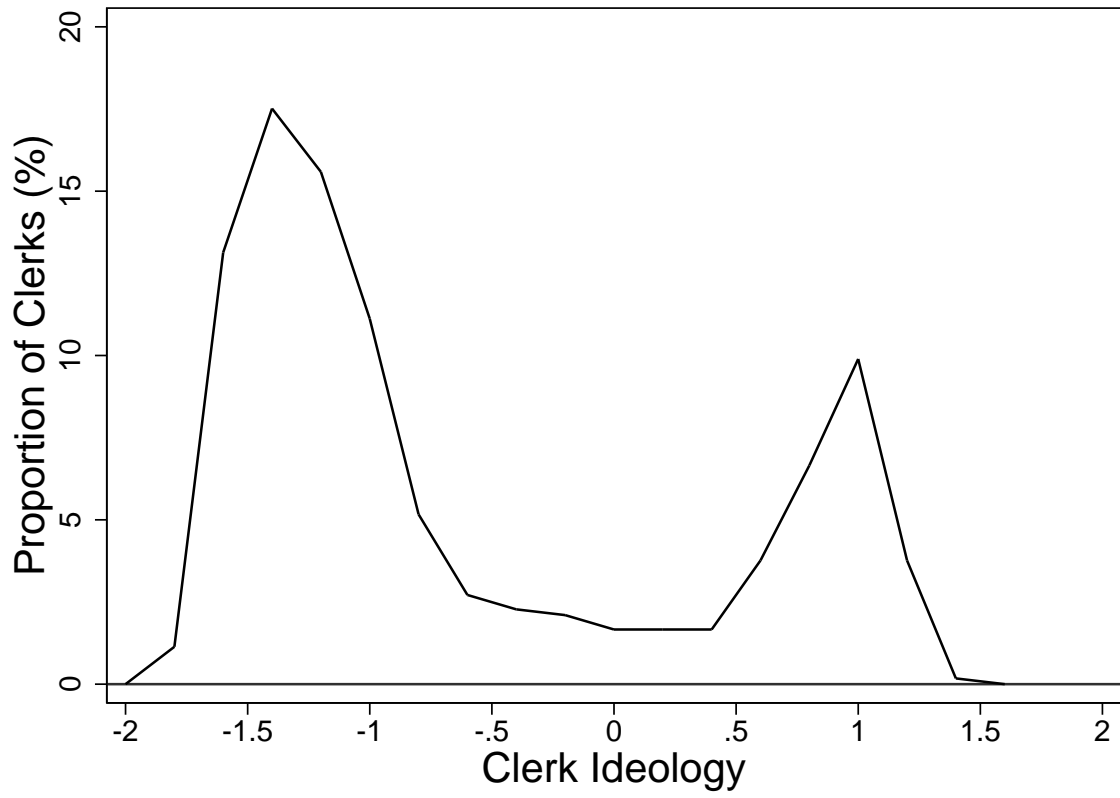


Figure 4: Influence of Clerk Ideology on Justice Voting

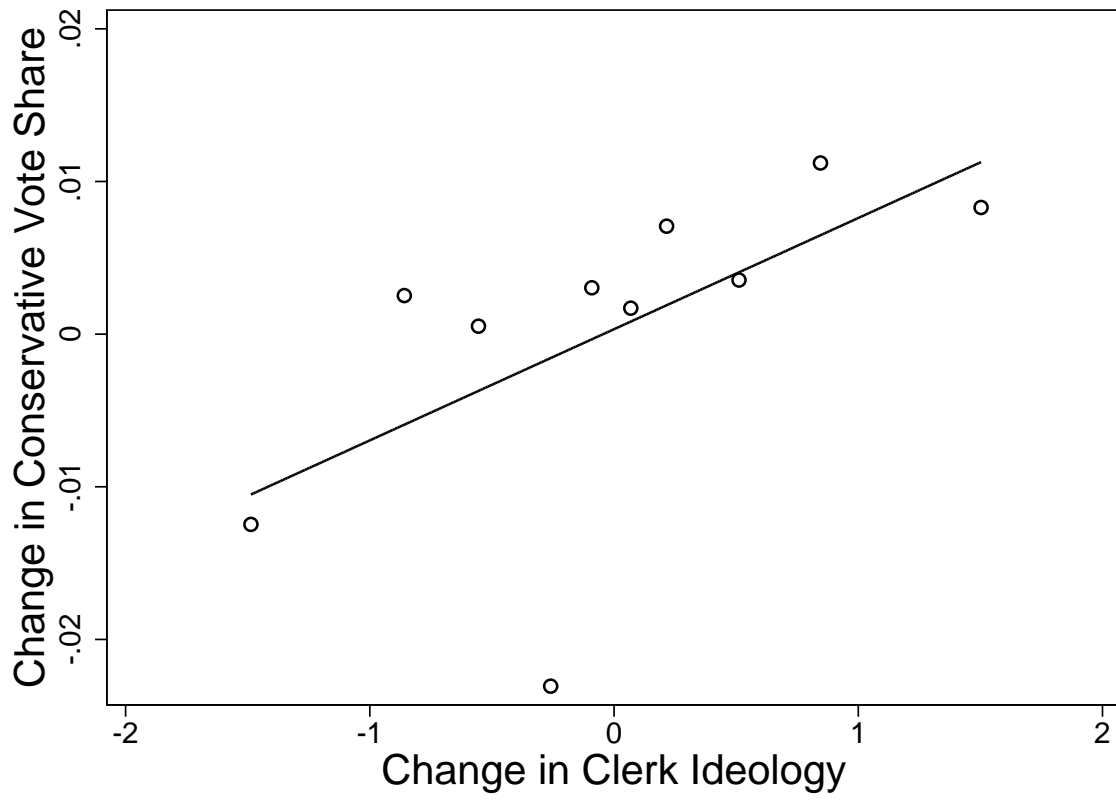
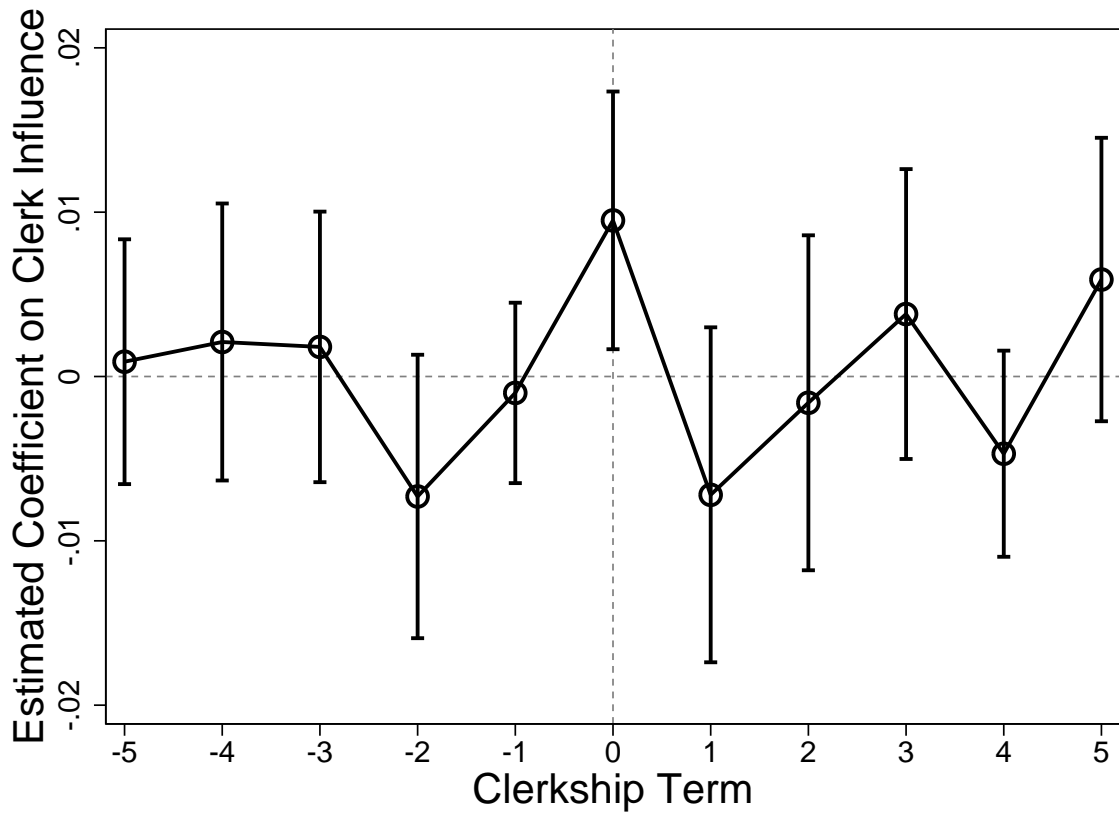


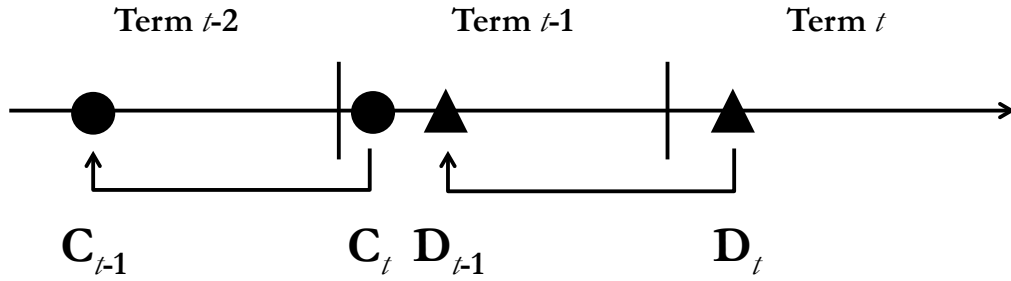
Figure 5: Placebo Tests



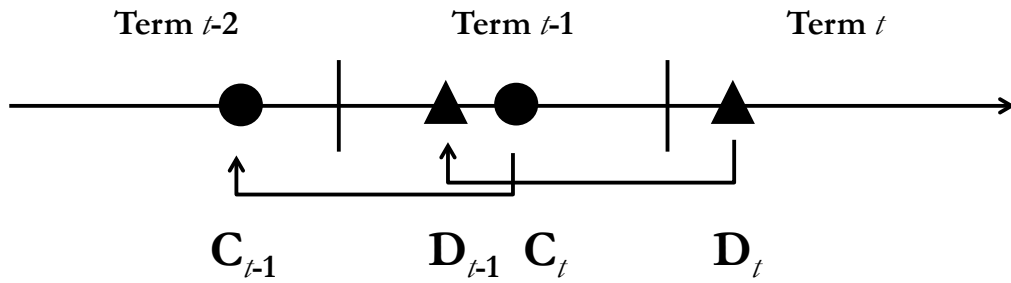
Note: Bars reflect 95% confidence interval (clustered standard errors).

Figure 6: Alternative Assumptions on the Timing of Hiring

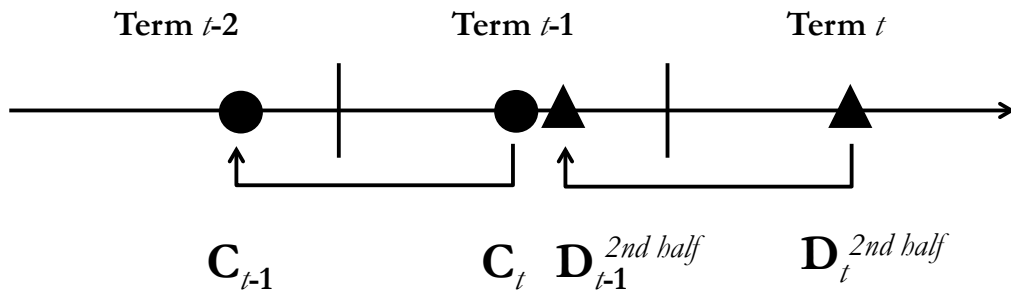
A. Clerks Hired Before Prior Term Cases Decided



B. Clerks Hired Less Than One Term Out



C. Clerks Hired Less Than One Term Out – Restricted Sample



A1 Technical Appendix

A1.1 Match Process

We matched the clerkship identity data to political contribution data in the Database on Ideology, Money, and Elections (DIME) by way of a two-step matching process. First, because the identifying information reported in the clerkship data was limited to name, law school attended, and year of graduation, we initially matched the clerkship data to the Martindale-Hubbell directory, a comprehensive database of attorneys in the United States today. Given the much smaller population of lawyers as compared to donors, a small amount of information (name, law school, and year of law degree) was sufficient to uniquely match most clerks against records in the Martindale-Hubbell directory.

Next, we matched the individuals matched to the Martindale-Hubbell directory to DIME, based on first, last, and middle name; suffix; title; address; and firm or employer. To adjust for variations across records, we used the Jaro-Winkler algorithm, which measures the distance between two strings (in this case, names); the lesser the distance, the more alike two strings are.

A1.2 Imputation of Missing Ideology Data

Using a nonparametric missing value imputation process based on a random forest model (Stekhoven and Buhlmann, 2012), we impute missing CFscores for clerks. The procedure is a machine-learning approach that accounts for nonlinearities and interactions between variables and proceeds in an iterative fashion, and accounts for variables that are only partially observed.

We include a number of attributes to impute clerk CFscores, including: (1) gender, (2) law school attended, (3) state in which lower court judges which employed the clerk is located, (4) the lower judge’s own estimated CFscore, (5) the identity of the president who appointed the lower court hiring judge, (6) attributes of the clerk later in life (e.g., employment type, current state of residence). See Bonica et al. (2016b) for a more detailed description of the dataset and how we obtained these additional variables for clerks, and see Bonica et al. (2016a) for more details of the imputation.

A1.3 Derivation of Adjustment for Missing Ideology Data

Let μ denote the fraction of the clerks with missing ideology scores. Suppose the mean ideology of the unobserved clerks in a justice-term is equal to ρ times the mean ideology of the observed clerks in that justice-term,

$$c_{jt}^U = \rho c_{jt} \tag{9}$$

where $\rho < 1$ corresponds to the case in which clerks with more intense ideological preferences are more likely to donate.

True clerk ideology in a justice-term is given by $c_{jt}^* = (1 - \mu)c_{jt} + \mu c_{jt}^U$, i.e., the weighted average of the observed and unobserved clerks. Using (9), we can re-write

this to obtain

$$c_{jt} = \frac{c_{jt}^*}{1 - \mu + \rho\mu} \quad (10)$$

Judicial voting in a justice-term is a function of the true clerk ideology in a justice-term, $y_{jt} = \beta c_{jt}^* + \varepsilon_{jt}$, or, using (10):

$$y_{jt} = \beta(1 - \mu + \rho\mu) c_{jt} + \varepsilon_{jt} \quad (11)$$

Consequently, regressing voting behavior (y_{jt}) on observed clerk ideology (c_{jt}) yields an estimated coefficient of $\hat{\beta} = \beta(1 - \mu + \rho\mu)$. The true effect of clerk influence on judicial voting can thus be obtained by scaling the estimated coefficient:

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

In our data, $\mu = 0.278$ and $\hat{\beta} = 0.010$ in our preferred specification, implying a true coefficient for clerk influence given by $\beta = \frac{0.010}{0.722 + .278*\rho}$. Thus, depending on the value of ρ (i.e., the degree to which variation in contribution rates are driven by variation in the intensity of clerks' ideological preference), the value of β lies between 0.010 and 0.014.

A2 Supplementary Appendix

The Supplementary Appendix provides additional information on five topics. First, Section [A2.1](#) provides information on the rate of missing ideology data in our sample. Figure [A1](#) reports the distribution of the number of missing clerks at the justice-term level. Table [A1](#) provides a breakdown of the number of observed and total clerks at the justice-term level.

Second, Section [A2.2](#) reports more details related to justice voting and their clerks. Table [A2](#) reports the Justice information on the number of terms present in the sample, the number of votes cast, and the percent of votes cast that were conservative. It also reports the number of clerks hired, the proportion of clerks observed, and the mean ideology of the clerks.

Third, Section [A2.3](#) provides more details on the distribution of clerk CFscores within justices. Figure [A2](#) reports the distribution of clerk CFscores by justice. Figure [A3](#) plots the clerk-level CFscores over time along with the mean annual CFscore by justice. Figure [A4](#) provides binned scatterplot of annual justice voting and mean clerk CFscore along with a lowess curve of the same relationship.

Fourth, Section [A2.4](#) provides details on conservative voting by subgroup. Table [A3](#) reports the mean conservative vote share and the proportion of cases within subgroups used in the analysis in the paper.

Fifth, Section [A2.5](#) provide alternative results using alternative statistics of annual clerk CFscores. Table [A4](#) reports the main results using the median, minimum, and maximum clerk CFscore as the primary independent variable.

Sixth, Section [A2.6](#) tests the stability of clerk ideology over time. Table [A5](#) reports the results of regressions estimating the relationship between clerk ideology before clerking and after clerking.

A2.1 Missing Ideology Data

Figure A1: Number of Clerks Unobserved by Justice-Term

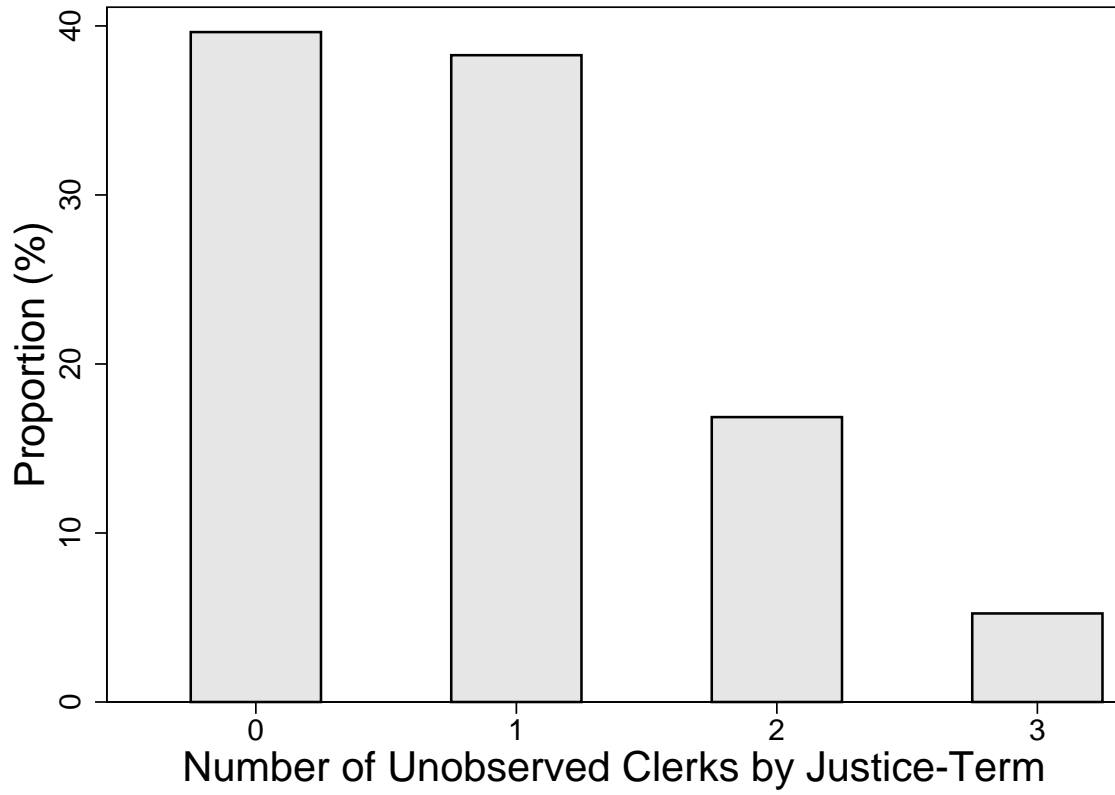


Table A1: Number of Law Clerks Matched to Donations Divided by Total Law Clerks by Justice-Term

Term	<i>Justice</i>																						
	Alito	Black	Blackmun	Brennan	Breyer	Burger	Clark	Douglas	Ginsburg	Harlan	Kennedy	Marshall	O'Connor	Powell	Rehnquist	Roberts	Scalia	Souter	Stevens	Stewart	Thomas	Warren	White
1960		1/2		1/2			2/2	1/1		2/2										0/1		2/2	
1961		1/1		2/2			1/2	1/1		2/2										0/1		1/3	1/2
1962		1/2		1/2			2/2	1/1		2/2										0/1		2/3	1/1
1963		1/1		1/2			2/2	1/1		2/2										2/2		2/3	1/2
1964		1/1		0/2			1/1	1/1		2/3										1/1		3/3	1/1
1965		2/2		2/2			2/2	1/1		1/2										2/2		2/3	1/1
1966		1/2		2/2			1/2	1/1		2/2										1/2		0/3	2/2
1967		1/2		2/2				1/2		2/2	2/2									2/2		1/3	2/2
1968		1/1		2/2				0/1		2/2	0/2									2/2		3/3	2/2
1969		1/2		1/2		4/7		1/1		2/3	2/2									2/2			1/2
1970		3/3	2/3	2/3		2/3		2/2		2/3	3/3									3/3			2/3
1971			2/2	3/3		2/5		1/3			2/3									2/3			3/3
1972			3/3	2/3		3/4		2/3			3/3		2/3	3/3						2/3			3/3
1973			1/3	2/3		2/3		2/3			3/3		3/3	3/3	1/3					3/3			3/3
1974			2/3	2/3		2/3		3/3			1/3		3/4	2/3						2/3			1/3
1975			2/3	3/3		3/3		2/2			3/3		2/4	1/3					1/1	3/3			2/2
1976			2/3	3/4		3/3					4/4		1/4	2/3					2/5	3/4			2/2
1977			3/4	4/4		3/4					3/4		4/4	2/3					3/3	2/4			3/4
1978			4/4	2/4		2/4					2/4		1/4	3/3					2/2	2/3			3/4
1979			2/4	4/4		2/4					3/4		4/4	3/3					2/2	3/3			3/4
1980			2/4	3/4		4/4					2/4		3/4	3/3					2/2	3/3			3/4
1981			3/4	4/4		4/4					4/4	3/4	4/4	3/3					1/2				3/3
1982			3/4	2/3		2/4					4/4	2/4	4/4	2/3					2/2				2/3
1983			1/4	3/4		4/4					3/4	4/4	3/4	2/3					2/2				3/4
1984			3/4	4/4		4/4					3/4	4/4	1/4	3/3					2/2				2/3
1985			3/4	3/4		4/4					3/4	4/4	2/4	1/3					2/2				3/3
1986			3/4	4/4							3/4	4/4	1/4	1/3			1/4		2/2				2/3
1987			3/4	4/4						3/4	4/4	4/4		1/3		4/4		3/3					3/4
1988			3/4	3/4						5/5	4/4	2/4		3/3		2/4		2/2					1/3
1989			3/4	2/4						4/4	2/4	4/4		2/3		2/4		1/2					4/4
1990			2/4							4/4	1/4	1/4		2/3		4/4	3/4	2/3					2/3
1991			4/4							3/4		2/4		2/3		3/4	2/3	2/3			5/5		2/3
1992			4/4							3/4		3/4		2/3		3/4	2/4	2/3			2/4		2/4
1993			3/4					4/4		4/4		4/4		1/3		4/4	4/4	2/4			4/4		
1994					4/4			4/4		1/4		3/4		1/3		3/4	4/4	2/3			2/4		
1995					3/4			2/5		3/4		3/4		2/4		3/5	4/4	2/3			3/5		
1996					4/4			4/4		2/5		2/4		2/3		4/4	4/4	3/3			4/4		
1997					3/4			4/4		3/4		3/5		3/4		2/4	4/4	3/3			3/4		
1998					2/4			2/4		2/5		3/4		0/3		2/5	4/5	3/4			4/4		
1999					3/4			3/5		3/4		2/4		2/3		3/4	2/4	3/3			4/4		
2000					4/4			3/4		4/4		3/4		2/3		2/4	3/5	3/3			3/5		
2001					3/4			3/4		4/4		3/4		1/3		2/4	3/4	3/3			4/4		
2002					2/4			3/4		4/4		3/4		3/3		4/4	4/4	2/4			2/4		
2003					3/4			4/4		3/4		3/4		2/3		2/4	4/4	2/4			3/4		
2004					3/4			3/4		2/4		3/4		3/3		3/4	3/5	2/4			3/4		
2005	1/2				1/4			3/4		2/4		3/4			1/3	5/7	1/4	4/4			3/3		
2006	4/4				3/4			4/4		2/4					2/4	3/4	3/4	3/4			3/4		
2007	1/4				1/4			3/4		4/4					2/4	3/4	3/4	1/4			3/4		
2008	3/4				2/4			3/4		2/4					3/4	1/4	3/4	0/4			3/4		
2009	1/4				2/4			2/4		3/4					3/4	4/4		2/4			3/4		

A2.2 Justice Voting

Table A2: Descriptive Statistics of Justice Voting and Clerks

Justice	<i>Justice Voting</i>			<i>Clerks</i>		
	Terms	Votes Cast	Conservative Votes (%)	Number Hired	Proportion Observed (%)	Mean Ideology
Alito, Samuel	4	425	60.9	18	55.0	0.83
Black, Hugo L	10	1970	30.6	19	77.3	-0.30
Blackmun, Harry A	23	4278	46.2	88	71.9	-0.81
Brennan, William J	29	5389	27.4	91	79.6	-0.78
Breyer, Stephen	15	1539	42.5	64	67.2	-1.16
Burger, Warren E	16	3225	62.9	67	76.3	-0.29
Clark, Tom C	6	1201	40.4	13	85.7	-0.46
Douglas, William O	15	2578	18.5	26	87.8	-0.72
Ginsburg, Ruth Bader	16	1667	39.8	70	77.9	-1.09
Harlan, John M	10	1970	54.0	25	86.4	-0.74
Kennedy, Anthony	22	2552	57.3	95	74.1	0.15
Marshall, Thurgood	23	4304	25.4	82	79.3	-0.89
O'Connor, Sandra Day	24	3305	59.3	101	74.4	-0.63
Powell, Lewis F	14	2874	55.3	58	66.1	-0.55
Rehnquist, William	32	4892	67.9	98	67.4	0.11
Roberts, John	4	452	60.6	19	56.7	0.11
Scalia, Antonin	23	2759	64.9	101	68.6	0.27
Souter, David	18	1981	42.1	78	76.8	-1.09
Stevens, John Paul	34	4762	37.8	100	79.4	-0.96
Stewart, Potter	17	3409	47.3	48	85.6	-0.67
Thomas, Clarence	18	1938	67.6	78	78.7	0.79
Warren, Earl	8	1452	23.8	23	70.8	-0.41
White, Byron R	31	5728	48.6	90	78.4	-0.59

A2.3 Clerk CFscores

Figure A2: Distribution of Clerk CFscores By Justice

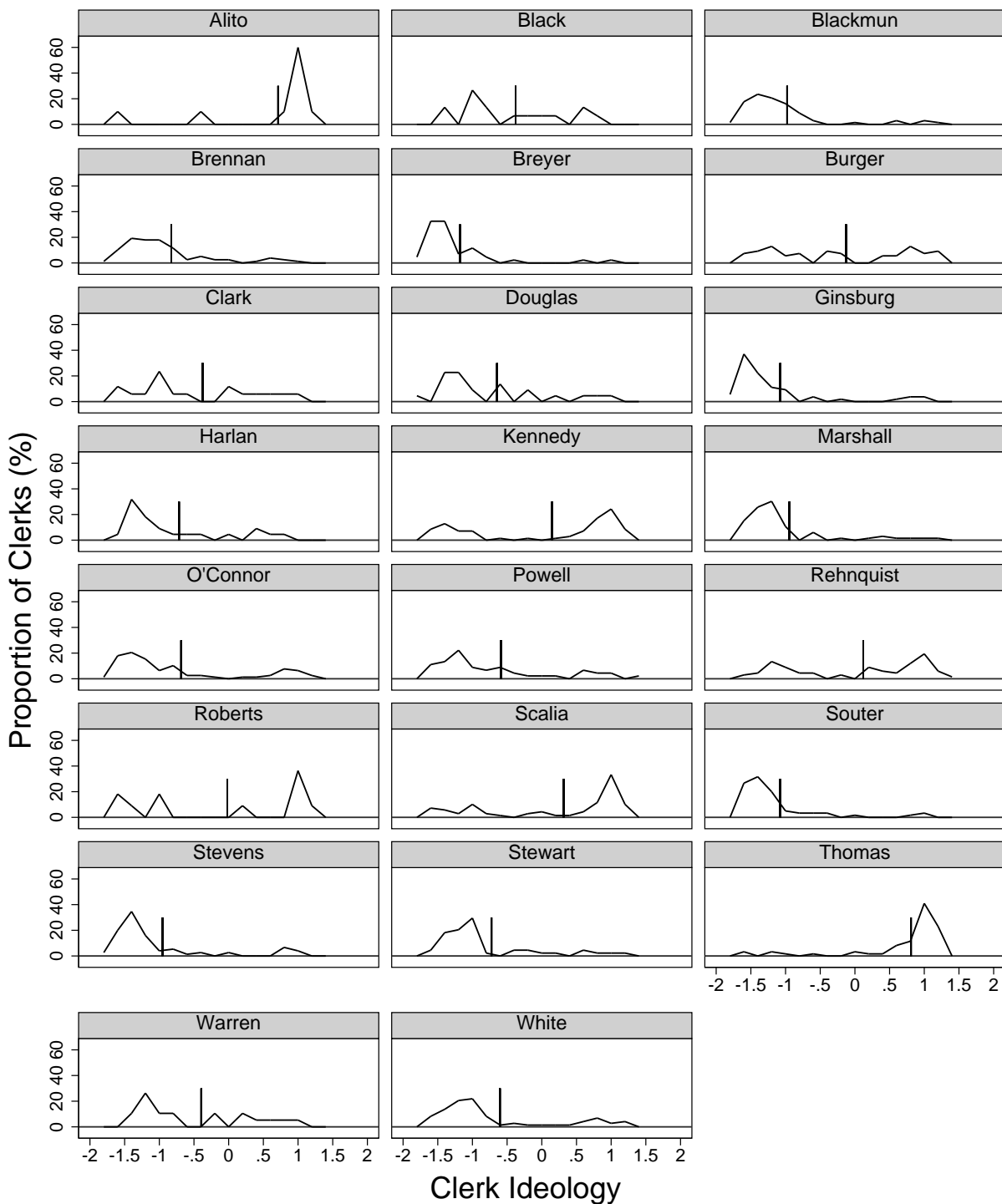


Figure A3: Scatterplot of Underlying Clerk Ideology by Justice Over Time

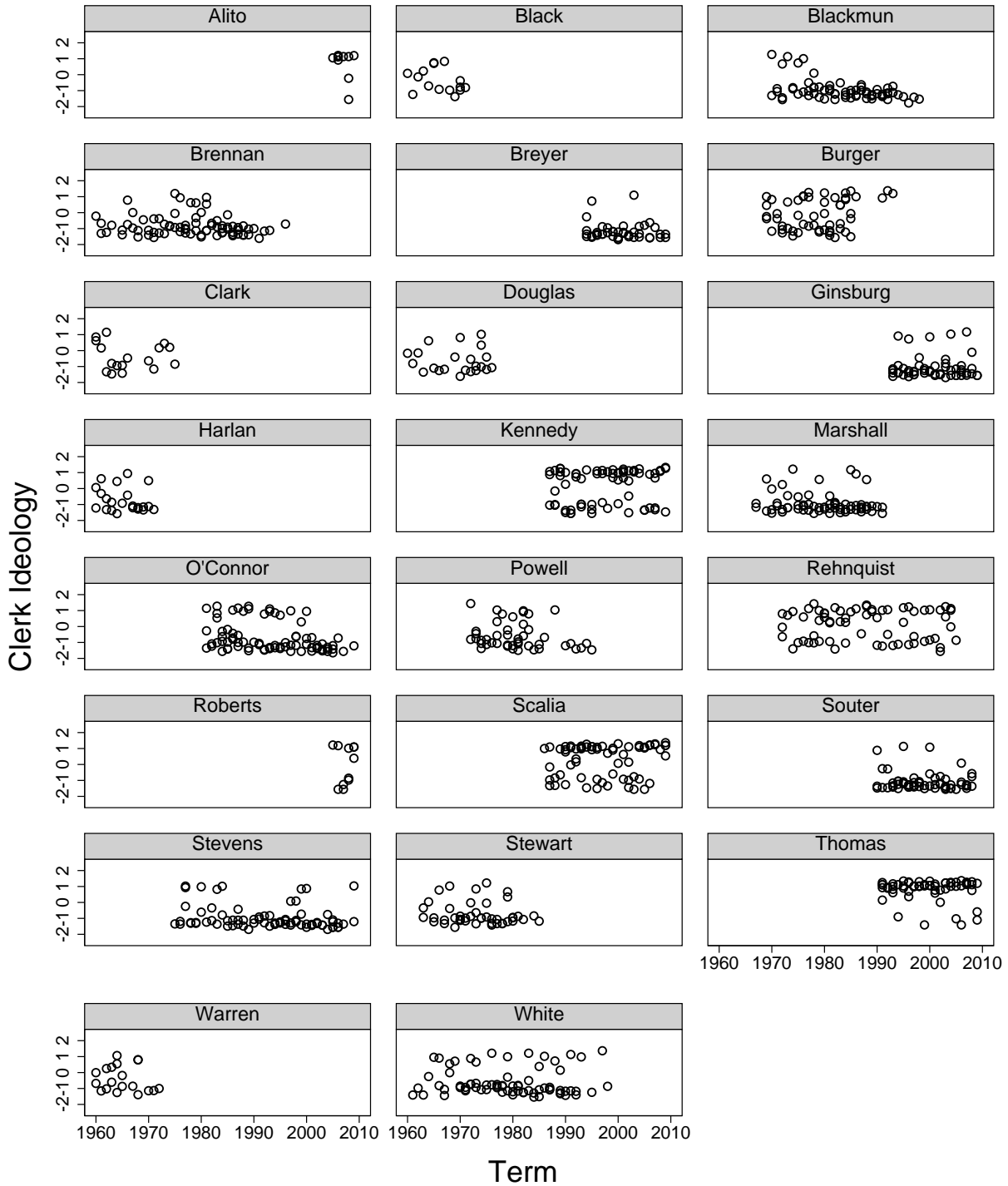
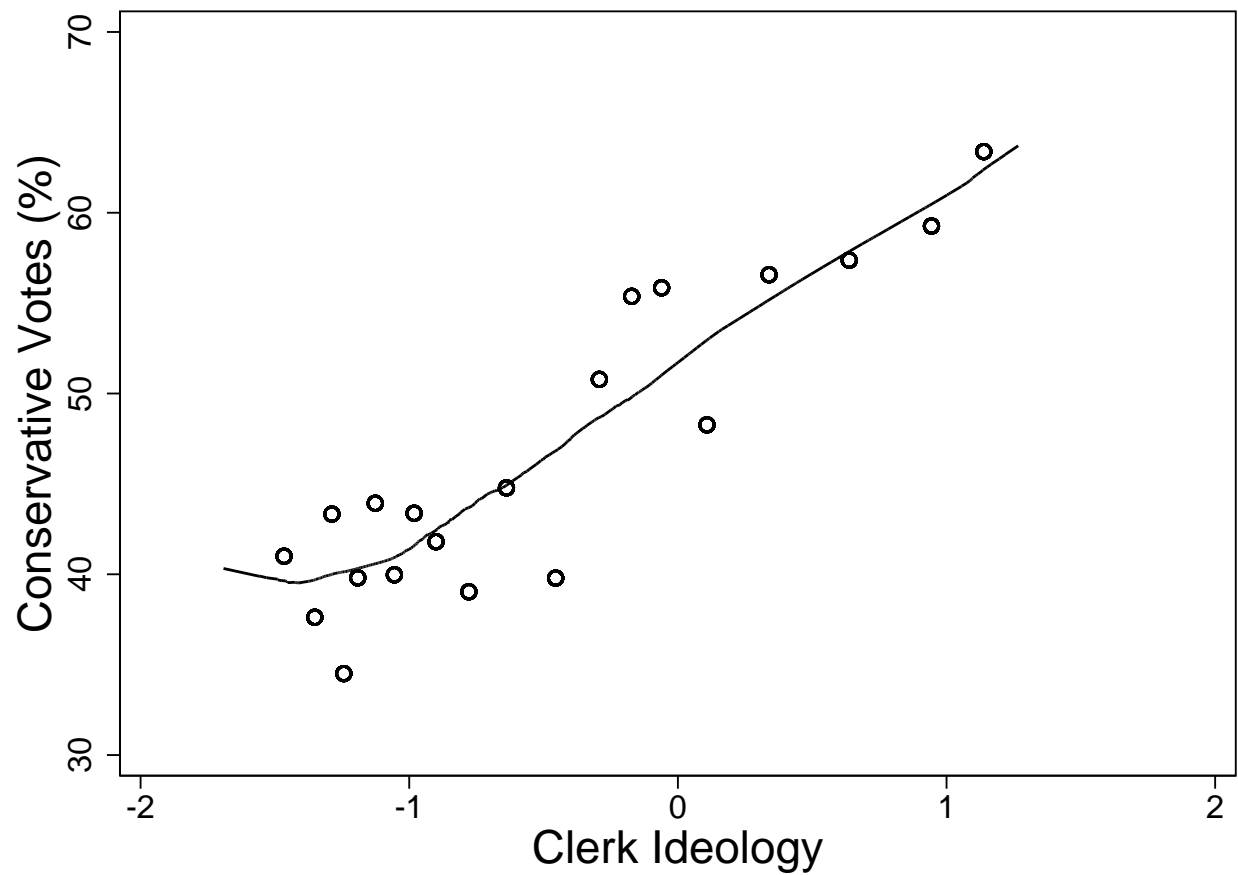


Figure A4: Relationship Between Justice-Term Level Mean Clerk CFscore and Percent Conservative Votes



A2.4 Conservative Votes by Subgroups

Table A3: Mean Conservative Vote Share and Proportion of Cases by Subgroups

	<i>Conservative Vote Share (%)</i>	<i>Proportion of Cases (%)</i>
All Cases	46.8	100.0
A. Case Attributes Subgroups		
NYT Cases	42.0	14.8
Not NYT Cases	47.4	85.2
CQ Cases	40.1	6.5
Not CQ Cases	47.3	93.5
Unanimous Cases	43.4	40.1
Not Unanimous Cases	48.9	59.9
Close Cases	48.0	29.7
Not Close Cases	46.1	70.3
B. Issue Areas Subgroups		
Civil Rights	43.8	18.3
Criminal Procedure	50.2	22.5
Economic Activity	41.9	18.3
First Amendment	41.3	8.3
Judicial Power	60.2	13.6
Other	38.5	18.9
C. Justice Attributes Subgroups		
Clerks Draft Opinion	48.2	93.1
Clerks Not Draft Opinion	24.0	6.9
Cons. Ideology above Median	57.5	57.2
Cons. Ideology below Median	32.5	42.8

Note: Each row reports the conservative vote share for subgroups of data indicated in the first column, as well as the proportion of total cases for the subgroup. Panel A reports statistics for: (i) high profile case, proxied by whether a case is high profile by whether it appears on the front page of the *New York Times* (NYT) (Epstein and Segal, 2000), (ii) legally significant cases, proxied by whether a case is classified as “major” by *Congressional Quarterly* (CQ), (iii) unanimous and non-unanimous cases, and (iv) “close” cases, defined as cases where the outcome is decided by a vote of 5-4 or 6-3. Panel B reports statistics by issue area of cases. Panel C reports statistics: (i) by whether the justice has clerks write a first draft of opinions, and (ii) liberal justices, based on whether the justice’s conservative vote share is above or below the sample median.

A2.5 Alternative Ideology Statistics

Table A4: Influence of Clerk Ideology on Justice Votes Using Alternative Statistics

	<i>Conservative Vote</i>		Δ <i>Conservative Votes</i>	
	(1)	(2)	(3)	(4)
A. Median Justice-Year Clerk CFscore				
Clerk Ideology	0.014** (0.006)	0.010 (0.009)		
Δ Clerk Ideology			0.007 (0.004)	0.008* (0.004)
R ²	0.129	0.134	0.509	0.513
B. Minimum Justice-Year Clerk CFscore				
Clerk Ideology	0.014*** (0.005)	0.013 (0.008)		
Δ Clerk Ideology			0.012*** (0.004)	0.013*** (0.004)
R ²	0.129	0.134	0.520	0.526
C. Maximum Justice-Year Clerk CFscore				
Clerk Ideology	0.011** (0.005)	0.005 (0.004)		
Δ Clerk Ideology			0.001 (0.002)	0.002 (0.002)
R ²	0.129	0.134	0.503	0.508
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	No	Yes
Justice Time Trends	No	Yes	No	No
N	66,209	66,209	404	404
Mean Conservative Votes	0.458	0.458	0.468	0.468

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Columns (1) and (2): standard errors clustered by justice and case. Specifications include issue area fixed effects. Columns (3) and (4): standard errors clustered by justice. Panels A, B, and C respectively report results using the median, minimum, and maximum clerk ideology for the justice-term.

A2.6 Stability of Clerk Ideology Over Time

Table A5: Clerk Ideology Before and After Clerking

	<i>Clerk Ideology After Clerkship</i>	
	(1)	(2)
Clerk Ideology Before Clerkship	0.970*** (0.039)	0.966*** (0.041)
Judge Ideology		0.016 (0.042)
Obs	158	158
R-squared	0.799	0.799
Dep Var Mean	-0.662	-0.662

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Sample includes clerks from all levels of Federal courts.