Influence and Ideology in the American Judiciary: Evidence from Supreme Court Law Clerks

Adam Bonica, Adam Chilton, Jacob Goldin, Kyle Rozema, & Maya Sen*

February 1, 2017

Supreme Court justices employ law clerks to help them perform their duties. We study whether these clerks influence how justices vote in the cases they hear. We exploit the timing of the clerkship hiring process to link variation in clerk ideology to variation in judicial voting. To measure clerk ideology, we match clerks to the universe of disclosed political donations. We find that clerks influence judicial voting, especially in cases that are high-profile, legally significant, or when justices are more evenly divided. We interpret these results to suggest that clerk influence occurs through persuasion rather than delegation of decision-making authority.

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

*Bonica: Stanford University, Department of Political Science, e-mail: bonica@stanford.edu. Chilton: University of Chicago Law School, e-mail: adamchilton@uchicago.edu. Goldin: Stanford Law School, e-mail: jsgoldin@law.stanford.edu. Rozema: Northwestern Pritzker School of Law, e-mail: kyle.rozema@law.northwestern.edu. Sen: Harvard University, John F. Kennedy School of Government, e-mail: maya_sen@hks.harvard.edu. For helpful comments, we are grateful to Chris Avery, Daniel Chen, Michael Frakes, Jonah Gelbach, Micheal Giles, William Hubbard, Claire Lim, Michael Munger, Crystal Yang, and seminar participants at Columbia University, Harvard University, UCLA, University of Chicago, the University of Texas, and the Conference on Empirical Legal Studies.

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; Kozinki and Bernstein, 1998) or may represent abberations 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 over 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. 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. As a result, changes in clerk ideology between two terms reflect changes in the justice's ideology during the *prior* two terms. In conjunction with the assumption that evolutions in judicial ideology are characterized by a random-walk process (with or without secular drift), this feature of the institution allows us to plausibly isolate the causal effect of clerk ideology on judicial voting behavior.

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. To do so, 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 continuous measure of ideological preferences as revealed by their actual donation decisions.

Our analysis provides strong evidence that clerk ideology does affect judicial voting behavior. Under our preferred specification, a standard devia-

tion change in clerk ideology is associated with approximately a 1 percentage point change in a justice's conservative vote share. To interpret the magnitude of this effect, our estimate suggests that, on average, a justice would cast approximately 4% more conservative votes in a term when employing his or her most conservative clerks, as compared to a term in which the justice employs his or her most liberal clerks. We find larger effects in cases that are higher profile (17%), cases that are legally significant (22%), and cases in which the justices are more evenly divided (12%). 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

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 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).[3] In part to attract the best talent, the justices normally hire

[1] In 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 (e.g., Woodward and Armstrong, 2005). Second, the papers of some deceased justices—most notably Justice Blackmun—have been later publicly released (e.g., Greenhouse, 2006). Third, a handful of clerks have written books or articles that recount their experiences working at the Supreme Court (e.g., Lazarus, 2005). Finally, some justices have publicly spoken or written about the inner workings of the court.

[3] Retired justices may hire one clerk. These clerks help the retired justice with their remaining workload—e.g. drafting opinions if the justice sits by designation on lower courts—for part of their time and are assigned to work for an active justice for the remainder of the

clerks well before their clerkship begins, typically at the beginning of the prior judicial term or earlier.

The specific responsibilities of 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.[4] There are at least two pathways for how such influence could occur: delegation and persuasion. First, justices may delegate a number of responsibilities to clerks, such as reading the briefs submitted by the parties, 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.[5] For example, a clerk may—consciously or unconsciously—emphasize the facts and arguments

time. We discuss how we account for the clerks that work for retired justices in Section 5.

[4]This debate has largely been motivated by prominent ancedotes and qualitative evidence. For example, there are a number of widely known stories about clerks having swayed justices' votes while clerking. See, for instance, 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, some justices' own behavior suggests that they believe clerks 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.

[5]A long literature in political science has considered how bureaucrats may use delegated authority to enact their own preferences instead of the relevant political leader (e.g., Kiewiet and McCubbins, 1991; McCubbins et al., 1989, 1987).

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 frequent 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. For example, one possibility is that justices learn about the personal lives of their clerks and are influenced via empathy (Glynn and Sen, 2015; Washington, 2008). Alternatively, it could be the case that 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. 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 j's voting in term t ($y_{j,t}$) 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.[6] 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 (e.g., Liptak, 2010; Bonica et al., 2016b).

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 specification 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.

To allow for idiosyncratic changes in judicial ideology from term to

[6] Because our focus is on the link between judicial and clerk ideology, Equation 1 abstracts away from other factors that could affect how a justice votes during a particular term, such as case composition or idiosyncratic case-preferences. Our empirical implementation below, however, includes term and issue-area fixed effects.

term, we model $d_{j,t}$ as a random walk with drift:[7]

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

where $g_j(t)$ is a justice-specific trend and where innovations in a justice's ideology ($\eta_{j,t}$) are assumed to be independent of justice and clerk ideology in the prior term. This requirement is stated formally as:

Assumption 1

$$\eta_{j,t} \perp\!\!\!\perp (d_{j,t-1},\, c_{j,t-1}) \tag{3}$$

Note that a standard fixed effects model of judicial ideology corresponds to the special case in which $g_j(t) = \eta_{j,t} = 0$.

To address the possibility that justices hire clerks based in part on the clerk's ideology, our empirical strategy relies on a useful institutional feature of the Supreme Court clerkship hiring process during our sample period: 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 July 2015 to June 2016 would likely be hired sometime in 2014.[8] It is this fact that permits identification of clerk influence given the non-random assignment of clerks to justices.

More formally, suppose that the clerks hired for justice j in term t are a function of the justice's ideology during the prior term (the term in which the clerk is hired):

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

[7] This 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. Note that this model is consistent with the possibility that a justice who votes particularly conservatively in one term would, ceteris paribus, vote more conservatively in the subsequent term as well.

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

In (4), $f(.)$ captures the potential dependence of a clerk's ideology on the justice's own ideology at the time the clerk is hired. In turn, $\nu_{j,t}$ is a residual term, capturing all other factors that shape which clerks a justice hires, such as the clerk's grades and law school attended.[9] Our identifying assumption will be that this residual variation in the clerks hired for term t is orthogonal to any innovations in judicial ideology that occur between term $t-1$ (when the clerks are selected) and term t (when the clerks begin employment):

Assumption 2

$$\nu_{j,t} \perp\!\!\!\perp \eta_{j,t} \tag{5}$$

Assumption 2 would fail if the term t clerks were hired based (in part) on the justice's term t ideology – for example, if justices could predict how their own ideology would evolve in future years and hired clerks on the basis of that evolution, rather than on their ideology at the time of the hire. In this case, $\eta_{j,t}$ would appear as part of $\nu_{j,t}$. Similarly, the assumption would be violated 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.[10]

Taken together, Assumptions 1 and 2 permit the unbiased identification of β in (1). In particular, taking first differences of the variables in (1) between consecutive terms, and applying (2), yields:

$$\Delta y_{j,t} = \beta\, \Delta c_{j,t} + g_j(t) + \eta_{j,t} \tag{6}$$

Because $\eta_{j,t}$ is unobserved, recovering β from (6) requires that $cov(\Delta c_{j,t},\, \eta_{j,t}) = 0$. From (4), $\Delta c_{j,t} = f(d_{j,t-1}) - f(d_{j,t-2}) + \nu_{j,t} - \nu_{j,t-1}$. Assumption 2 implies $cov(\nu_{j,t},\, \eta_{j,t}) = 0$, and Assumption 1 implies that the covariance of $\nu_{j,t}$ with

[9] Factors that depend on the justice's ideology in terms prior to $t-1$, such as the clerks employed by the justice during term $t-1$, would also appear in $\nu_{j,t}$.

[10] Another possibility is that an event could occur that simultaneously affects the ideology of the justice and a newly selected (but not yet employed) clerk. For example, this might be a significant political shock, such as an economic recession or terrorist attack, or the election of an unusual presidential candidate. 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.

the other components of $\Delta c_{j,t}$ is 0. Consequently, when Assumptions 1 and 2 hold, the econometric model we estimate below yields an unbiased estimate for β.

To summarize, our identifying assumption is that, after accounting for trends, the change in clerk ideology between two terms is uncorrelated with changes in justice ideology between those same two terms. The reason this assumption is plausible in our context is that clerks are hired well in advance of when the clerkship begins; as a result, changes in clerk ideology between two terms are likely to be correlated with changes in justice ideology during the *prior* two terms. And, if innovations in judicial ideology are well-described by a random walk process (after accounting for trends), changes in a justice's ideology between two terms will be uncorrelated with changes during the subsequent two terms.

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.[11]

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.[12]

[11] This excludes 4,848 votes coded as "unspecifiable."

[12] We report details of the matching process in the Technical Appendix.

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.[13]

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.

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

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

liberal justice always cast liberal votes.

Column 2 of Table 1 presents justice voting behavior at the justice-term level. We observe 439 justice-term combinations in our sample. The conservative vote shares in these terms range from 5.5% (Justice White in 1961) at the liberal end of the spectrum to 77.9% (Justice 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 lawyers is 40% (Bonica et al., 2015). As shown in Appendix Figure A1, most justice-terms are missing 0 to 1 clerks.[14]

Figure 1 presents the mean conservative vote share among Supreme Court justices over time.[15] 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.

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 1960s to 92 cases per term in the 2000s. 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

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

[15]Section A2.2 of the Supplementary Appendix presents information about voting by justice.

per term during the 2000s.

Finally, Figure 3 presents the ideological distribution of the Supreme Court clerks in our sample.[16] 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.

4 Results

4.1 Primary Results

In this section, we implement the empirical model based on the framework developed in Section 2. We first consider a specification with justice fixed effects:

$$y_{cjt} = \alpha + \beta\, c_{j,t} + \mu_j + \delta_t + \zeta\, X_c + \varepsilon_{cjt} \tag{7}$$

In (7), y indicates a conservative vote by justice j in term t on case c, $c_{j,t}$ 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.[17] 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 (7) 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.

[16] Section A2.3 of the Supplementary Appendix presents the ideological distribution of clerks by justice and the correlation between clerk and conservative vote share. 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).

[17] We consider alternative specifications using median, minimum, and maximum clerk ideology per term in Supplemental Table A3. Note that these specifications are robust to a hiring model in which justices seek to hire "counter-clerks" with ideologies opposite to their own.

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% change relative to the mean conservative vote share of 0.46.

Column 2 of Table 2 adds justice-specific linear time trends. This specification corresponds to setting $g_j(t) = g_j * t$. 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 specification, which exploits the timing of the Supreme Clerk hiring process to disentangle changes in judicial ideology from changes in clerk ideological composition. 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.[18] The figure suggests a moderate but positive-sloping relationship: larger increases in the degree of clerk conservativeness are associated with larger increases in the justice's conservative vote share.

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

$$\Delta y_{j,t} = \alpha' + \beta \, \Delta c_{j,t} + \delta'_t + \varepsilon'_{jt} \tag{8}$$

where $\Delta y_{j,t} = y_{j,t} - y_{j,t-1}$ and $\Delta c_{j,t} = c_{j,t} - c_{j,t-1}$. Note that (8) is estimated at the justice-term, rather than the case-justice-term, level. Estimating (8) 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

[18] Appendix Figure A4 plots clerk ideology and conservative vote share, in levels.

time trend, which we implement by adding a justice fixed effect to the first-differences specification in (8). The inclusion of a linear time trend is appropriate in the first-differences specification 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 the justice's annual set of most liberal clerks with a judicial chambers composed of the justice's annual set of most conservative clerks would result in a 4% increase in the number of conservative votes by the justice during the term.[19]

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: (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

[19] We obtain this estimate by computing the within-justice difference in mean clerk ideology between the term in which the justice employs his or her most liberal clerks and the term in which the justice employs his or her most conservative clerks. We then average this difference across justices. The reported percent change is obtained by multiplying the mean within-justice difference (1.96) by the point estimate (0.010), and scaling by the mean fraction of conservative votes (0.468).

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 the justices voting in line with their preferences.

Table 3 explores these possibilities by comparing clerk influence across various types of cases.[20] 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:

$$\Delta y_{jtu} = \alpha' + \beta\, \Delta c_{jt} + \gamma\, \pi_u\, \Delta c_{jt} + \pi_u + \delta'_t + \varepsilon'_{jt} \tag{9}$$

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 "major" by *Congressional Quarterly* in terms of its legal significance.[21] The results in Column 2 provide strong evidence

[20] Table A4 in the Supplemental Appendix presents summary statistics on the subgroups of cases analyzed in this section.

[21] While the *New York Times* measure captures the attention a case received when it was

that clerk influence is strongest in legally significant cases and virtually non-existent in other cases. Like Column 1, these results are difficult to explain 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. However, a delegation theory of influence would nonetheless predict that among non-unanimous cases, clerk influence would be greater in relatively uncontroversial cases as opposed to relatively controversial ones. 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. The results suggest that clerk influence is entirely driven by close cases. Overall, we interpret the results in Table 3 as being more consistent with persuasion models of influence than with delegation models of influence.

Figure 5 provides graphical evidence that supports the results in Table 3. The figure plots changes in a justices conservative vote share in consequential terms against changes in the ideology of their clerks stratified by whether cases meet a given criteria, as indicated in the title of each of the figures. It provides binned scatterplots, which reveal positive relationships for

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).

changes in cases that are higher profile, legally significant, and closely decided.

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 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 that clerks are most passionate about first amendment and civil rights issues, and so are most motivated to influence their justices on these types of cases. Alternatively, it may be that these are the issue areas 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.

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,[22] 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

[22]It 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) (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.

heavily in the opinion-writing process.[23] However, we do not wish to make 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.

Column 2 of Table 5 investigates clerk influence by variation in the extent to which the justice relies on the clerk during the writing process. For this analysis, we rely on a measure known as the "Vscore" developed by Rosenthal and Yoon (2011). To create the measure, Rosenthal and Yoon analyzed the text of justices' opinions to estimate each justices' variability in writing style from term to term. Higher Vscores are associated with higher variability, and as Rosenthal and Yoon argue, suggest higher reliance on clerks during the writing process. Our results are consistent with the hypothesis that the effect of clerk ideology increases for justices who rely more heavily on clerks to draft their opinions, but the estimated coefficient is imprecisely estimated.

Column 3 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. 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, Columns 4 and 5 of Table 5 investigates clerk influence by years of experience on the bench and justice age.[24] There may be concerns that justices overly rely on their clerks at the beginning of their career because of their limited experience or at the end of their career because of their advantaged age. The results provide no evidence that clerk influence varies over

[23]The estimated coefficient on the interaction term for clerk ideology and workload reported in 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.

[24]Because judicial experience and justice age are perfectly colinear with the justice fixed effects in this first difference specification, we do not include a main effect for experience in this analysis.

the course of a justices career.

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

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 6 presents a placebo test of the association between judicial voting in tern t and clerk ideology in each term during the five terms before and after term tern t, 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.

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.

To investigate this concern, we conducted a survey of former Supreme Court clerks.[25] Specifically, we randomly selected a sample of 10% of the clerks in our dataset and searched for those clerks' email addresses using the information from our dataset. We were able to successfully identify the email addresses of 102 former clerks (65% of our random sample). We then emailed these clerks and asked when they were offered their clerkship and when they started their clerkship. In total, 66 people responded to our survey, of which 62 were able to provide some information about when they were offered their clerkship. The mean hiring date was 10 months before the clerkship began. In addition, 25% of respondents reported having been hired over 12 months prior to when their clerkship began, and 80% reported having been hired prior to the beginning of the calendar year in which their clerkship began.

Because it appears that many clerks are hired after the start of the prior year's term, we consider a restriction of our analysis to cases that are decided relatively late in the term, after the subsequent year's clerks are likely to have been hired. Panel A of Table 6 implements this robustness check by replicating Table 2 while restricting the sample to cases argued between

[25]Section A2.6 of the Supplementary Appendix presents more information about this survey.

January through June.[26] 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 of Table 6 restricts the analysis to the second half of our sample. The results provide evidence that our main findings are not driven by a bias in the early years.

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 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

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

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 0.020, 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 from our baseline specification.

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.[27] For this analysis, we use the Judicial Common Space (JCS) ideology measure of the prior judge.[28] 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 Reflects 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 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 into missingness in the data and could also affect the 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-

[27] Kromphardt (2015) employs a similar approach to measure clerk ideology.

[28] JCS 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).

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, $c^U_{jt} = \rho\, c_{jt}$, where c^U_{jt} 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

$$c^*_{jt} = (1 - \mu)\, c_{jt} + \mu\, c^U_{jt}$$

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.[29] 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

[29] The Technical Appendix provides this calculation.

estimated coefficients towards zero but is unlikely to be driving our finding of a non-zero effect.

5.3 Changes in Clerk Ideology

A third potential issue 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 those in the year of the clerkship. This section investigates whether changes in clerk ideology over time may bias our results.

First, an initial concern is that contributions made later in life may not reflect the ideology of the clerk at the time of their clerkship. To explore this possibility, we tested the robustness of our results by using a measure of ideology that is based on donations in a limited time window after the clerkship. Instead of measuring a clerk's ideology based on the clerk's contributions over his or her lifetime, the analysis in Supplemental Table A5 is based on the clerk's contributions that occur within 5 years of the clerkship. Presumably, such contributions are a better reflection of the clerk's ideology during the clerkship than are contributions made later in life. The estimated effect of clerk ideology resulting from this analysis is slightly greater than the estimated effect from our baseline specification and, despite the smaller sample size, is more precisely estimated.

Second, another concern is that the justice a clerk works for 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.[30] Because our identification strategy exploits within-justice variation in clerk ideology, endogenous ideological evolution among clerks is unlikely to be generating our results. That is, one 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

[30] As we demonstrate in this section, we find no evidence that such a pattern is driving our results. However, as we have shown, we find evidence of the reverse relationship—that clerks influence justices. This tension can be explained by the fact that we are not examining the influence of clerks on the justices' ideologies, but, rather, the influence of clerks on the justices' voting on specific cases and during specific terms. Whether clerks influence justices' ideological leanings over time is an issue we leave to future research.

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).

To investigate the possibility that justices influence clerk ideology, we take advantage of 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 of this analysis, presented in Supplemental Table A6, 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 the clerk's subsequent ideological evolution. One caveat to this result, however, is that it's quite possible that clerks that donate before their clerkship have more fixed ideologies than clerks that only donate after their clerkship.

Third, a final concern is 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. Although theoretically possible, this mechanism seems unlikely to be large enough to be responsible for much of our estimated effect, given the observed stickiness of political ideology. That is, to explain our effect, it would need to be the case that working on a slightly more conservative set of cases in one term (approximately 1%) could shift clerks' ideologies by one standard deviation—a dramatically larger effect than is associated with moving from liberal to conservative cities (Bonica, 2016), large fluctuations in wealth (Bonica and

Rosenthal, 2016), or changes in ideology by age (Ghitza and Gelman, 2014).[31]

As an additional check that reverse causation is not driving our results, we constructed an instrument for clerk ideology based on characteristics of the clerk that are fixed prior to the beginning of the Supreme Court clerkship, namely, law school, judge of prior-clerkship, and clerk gender. The instrument is valid if these characteristics do not affect judicial voting apart from their association with clerk ideology. To construct the instrument, we regressed clerk CFscores on indicators for top 14 law schools, indicators for court of appeals judges who sent at least than two clerks to the Supreme Court, and gender. In particular, we ran separate regressions for each clerk, where we exclude the clerk in question from the regression, and then obtain a predicted CFscore of the excluded clerk's ideology. We then use the predicted CFscore as an instrument for observed clerk ideology in a two-stage least squares regression. The point estimate from this analysis is 0.012, similar to our preferred specification, but the standard error increases substantially relative to the non-IV analysis, so that the coefficient is not statistically different from 0. The results are reported in Table A7 in the Supplementary Appendix.

5.4 Retired Justices' Clerks

A unique institutional feature of the Supreme Court is that retired justices are able to hire one clerk each year, and these clerks are often assigned to spend part of their time working for an active justice that did not directly hire them. It is possible the presence of these clerks in an active justices' chamber may influence their voting. To investigate whether this institutional feature biases our results, we searched for each clerk employed by a retired justice to find the other justice they worked for doing the term. By looking up the clerks' professional biographies, we were able to identify the active justice for 66 of the 93 clerks that were employed by retired justices. We then re-

[31] Although some studies have found ideological evolution among Supreme Court *justices* over the course of their careers, there is little reason to believe that clerks' ideologies evolve in a similar fashion, given that unlike justices, clerks are not forced to confront the same ideological issues over and over again during the course of their careers.

estimated our primary results while assigning these clerks to the active justice they spent part of their time working for. As Supplemental Table A8 shows, doing so does not substantively change our primary results.

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 above, 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 contributed 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 nor 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.

References

Baier, P. (1973). The Law Clerks: Profile of an Institution. *Vanderbilt Law Review 26*(2), 1125–1177.

Baker, S. and C. Mezzetti (2012). A Theory of Rational Jurisprudence. *Journal of Political Economy 120*(3), 513–551.

Barnett, R. E. (2012). The Disdain Campaign. *Harvard Law Review Forum 126*(1), 5–9.

Bonica, A. (2014). Mapping the Ideological Marketplace. *American Journal of Political Science 58*(2), 367–386.

Bonica, A. (2016). Avenues of Influence: On the Political Expenditures of Corporations and Their Directors and Executives. *Working Paper*.

Bonica, A., A. Chilton, J. Goldin, K. Rozema, and M. Sen (2016a). Measuring Judicial Ideology Using Clerk Hiring. *American Law and Economics Review* (Forthcoming).

Bonica, A., A. Chilton, J. Goldin, K. Rozema, and M. Sen (2016b). The Political Ideologies of Law Clerks. *American Law and Economics Review* (Forthcoming).

Bonica, A., A. Chilton, and M. Sen (2015). The Political Ideologies of American Lawyers. *Journal of Legal Analysis* (Forthcoming).

Bonica, A. and H. Rosenthal (2016). Increasing Inequality in Wealth and the Political Consumption of Billionaires. *Working Paper*.

Bonica, A. and M. Sen (2016). A Common-Space Scaling of the American Judiciary and Legal Profession. *Political Analysis* (forthcoming).

Cameron, C., J. Gelbach, and D. Miller (2011). Robust Inference with Multiway Clustering. *Journal of Business and Economic Statistics 28*(2), 238–249.

Cameron, C. and D. Miller (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources 50*(2), 317–372.

Carlson, K., M. Livermore, and D. Rockmore (2016). A Quantitative Analysis of Writing Style on the U.S. Supreme Court. *Washington University Law Review 93*(6), 1–62.

Choi, S. and G. Gulati (2005). Which Judges Write Their Opinions (And Should We Care?). *Florida State University Law Review 32*, 1077–1122.

Clark, T. (1959). Internal Operation of the United States Supreme Court. *Journal of American Judicial Society 43*, 45–52.

Crawford, J. (2012). Roberts Switched Views to Uphold Health Care Law. *CBS News, July 2, 2012*.

Epstein, L., D. Ho, G. King, and J. Segal (2005). The Supreme Court During Crisis: How War Affects Only Non-War Cases. *New York University Law Review 80*(1), 1–116.

Epstein, L., W. Landes, and R. Posner (2013). *The Behavior of Federal Judges: A Theoretical and Empirical Study of Rational Choice*. Harvard University Press.

Epstein, L., A. Martin, K. Quinn, and J. Segal (2007). Ideological Drift Among Supreme Court Justices: Who, When, and How Important? *Northwestern University Law Review 101*(4), 1483–1542.

Epstein, L., A. Martin, J. Segal, and C. Westerland (2007). The Judicial Common Space. *Journal of Law, Economics, and Organization 23*(2), 303–325.

Epstein, L. and J. A. Segal (2000). Measuring Issue Salience. *American Journal of Political Science 44*(1), 66–83.

Fowler, J., T. Johnson, J. Spriggs, S. Jeon, and P. Wahlbeck (2007). Network Analysis and the Law: Measuring the Legal Importance of Precedents at the U.S. Supreme Court. *Political Analysis 15*(3), 324–346.

Garrow, D. (2014). How *Roe v. Wade* Was Written. *Washington and Lee Law Review 71*(2), 893–923.

Gennaioli, N. and A. Shleifer (2007). The Evolution of Common Law. *Journal of Political Economy 115*(1), 43–68.

Ghitza, Y. and A. Gelman (2014). The Great Society, Reagan's Revolution, and Generations of Presidential Voting. *Working Paper*.

Glynn, A. and M. Sen (2015). Identifying Judicial Empathy: Does Having

Daughters Cause Judges to Rule for Women's Issues? *American Journal of Political Science 59*(1), 37–54.

Green, D. P., B. Palmquist, and E. Schickler (2004). *Partisan Hearts and Minds: Political Parties and the Social Identities of Voters*. Yale University Press.

Greenhouse, L. (2006). *Becoming Justice Blackmun: Harry Blackmun's Supreme Court Journey*. Times Books.

Hutchinson, D. (1998). *The Man Who Once Was Whizzer White: A Portrait of Justice Byron R. White*. Free Press.

Iaryczower, M. and M. Shum (2012). The Value of Information in the Court: Get It Right, Keep It Tight. *American Economic Review 102*(1), 202–37.

Kiewiet, D. and M. McCubbins (1991). *The Logic of Delegation*. University of Chicago Press.

Kozinki, A. and F. Bernstein (1998). Clerkship Politics. *The Green Bag 2*(1), 57–441.

Kromphardt, C. (2015). U.S. Supreme Court Law Clerks as Information Sources. *Journal of Law and Courts 3*(2), 277–304.

Lazarus, E. (2005). *Closed Chambers: The Rise, Fall, and Future of the Modern Supreme Court Paperback*. Penguin Books.

Li, W., P. Azar, D. Larochelle, P. Hill, J. Cox, R. Berwick, and A. Lo (2013). Using Algorithmic Attribution Techniques to Determine Authorship in Unsigned Judicial Opinions. *Stanford Technology Law Review 16*(3), 503–534.

Lim, C. (2013). Preferences and Incentives of Appointed and Elected Public Officials: Evidence from State Trial Court Judges. *American Economic Review 103*(4), 1360–1397.

Lim, C., J. Snyder, and D. Stromberg (2015). The Judge, the Politician, and the Press: Newspaper Coverage and Criminal Sentencing across Electoral Systems. *American Economic Journal: Applied Economics 7*(4), 103–35.

Liptak, A. (2010). A Sign of the Court's Polarization: Choice of Clerks. *New York Times, Sept. 6, 2010*.

Martin, A. and K. Quinn (2002). Dynamic Ideal Point Estimation via Markov Chain Monte Carlo for the U.S. Supreme Court, 1953-1999. *Political Analysis 10*(2), 134–153.

Maskin, E. and J. Tirole (2004). The Politician and the Judge: Accountability in Government. *American Economic Review 94*(4), 1034–1054.

McCubbins, M., R. Noll, and B. Weingast (1987). Administrative Procedures as Instruments of Political Control. *Journal of Law, Economics, and Organization 3*, 243–277.

McCubbins, M., R. Noll, and B. Weingast (1989). Structure and Process, Politics and Policy: Administrative Arrangements and the Political Control of Agencies. *Virginia Law Review 75*, 431–482.

Newland, C. (1961). Personal Assistants to Supreme Court Justices: The Law Clerks. *Oregon Law Review 40*(1), 299–317.

Peppers, T. (2006). *Courtiers of the Marble Palace: The Rise and Influence of the Supreme Court Law Clerk*. Stanford Law and Politics.

Peppers, T. (2012). Of Leakers and Legal Briefers: The Modern Supreme Court Law Clerk. *Charleston Law Review 7*(1), 95–110.

Peppers, T. and C. Zorn (2008). Law Clerk Influence on Supreme Court Decision-Making. *DePaul Law Review 58*(3), 51–78.

Posner, R. (1983). Will the Federal Courts of Appeals Survive until 1984: An Essay on Delegation and Specialization of the Judicial Function. *Southern California Law Review 56*(3), 761–791.

Rehnquist, W. (1957). Who Writes Decisions of the Supreme Court. *U.S. News & World Report, December, 13, 1957*.

Rosenthal, J. and A. Yoon (2011). Detecting Multiple Authorship of United States Supreme Court Legal Decisions Using Function Words. *Annals of Applied Statistics 5*, 283–.

Rozema, K. and S. Peng (2015). The Role of External Referrals in Hiring: Evidence from Judicial Law Clerks. *Working Paper*.

Spaeth, H., L. Epstein, A. Martin, J. Segal, T. Ruger, and S. Benesh (2015). Supreme Court Database, Version 2015, Release 01.

Stekhoven, D. and P. Buhlmann (2012). MissForest–Non-Parametric Missing Value Imputation for Mixed-Type Data. *Bioinformatics 28*(1), 112–118.

Toobin, J. (2007). *The Nine: Inside the Secret World of the Supreme Court*. Anchor.

Tribe, L. H. (1991). The Constitution in Cyberspace: Law and Liberty Beyond the Electronic Frontier. Keynote Address at the First Conference on Computers, Freedom, and Privacy [Accessed: 2016 08 22].

Wahlbeck, P., J. Spriggs, and L. Sigelman (2002). Ghostwriters on the Court? A Stylistic Analysis of U.S. Supreme Court Opinion Drafts. *American Politics Research 30*(2), 166–192.

Ward, A. and D. Weiden (2006). *Sorcerers' Apprenticies: 100 Years of Law Clerks at the United States Supreme Court*. New York University Press.

Washington, E. (2008). Female Socialization: How Daughters Affect Their Legislator Fathers. *American Economic Review 98*(1), 311–32.

Woodward, B. and S. Armstrong (2005). *The Brethren: Inside the Supreme Court*. Simon & Schuster.

Yang, C. (2015). Free at Last? Judicial Discretion and Racial Disparities in Federal Sentencing. *Journal of Legal Studies 44*(1), 75–111.

Table 1: Descriptive Statistics

	Conservative Vote		***Clerk Ideology***	
	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
Within-Justice St. Dev.	0.476	0.072	0.802	0.564
Obs.	66209	439	1155	498

Table 2: Influence of Clerk Ideology on Justice Voting

	Conservative Vote		***Δ Conservative Votes***	
	(1)	(2)	(3)	(4)
Clerk Ideology	0.017***	0.012***		
	(0.006)	(0.003)		
Δ Clerk Ideology			0.008*	0.010**
			(0.004)	(0.004)
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
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

	Major Case		***Controversial***	
	NYT	***CQ***	***Unanimous***	***Close***
	(1)	(2)	(3)	(4)
Δ Clerk Ideology	0.002	-0.003	0.021***	-0.005
	(0.007)	(0.012)	(0.007)	(0.006)
Major Case	-0.004	-0.004		
	(0.003)	(0.004)		
Major Case	0.039***	0.049**		
$\times$ Δ Clerk Ideology	(0.011)	(0.021)		
Unanimous Case			0.009***	
			(0.003)	
Unanimous Case			-0.028***	
$\times$ Δ Clerk Ideology			(0.009)	
Close Case				-0.005
				(0.004)
Close Case				0.037***
$\times$ Δ Clerk Ideology				(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***	0.009	0.011	0.034***	-0.007	-0.002
	(0.008)	(0.007)	(0.010)	(0.011)	(0.010)	(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

	Workload (1)	***Vscore*** (2)	***Ideology*** (3)	***Experience*** (4)	***Age*** (5)
Δ Clerk Ideology	-0.006	0.001	0.010**	0.015**	0.023
	(0.011)	(0.008)	(0.004)	(0.007)	(0.037)
Workload	0.018				
$\times$ Δ Clerk Ideology	(0.011)				
Vscore		0.015			
$\times$ Δ Clerk Ideology		(0.012)			
Liberal Justice			-0.001		
$\times$ Δ Clerk Ideology			(0.009)		
Experience				-0.000	
$\times$ Δ Clerk Ideology				(0.001)	
Age					-0.000
$\times$ Δ Clerk Ideology					(0.001)
N	404	404	404	404	404
R^2	0.518	0.517	0.515	0.516	0.535
Mean Conservative Votes	0.468	0.468	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. Vscore estimates each justices' variability in writing style from term to term, which has been used as a proxy for reliance on clerks during the writing process (Rosenthal and Yoon, 2011). 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

	Conservative Vote		**Δ *Conservative Votes***	
	(1)	(2)	(3)	(4)
A. *Cases After December*				
Clerk Ideology	0.017***	0.010*		
	(0.006)	(0.006)		
Δ Clerk Ideology			0.008	0.010*
			(0.005)	(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***	0.009		
	(0.005)	(0.008)		
Δ Clerk Ideology			0.011***	0.011**
			(0.004)	(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

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. Panel A restricts the sample to cases argued in January through June, after the subsequent year's clerk will almost always have been hired.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

	Fill in Missing With		
	Imputed	***JCS Score***	***Zeros***
	(1)	(2)	(3)
Δ Clerk Ideology	0.009*	0.013**	0.011*
	(0.005)	(0.005)	(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.

Figure 1: Conservative Votes by Term

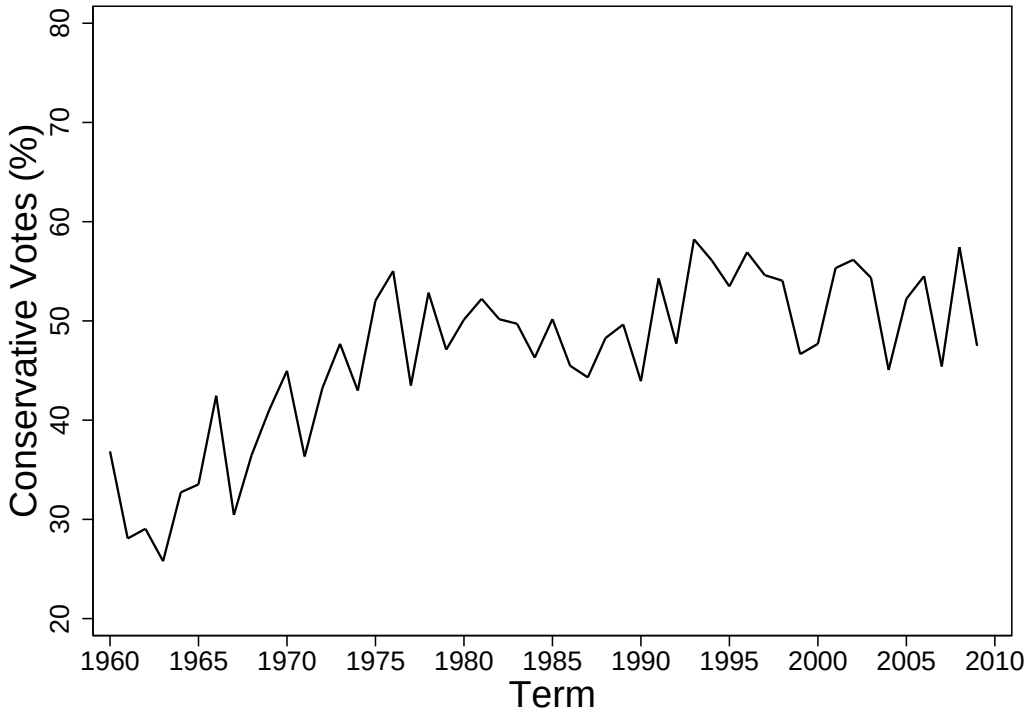


Figure 2: Number of Cases and Clerks By Term

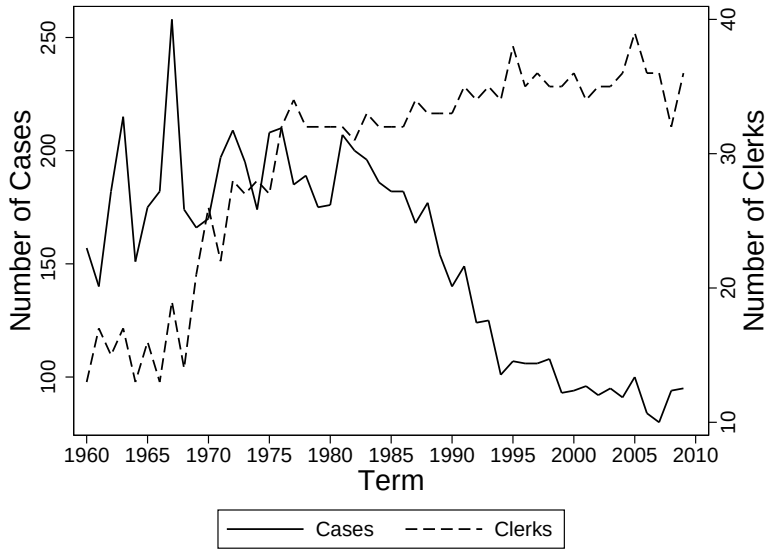


Figure 3: Supreme Court Clerk Ideology

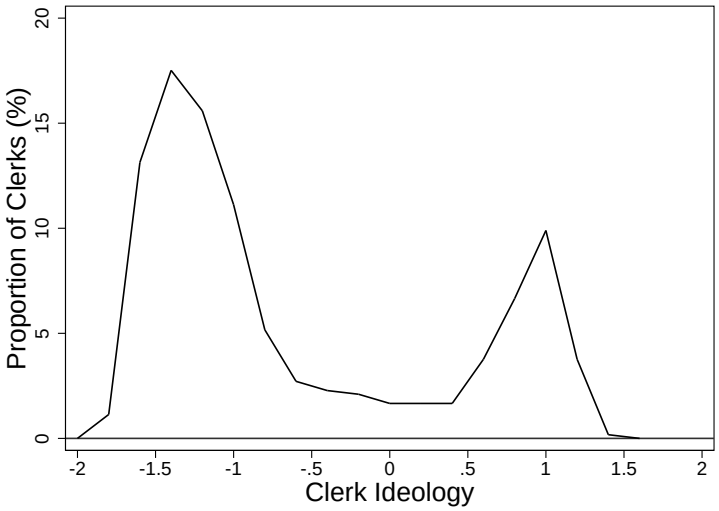


Figure 4: Influence of Clerk Ideology on Justice Voting

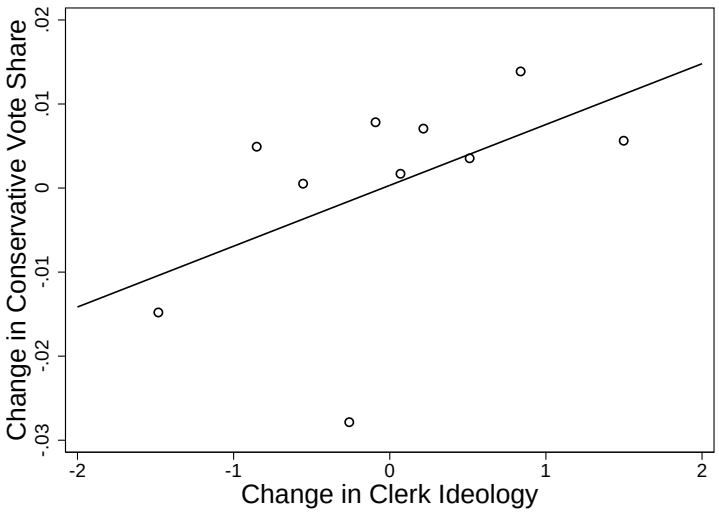


Figure 5: Influence of Clerk Ideology on Justice Voting

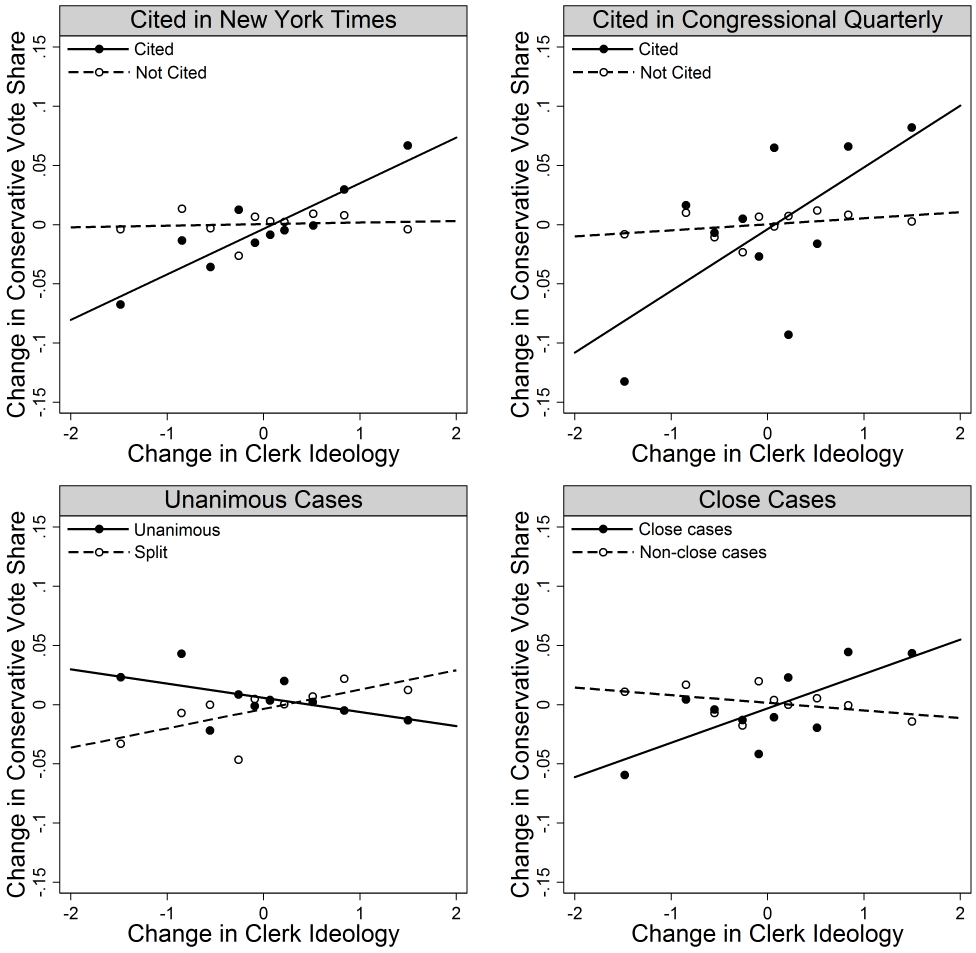
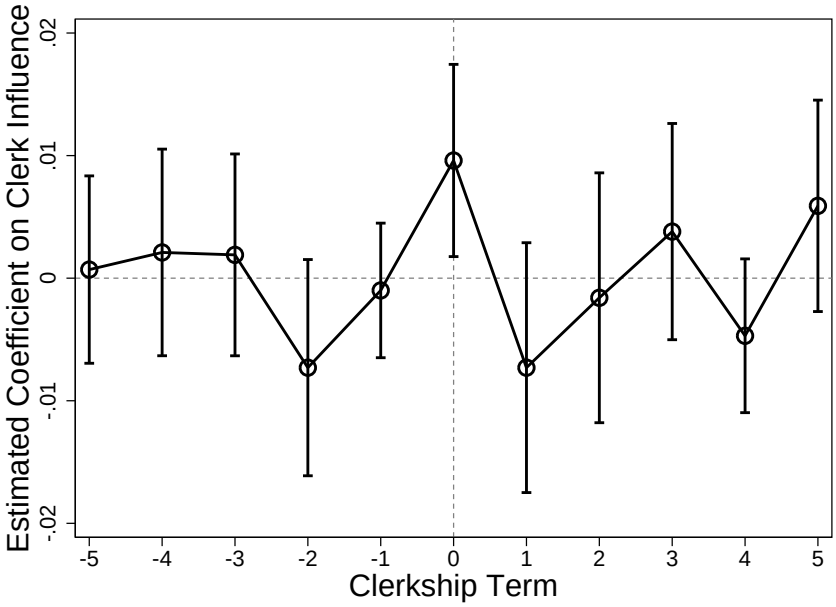


Figure 6: Placebo Tests



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

A1 For Online Publication: Technical Appendix

A1.1 Match Process

We matched the clerkship identity data to political donations 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 CF-score, (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,

$$c_{jt}^{U} = \rho \, c_{jt} \tag{10}$$

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 (10), we can re-write this to obtain:

$$c_{jt} = \frac{c_{jt}^{*}}{1 - \mu + \rho\mu} \tag{11}$$

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 (11):

$$y_{jt} = \beta(1 - \mu + \rho\mu) \, c_{jt} + \varepsilon_{jt} \tag{12}$$

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 For Online Publication: Supplementary Appendix

The Supplementary Appendix provides additional information on eight 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.

Fourth, Section A2.4 provide alternative results using alternative statistics of annual clerk CFscores. Table A3 reports the main results using the median, minimum, and maximum clerk CFscore as the primary independent variable.

Fifth, Section A2.5 provides details on conservative voting by subgroup. Table A4 reports the mean conservative vote share and the proportion of cases within subgroups used in the analysis in the paper.

Sixth, Section A2.6 provides details of the survey we conducted on the timing of Supreme Court clerk hiring. It also contains the wording of the email we sent to a random sample of clerks. Figure A5 provides the distribution of lag times between when a clerk reported having been hired and when the clerkship began.

Seventh, Section A2.7 tests the stability of clerk ideology over time. Table A5 reports the results of regressions restricting clerk donations to those that occurred within (or before) five years after the clerkship. Table A7 investigates reverse causation by constructing an instrument for clerk ideology. It reports the results of regressions using an instrument for clerk ideology based on characteristics of the clerk that are fixed prior to the beginning of the Supreme Court clerkship, namely, law school, judge of prior-clerkship, and clerk gender. Table A6 reports the results of regressions estimating the relationship between clerk ideology before clerking and after clerking.

Eighth, Section A2.8 provides alternative results when accounting for clerks for retired justices. Table A8 reports the main results when assigning clerks for retired justices' to the active justices the clerks worked for.

A2.1 Missing Ideology Data

Figure A1: Number of Clerks Unobserved by Justice-Term

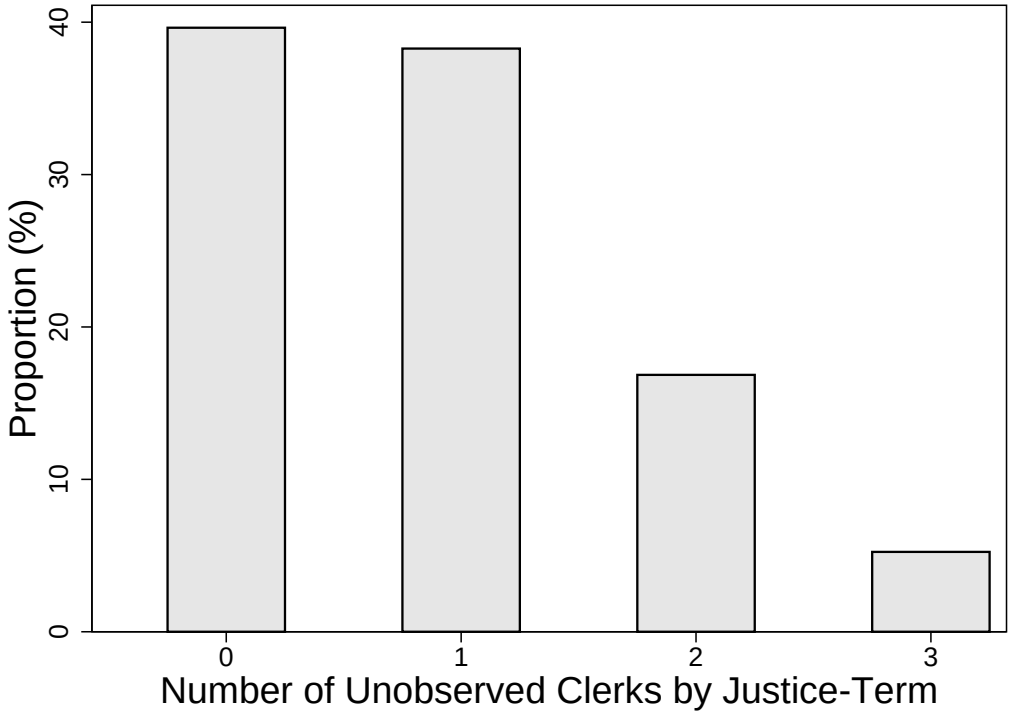


Table A1: Number of Law Clerks Matched to Donations Divided by Total Law Clerks by Justice-Term

	Justice																					
Term	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	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 Voting			***Clerks***		
Justice	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

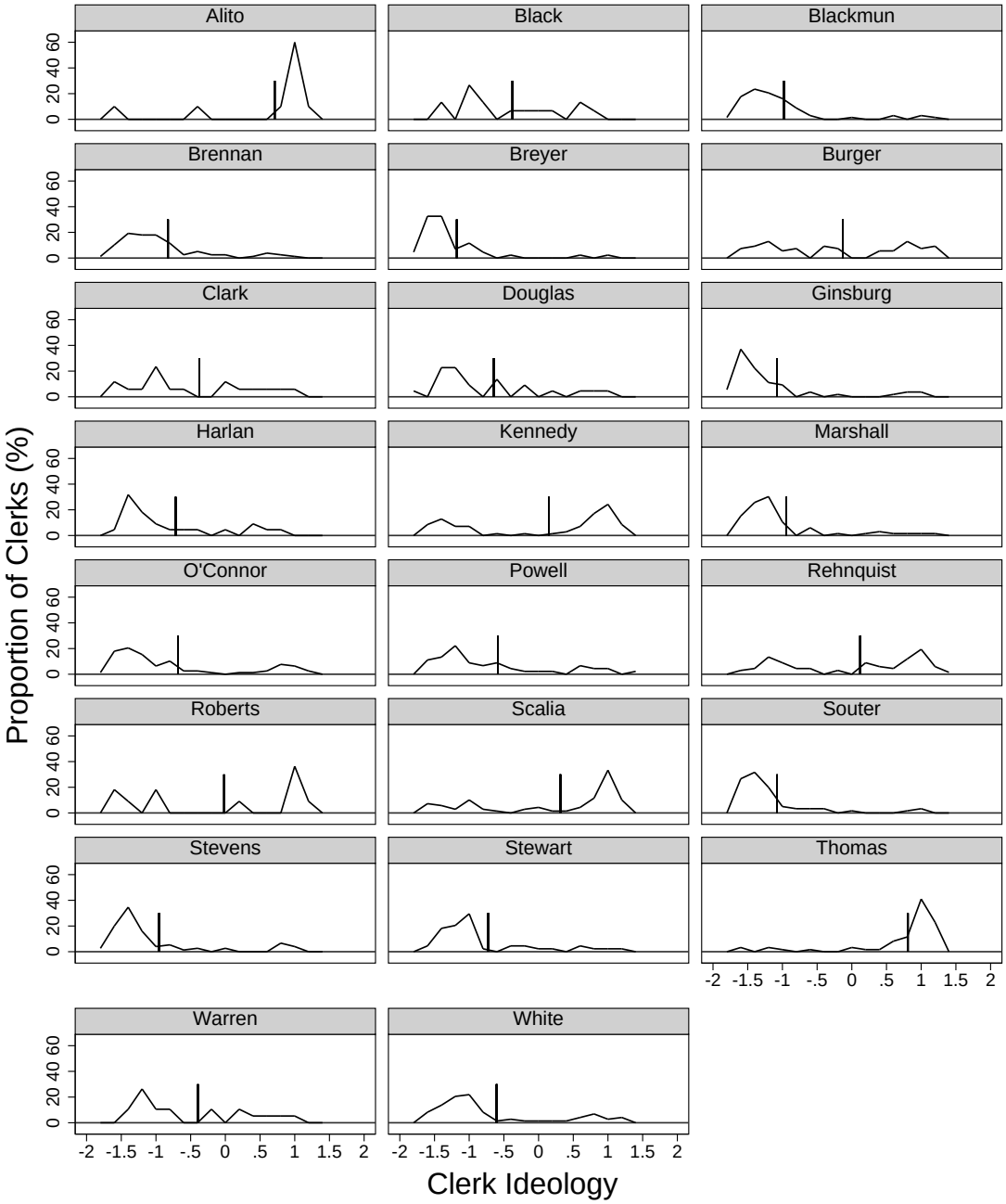


Figure A3: Scatterplot of Underlying Clerk Ideology by Justice Over Time

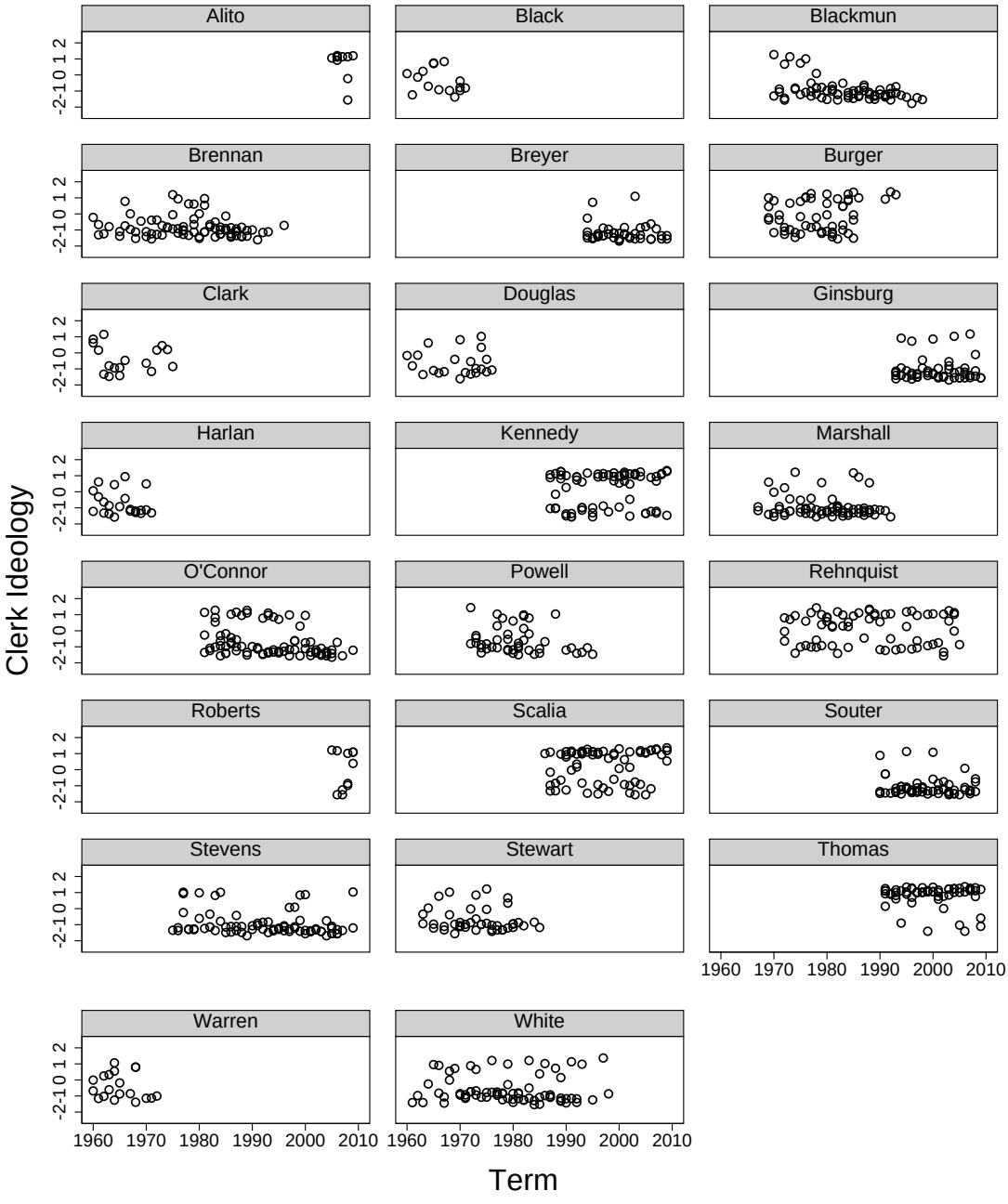
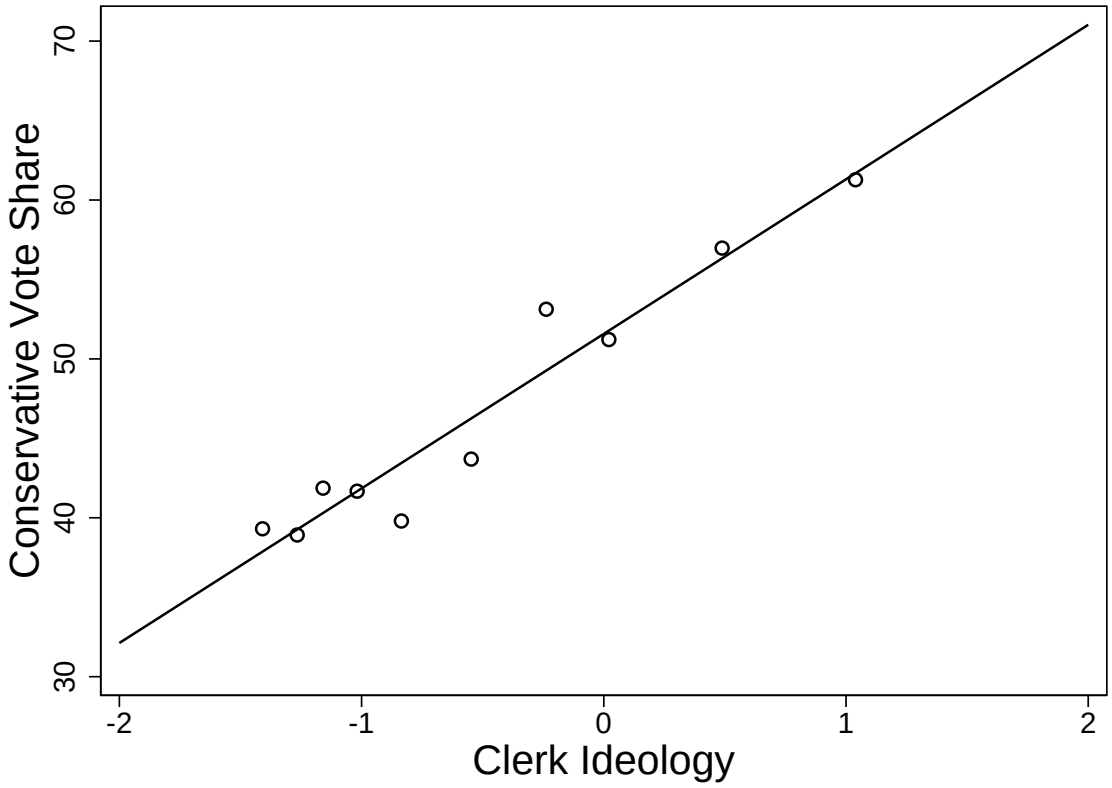


Figure A4: Relationship Between Justice-Term Level Mean Clerk CFscore and Percent Conservative Votes



A2.4 Alternative Ideology Statistics

Table A3: Influence of Clerk Ideology on Justice Votes Using Alternative Statistics

	Conservative Vote		***Δ Conservative Votes***	
	(1)	(2)	(3)	(4)
A. Median Justice-Year Clerk CFscore				
Clerk Ideology	0.014**	0.010		
	(0.006)	(0.009)		
Δ Clerk Ideology			0.007	0.008*
			(0.004)	(0.004)
R^2	0.129	0.134	0.509	0.513
B. Minimum Justice-Year Clerk CFscore				
Clerk Ideology	0.014***	0.013		
	(0.005)	(0.008)		
Δ Clerk Ideology			0.012***	0.013***
			(0.004)	(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.

A2.5 Conservative Votes by Subgroups

Table A4: Mean Conservative Vote Share and Proportion of Cases by Subgroups

	Conservative Vote Share (%)	***Proportion of Cases (%)***
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.

A2.6 Survey of Former Clerks

To verify that clerks are typically hired early in the prior term, we conducted a survey of former Supreme Court clerks. To do so, we randomly selected 10% of our sample of clerks. We then searched for the email addresses of the former clerks in this sample using the information in our dataset (e.g., their name, law school attended, justice they clerked for). Through this process, we identified the email addresses of 102 former clerks.

We then sent each of those clerks the email on the next page. In total, 66 former clerks responded to us. Of those, 4 respondents reported having no memory of when they were hired. The other 62 respondents provided information on when they were hired. Although some respondents provided a month they believed they were hired, roughly half of respondents provided a range of time. For example, respondents provided answers like: "in the fall of 1984"; "late 1999 or early 2000"; or "during the third year of law school, 1993-94".

Given the form of the responses, we took a conservative approach and established the earliest quarter the clerk reported to have been hired. For example, for most clerks who reported starting in July, we recored clerks hired in April to June of the year they started as one quarter prior to the start date; January to March as two quarters prior to the start date; October to December as three quarters prior to the start date; and any time earlier as four or more quarters prior to the start date. The distribution of responses are reported below in Figure A5.

Text of Email Used for Our Survey

Dear [Insert First Name],

I am [Redacted for Review], and I am currently conducting research on the Supreme Court. As part of that research, I am trying to understand when Supreme Court clerks were hired for their clerkship. It is my understanding that you are a former Supreme Court clerk. I was hoping you would be willing to answer two short questions for me:

1. What is the month and year that you were offered your Supreme Court clerkship?

2. What is the month and year that you started your Supreme Court clerkship?

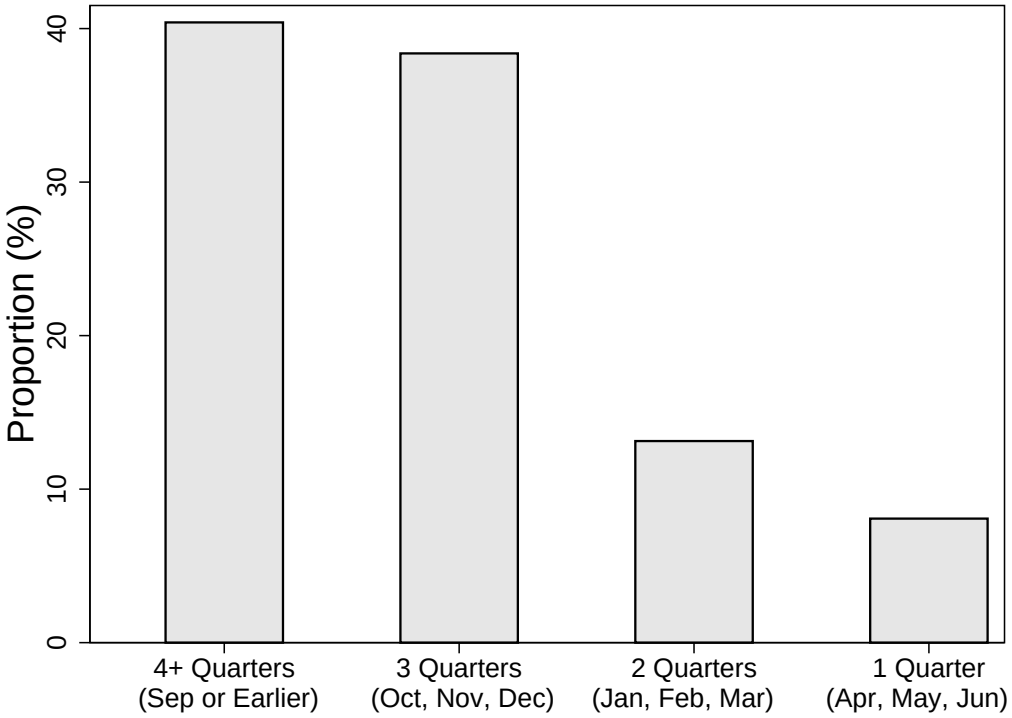
If you do not remember, any information would be helpful (as well as simply knowing that you do not remember). Your answers will be kept confidential. I am simply trying to document the average amount of time Supreme Court clerks are hired before they started, and will not in any way reveal personalized information.

Thank you for your time and help,

[Redacted for Review]

This research has been approved by the [University Redacted] Institutional Review Board. If you have any questions about participating in this research, you can contact the [Relevant Institutional Review Board] at [Contact Information Redacted].

Figure A5: Time Between the Hiring and Start Dates for Supreme Court Law Clerks



A2.7 Stability of Clerk Ideology Over Time

Table A5: Influence of Clerk Ideology on Justice Voting after Restricting Donations to those that Occurred Within Five Years after (or Before) the Clerkship

	Conservative Vote		***Δ Conservative Votes***	
	(1)	(2)	(3)	(4)
Mean Clerk CFscore	0.015**	0.011*		
	(0.007)	(0.005)		
Δ Mean Clerk CFscore			0.012***	0.012***
			(0.003)	(0.003)
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	No	Yes
Justice Time Trends	No	Yes	No	No
Obs	26,243	26,243	204	204
R-squared	0.112	0.114	0.564	0.564
Dep Var Mean	0.507	0.507	0.474	0.474

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 A6: Clerk Ideology Before and After Clerking

	Clerk Ideology After Clerkship	
	(1)	(2)
Clerk Ideology Before Clerkship	0.970***	0.966***
	(0.039)	(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.

Table A7: Influence of Clerk Ideology on Justice Voting using Predicted Clerk Ideology as an Instrument for Clerk Ideology

	Δ Mean Clerk Ideology	Conservative Vote
	First Stage	IV Regression
	(1)	(2)
Δ Mean Predicted Clerk Ideology	0.373**	
	(0.166)	
Δ Mean Clerk Ideology		0.012
		(0.027)
N	404	404
R-squared	0.142	0.534
Dep. Var. Mean	-0.014	0.468

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by justice. Results from a two stage least squares regression, where clerk ideology is instrumented for using a predicted value of clerk ideology based on the clerk's law school, previous judge, and gender.

A2.8 Accounting for the Retired Justices' Clerks

Table A8: Influence of Clerk Ideology on Justice Voting with Retired Clerk Reassignment

	Conservative Vote		***Δ Conservative Votes***	
	(1)	(2)	(3)	(4)
Mean Clerk CFscore	0.017**	0.012***		
	(0.007)	(0.004)		
Δ Mean Clerk CFscore			0.009*	0.010**
			(0.005)	(0.004)
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
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.