Legal Rasputins? Law Clerk Influence on Voting at the US Supreme Court

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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 exert modest influence on judicial voting overall, but substantial influence in cases that are high-profile, legally significant, or close decisions. We interpret these results to suggest that clerk influence occurs through persuasion rather than delegation of decision-making authority. (*JEL* K00, D72)

1. 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

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> 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 chambers of Supreme Court justices are notoriously secretive. As a result, key features of the Supreme Court—the central institution in the American judiciary—remain poorly understood. One perennial topic of interest about which little is known concerns the role played by Supreme Court law clerks. 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 an open question, however, is whether clerks, who are recent law school graduates, influence the actual votes cast by Supreme Court justices on the cases they hear. 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), but this view may be colored by clerks' exaggerated sense of their own importance in the process or may represent aberrations from the norm (Kozinski and Bernstein 1998; Toobin 2007).

Two methodological challenges have hindered efforts to study whether law clerks influence judicial decision-making. The first challenge is identification: 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 challenge relates to data: 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 how justices vote.

To deal with the fact that the assignment of clerks to justices is nonrandom 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 2 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 justices do not account for future, out-of-trend evolutions in their own political ideology when hiring clerks (either because such evolutions are unpredictable or because they do not reflect the justice's current ideological preferences), this feature of the institution allows us to plausibly isolate the causal effect of clerk ideology on judicial voting behavior.

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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 evidence that clerk ideology has a modest effect on judicial voting behavior, and that the degree of influence varies widely by the type of case. Under our preferred specification, a standard deviation change in clerk ideology is associated with approximately a 1 percentage point change in a justice's conservative vote share across all cases. 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, when compared with a term in which the justice employs his or her most liberal clerks. We find substantially 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.

Several recent papers have studied the role of clerks on the Supreme Court. Most notably, 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.[1] 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

1. Additionally, Black and Boyd (2012) studied the extent that Justice Harry Blackmun's clerks influenced whether 305 cases in four terms were granted review, and Kromphardt (2017) studied the extent that female clerks influence a justice's vote in sex discrimination and abortion cases. 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). Unlike our study, this line of research investigates clerk influence in the language of judicial opinions rather than whether clerks influence the substantive outcome of the case.

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measure of clerk ideology and by utilizing an identification strategy that is robust to changes in judicial ideology over time.

Our finding that law clerks influence voting at the Supreme Court sheds light on the political economy of the American judiciary in at least two ways. First, although one might expect the decisions of political principals across many domains to be influenced by the ideological leanings of their staff, there is good reason to think the same may not be true for judges. For instance, whereas political principals may hire and rely on staff precisely because of their expertise, judges uniformly have much more legal expertise than their clerks (as the epigraph from Justice Rehnquist suggests). Second, documenting the kinds of cases in which clerks influence how justices vote can provide insight into the judicial decision-making process itself. For instance, examining the types of cases in which clerks exert influence can help shed light on the process by which justices arrive at their decisions, such as by differentiating delegation and persuasion theories of influence (as described above).

The remainder of the paper proceeds as follows. Section 2 provides general background about Supreme Court clerkships and describes potential mechanisms by which clerks may influence judicial voting behavior. Section 3 describes our empirical strategy. Section 4 describes our data and provides descriptive statistics. Section 5 presents our baseline results as well as additional analyses. Section 6 conducts robustness checks relating to our identifying assumption, sample selection concerns, and the stability of clerk ideology over time. Section 7 concludes.

2. Background

Supreme Court clerkships are prestigious, 1-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). 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

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—for example, drafting opinions if the justice sits by designation on

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part to attract the best talent, the justices normally hire clerks well before their clerkship begins, typically at the beginning of the prior judicial term or earlier.

The specific responsibilities of clerks varies by justice and have evolved over time (Newland 1961; Baier 1973; Peppers 2006; Peppers and Zorn 2008). 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, writing the first drafts of opinions, and working with the justices to edit and polish the final drafts. The clerks work in close quarters with the justices and have frequent professional and social interactions with them, ranging from formal meetings before oral arguments to lunches and informal socializing. Through these formal and informal interactions, the clerks frequently discuss the merits of the cases with the justices, and often will try to persuade the justices about how they should vote or which arguments should be made in the opinions.

As we noted above, there is a running debate about 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 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

lower courts—for part of their time and are assigned to work for an active justice for the remainder of the time. We discuss how we account for the small number of clerks in our sample that work for retired justices in Section 6.

4. This debate has largely been motivated by prominent anecdotes and qualitative evidence. For example, there are a number of widely known stories about clerks having swayed justices' votes while clerking. See, for instance, Tribe (1991) discussing his role in swaying the outcome of *Katz v. United States*, 389 US 347 (1967), Garrow (2014) discussing the role of clerks in *Roe v. Wade*, 410 US 113 (1973), and Ward and Weiden (2006) discussing the influence of clerks on the outcome of *Planned Parenthood v. Casey*, 505 US 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., McCubbins et al. 1989, 1987; Kiewiet and McCubbins 1991).

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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 (Washington 2008; Glynn and Sen 2015). 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 that addresses this question below.

3. Empirical Framework

This section describes our empirical strategy for estimating the influence of clerk ideology on justice voting. 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:

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

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 $c_{j,t}$

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.

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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. 2017b).

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 Equation (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 at the end of her career compared with the beginning.

To allow for idiosyncratic changes in judicial ideology from term to term, we model $d_{j,t}$ as a non-stationary process that evolves in response to idiosyncratic innovations around a trend[7]:

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

where $g_j(t)$ is a justice-specific time 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 $g_j(t) = \eta_{j,t} = 0$. Even with the trend term, $g_j(t)$, Assumption 1 would be violated if a justice voting more conservatively in one term directly affected the propensity of the justice to vote conservatively in the subsequent term (e.g., to reduce cognitive dissonance or to avoid being perceived as an extremist).[8]

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

7. This is similar to the assumption made in other papers on judicial ideology, for example, Martin and Quinn (2002), who model the justices' ideologies with a random walk prior.

8. Another scenario in which Assumption 1 could fail is if a clerk influences the ideology of the justice who employs her, and this effect persists beyond the duration of the clerk's own term. A failure of the assumption along such lines would reduce the estimated effect of clerk ideology in the first-differences estimator described below; intuitively, the effect of a particularly liberal clerk who caused her justice to become more liberal in the subsequent term would be conflated with the subsequent term's (likely less liberal) clerk.

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2014.[9] 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):

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

In Equation (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.[10] 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.[11]

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

$$\Delta y_{j,t} = \beta \Delta c_{j,t} + g_j'(t) + \eta_{j,t}, \tag{6}$$

where $g_j'(t) = g_j(t) - g_j(t-1)$. Because $\eta_{j,t}$ is unobserved, recovering β from Equation (6) requires that $\text{cov}(\Delta c_{j,t}, \eta_{j,t}) = 0$. From Equation (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 $\text{cov}(\nu_{j,t}, \eta_{j,t}) = 0$ and Assumption 1 implies that the covariance of $\eta_{j,t}$ with the other

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

10. 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 $\nu_{j,t}$.

11. 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, 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.

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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 out-of-trend innovations in judicial ideology are sufficiently idiosyncratic (as formalized above), the change in a justice's ideology between two terms will be uncorrelated with changes during the subsequent two terms.

4. Data

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

4.1 Sources of Data

4.1.1 Supreme Court Vote Data.

Our outcome data are 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. (2015) 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.[12]

4.1.2 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.[13] We specifically match clerks to a measure of ideology based on these campaign finance donations developed by Bonica (2014) known as the "CFscore." The CFscore is calculated by first placing all candidates that receive donations on a unidimensional scale based on their share of common donors. The ideology of individual donors is then calculated based on the ideology of the candidates that they donate to, weighted by the share of the donations the individual gives to each candidate. The result is a measure of ideology that arranges individuals on a unidimensional ideological scale by Bonica (2014) from extremely liberal

12. This excludes 4848 votes coded as "unspecifiable."

13. The donations data include approximately 100 million political donations. We report details of the matching process in the Online Technical Appendix.

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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 US donors. The resulting CFscore reflects an individual's ideology to the extent the individual tends to make political donations to support candidates who share his or her political beliefs.[14]

There are two concerns with using these 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 6.2.

A second concern is that very few clerks donate before their clerkship. As a result, we construct our measure of clerk ideology from donations over the course of the clerk's life. We do so because a broad literature within political science suggests that most people have fairly stable partisan affiliations over their lifetimes (e.g., Green et al. 2004; Bonica 2014), and because using lifetime donations as a measure of ideology is consistent with prior research (e.g., Chilton and Posner 2015; Bonica and Sen 2017). However, 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 6.3 by, among other tests, calculating our ideology measure using only donations made within 5 years of the clerkship.

4.2 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% are coded as conservative. The standard deviation of votes cast captures both within- and between-variation in the voting behavior of individual justices. In contrast, the mean within-justice standard deviation captures the average degree of variation in the direction of votes cast by a single justice—for instance, this number would be zero if each conservative justice always cast conservative votes and if each liberal justice always cast liberal votes.

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.

Column 3 of Table 1 presents information on CFscores for all clerks in the sample. Column 4 of Table 1 presents information on the CFscores at

14. Bonica (2014, 2019) and Bonica and Sen (2017) provide evidence about the internal and external validity of using political donations as a measure of ideology.

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Table 1. Descriptive Statistics

	Judicial conservative vote		Clerk ideology	
	By vote (1)	By justice-term (2)	By clerk (3)	By justice-term (4)
Mean	0.458	0.468	−0.500	−0.521
Min	0	0.055	−1.775	−1.775
Max	1	0.779	1.440	1.376
Standard deviation	0.498	0.163	0.976	0.788
Within-justice Standard deviation	0.474	0.071	0.739	0.564
N	66,209	439	1149	437

the justice-term level, where clerk ideology for a given justice-term is the mean CFscore of the clerks working for the justice in the particular term. The total number of individuals clerking for the Supreme Court during our sample period is 1599, 72% of whom we match to an individual record in the political donations data. The donation rate among Supreme Court clerks is quite high compared with other groups; for comparison, the mean donation rate among all Americans is 5% and among lawyers is 40% (Bonica et al. 2016). As a result, we observe clerk ideology for 437 justice-term combinations in our sample. This is because, as shown in Online Appendix Figure A1, most justice-terms are missing 0 to 1 clerks.[15]

Figure 1 presents the mean conservative vote share among Supreme Court justices over time.[16] The figure is consistent with the common understanding that the Court was more conservative during the 1970s and 1980s (during the Burger Court) compared with 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 per term during the 2000s.

Finally, Figure 3 presents the ideological distribution of the Supreme Court clerks in our sample.[17] The mean CFscore is −0.50. To put this in

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

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

17. Section A2.3 of the Online 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. (2017a).

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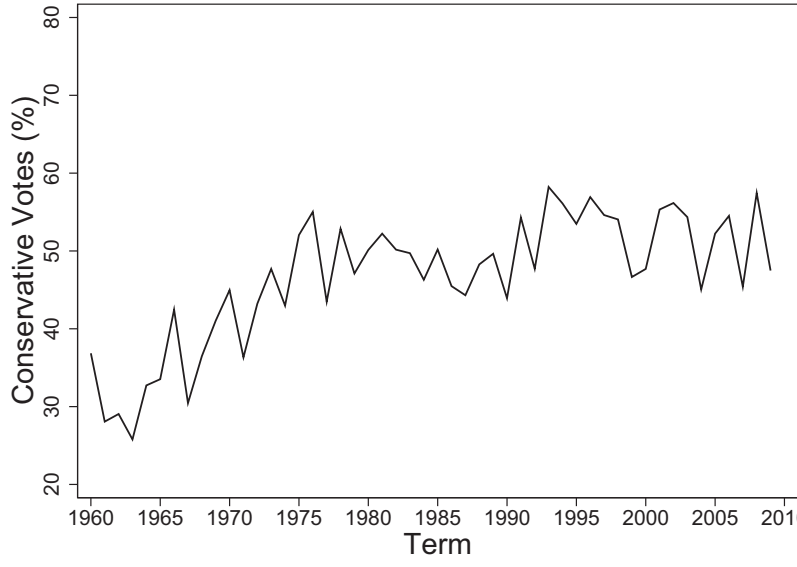


Figure 1. Conservative Votes by Term.

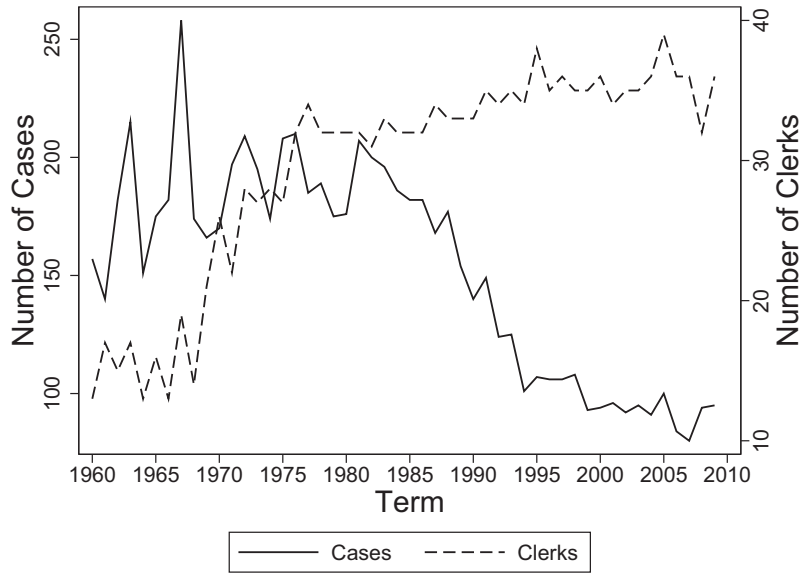


Figure 2. Number of Cases and Clerks by Term.

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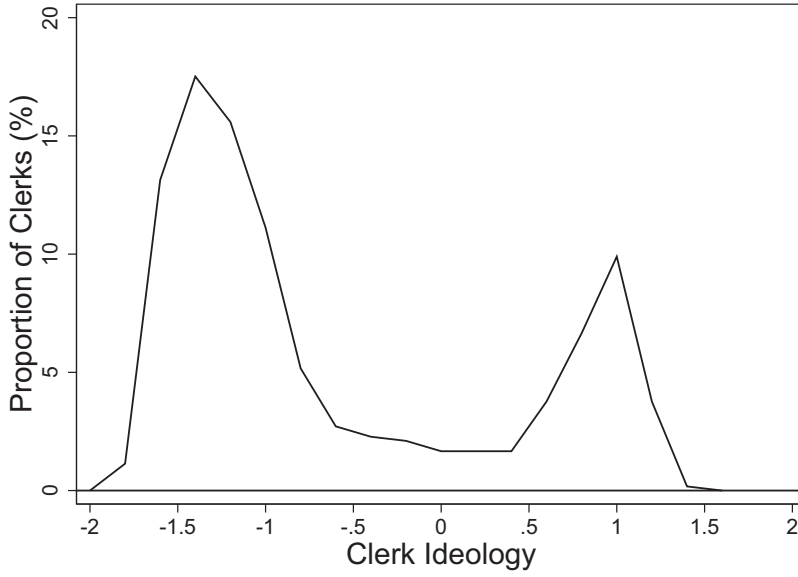


Figure 3. Supreme Court Clerk Ideology.

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.

5. Results

5.1 Primary Results

In this section, we implement the empirical model based on the framework developed in Section 3. We first consider a specification with justice-fixed effects:

$$y_{cjt} = \alpha + \beta c_{j,t} + \mu_j + \delta_t + \zeta X_c + \epsilon_{cjt}. \quad (7)$$

In Equation (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.[18] 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

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

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Miller (2015). As discussed in Section 3, estimating Equation (1) yields an unbiased estimate of β only if within-justice variation in ideology is uncorrelated with variation in the ideology of the clerks hired by the justice. Equation (7) is estimated at the case–justice-term level, which results in a sample of 66,209.

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 3, 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, as described in Section 3. 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. Panel A presents a scatterplot of all 404 justice-term level observations for which changes between terms in justice's conservative vote share and the ideology of their clerks are available, and Panel B presents a binned-scatter plot that groups the observations into deciles.[19] The graphical evidence in Figure 4 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:

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

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 Equation (2) is estimated at the justice-term, rather than the case–justice-term, level. This results in the number of observations dropping from 66,209 in Columns 1 and 2 to 404 for Columns 3 and 4. Estimating Equation (2) yields a point estimate of 0.008, slightly smaller than the estimated effect reported in Column 2.

Finally, Column 4 of Table 2 incorporates a justice-specific linear time trend, which we implement by adding a justice-fixed effect to the first-differences specification in Equation (2). The inclusion of this justice-

19. Online Appendix Figure A4 plots clerk ideology and conservative vote share, in levels.

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Table 2. Influence of Clerk Ideology on Justice Voting

	Conservative vote		ΔConservative votes	
	(1)	(2)	(3)	(4)
Clerk ideology	0.017**	0.012***		
	(0.007)	(0.004)		
ΔClerk ideology			0.008**	0.010**
			(0.004)	(0.004)
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	No	Yes
Issue area FE	Yes	Yes	—	—
Justice time trends	No	Yes	No	No
N	66,209	66,209	404	404
R^2	0.129	0.134	0.500	0.515
Mean conservative votes	0.458	0.458	0.468	0.468

Notes: Standard errors in parentheses.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Columns (1) and (2), standard errors clustered by justice and case. Columns (3) and (4), standard errors clustered by justice.

fixed effect is appropriate in the first-differences specification if justices' ideologies drift over time in ways that vary by justice. As shown in Column 4, the point estimate increases slightly following this addition, to approximately 0.010.[20]

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.[21]

20. The results are qualitatively similar when the justice-specific time trend is assumed to be quadratic rather than linear (the estimated coefficient on clerk influence is 0.011 with a standard error of 0.004).

21. 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 compute the average across justices of this within-justice difference. The reported percent change is obtained by multiplying this quantity (1.96) by the point estimate (0.010), and scaling by the mean fraction of conservative votes (0.468).

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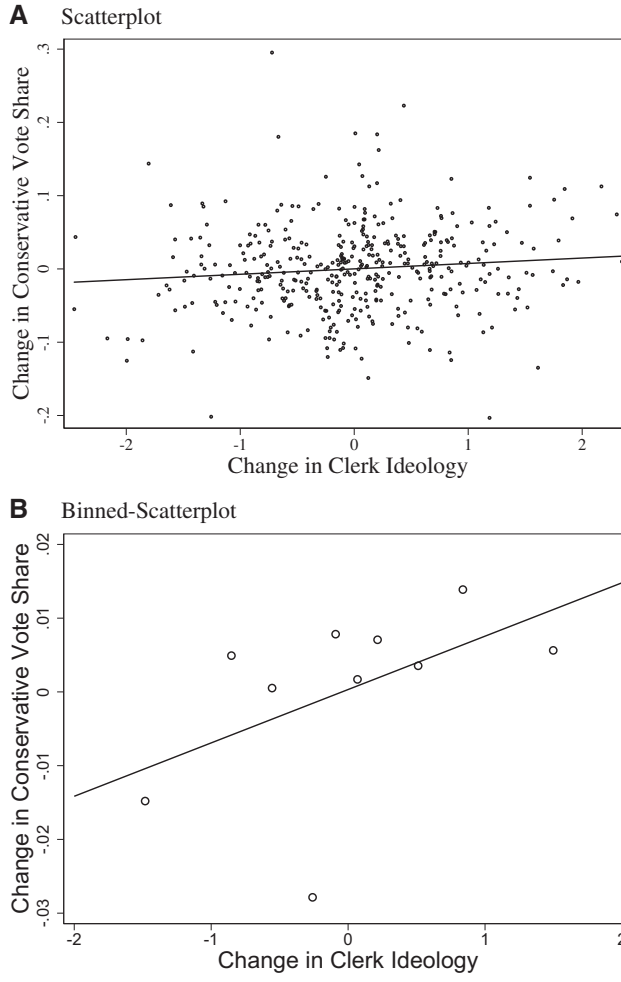


Figure 4. Influence of Clerk Ideology on Justice Voting.

5.2 Additional Results and Possible Mechanisms

As discussed in Section 2, 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 the most pronounced in the most important cases, where

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Table 3. Heterogeneous Effects: Case Attributes

	Major case		Controversial	
	NYT (1)	CQ (2)	Split (3)	Close (4)
ΔClerk ideology	0.001	−0.003	−0.008*	−0.005
	(0.007)	(0.012)	(0.004)	(0.006)
Major case	−0.004	−0.004		
	(0.003)	(0.004)		
Major case × ΔClerk ideology	0.039***	0.049**		
	(0.011)	(0.021)		
Split case			−0.009***	
			(0.003)	
Split case × ΔClerk ideology			0.029***	
			(0.009)	
Close case				−0.005
				(0.004)
Close case × ΔClerk ideology				0.037***
				(0.012)
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	Yes	Yes
N	807	807	808	806
R^2	0.261	0.286	0.371	0.249
Mean conservative votes	0.447	0.437	0.461	0.471

Notes: 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): Split indicates whether the decision was non-unanimous. Column (4): Close case indicates the vote on the decision was 5–4 or 6–3.

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.[22] 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. Because there are two observations for each justice-term, the number of observations for the regressions in Table 3 is roughly double the number

22. Table A4 in the Online Supplementary Appendix presents summary statistics on the subgroups of cases analyzed in this section.

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of observations of Column 4 of Table 2. The empirical specification is given by:

$$\Delta y_{jtu} = \alpha' + \beta \Delta c_{jt} + \gamma \pi_u \Delta c_{jt} + \pi_u + \delta'_t + \epsilon'_{jt}, \quad (9)$$

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 the issue it decides is legally significant. Column 2 repeats the analysis in Column 1, focusing on whether the legal significance of a case was classified as "major" by an expert survey conducted by *Congressional Quarterly*.[23] The results in Column 2 provide strong evidence that clerk influence is strongest in legally significant cases and virtually non-existent in other cases. Like Column 1, these results are difficult to explain with a delegation model of influence, but consistent with a model by which clerk influence operates through persuasion.[24]

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 clerks influence cases that are non-unanimous (e.g., "Split" decisions) as opposed to cases that are unanimous. The analysis shows that the estimated positive effect of clerk influence appears to be driven by cases with split decisions; for unanimous cases, our point estimate is actually negative but only marginally significant.

Although the finding that clerk influence is driven by split decisions 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

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

24. One potential concern with this interpretation is that the low-profile and legally insignificant cases may be straightforward to resolve and hence lack scope for the clerks to have any effect. However, restricting the analysis in Columns (1) and (2) to the subset of cases that are non-unanimous yields results that are similar to those reported in Table 3.

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delegation theory of influence would nonetheless predict that among cases with split decisions, 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 consecutive 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 changes in cases that are higher profile, legally significant, and closely decided.

Table 4 reports the results of our primary specification by issue area. Because the relevant comparison for this analysis is between cases from different issue areas, and not between cases in a given issue area and all other cases, Table 4 returns to using one observation from each justice-term. Each observation represents a justice's mean conservative vote share during the term for cases that fall into the specified 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 3.0 and 2.4 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.

Next, we turn from characteristics of the case to characteristics of the justice in determining the factors that shape clerk influence. We assess five justice attributes. First, we assess whether clerks exert more influence when they work for justices who have the clerk author first drafts of their opinions. To do so, 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,[25] this variable may proxy the degree of responsibility the

25. 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

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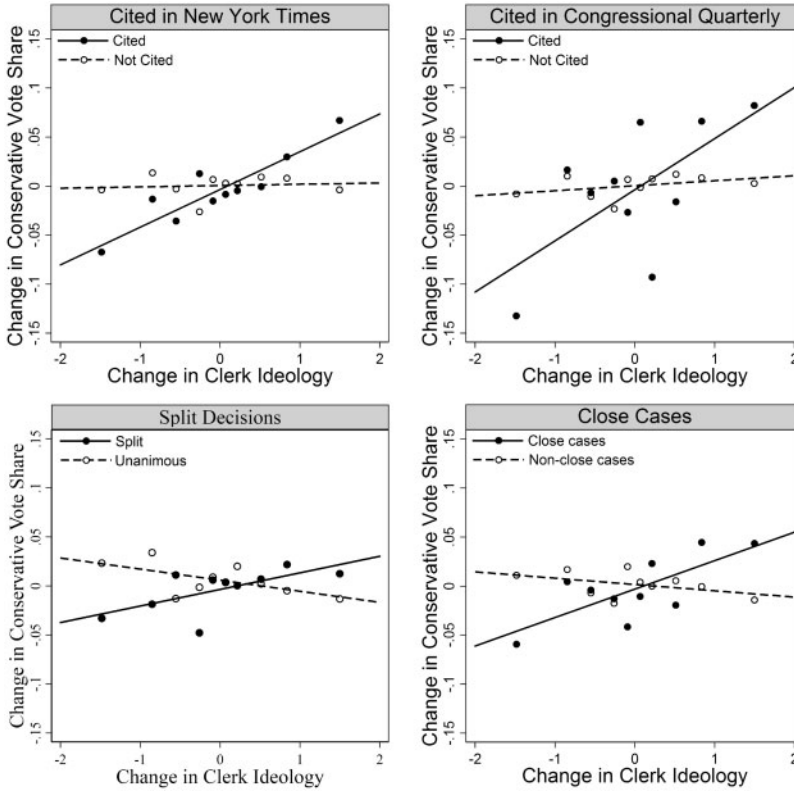


Figure 5. Influence of Clerk Ideology on Justice Voting.

Table 4. Heterogeneous Effects: Issue Areas

	Issue area					
	Civil rights (1)	Criminal procedure (2)	Economic activity (3)	First amendment (4)	Judicial power (5)	Other (6)
ΔClerk ideology	0.024***	0.006	0.010	0.030**	−0.008	0.000
	(0.008)	(0.007)	(0.010)	(0.011)	(0.010)	(0.009)
Covariates						
Term FE	Yes	Yes	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	Yes	Yes	Yes	Yes
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

Notes: 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.

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justice assigns to clerks generally, or, less charitably, the justice's own disengagement from the process. Second, we assess whether clerks exert more influence for justices that rely 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. Third, we assess whether clerks exert differential influence on liberal and conservative justices, as measured by whether the justice's conservative vote share is below the sample median. Fourth, we assess whether clerks exert more influence on justices by their years of experience on the bench. Fifth, we assess whether clerks exert differential influence on justices by the justices' age.[26]

Table 5 reports the results. There are three noteworthy findings. First, our results are consistent with the hypothesis that the effect of clerk ideology increases for justices who rely more heavily on clerks in the opinion-writing process, but the estimated coefficients are imprecisely estimated. Second, the results suggest that the effect of clerk influence on judicial voting is quite similar across justices on both sides of the ideological divide. Third, the results provide no evidence that clerk influence varies over the course of a justice's career, which might alleviate 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 advanced age.

6. Robustness Checks

6.1 Investigating Identifying Assumptions

As discussed in Section 3, 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.

US __ (2012) (Barnett 2012; Crawford 2012). In another example, future-judge Richard Posner apparently swayed the outcome of *Sanders v. US*, 373 US 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.

26. 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 and age.

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Table 5. Heterogeneous Effects: Justice Attributes

	ΔConservative votes
ΔClerk ideology	−0.016
	(0.049)
Workload × ΔClerk ideology	0.017
	(0.018)
Vscore × ΔClerk ideology	0.006
	(0.016)
Liberal justice × ΔClerk ideology	0.006
	(0.010)
Experience × ΔClerk ideology	−0.000
	(0.001)
Age × ΔClerk ideology	0.000
	(0.001)
Covariates	
Term FE	Yes
Justice FE	Yes
N	404
R^2	0.518
Mean conservative votes	0.468

Notes: 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 indicates an estimate of 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. Age indicates the age of the justice.

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 clerk's 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 term *t* and clerk ideology in each term during the two terms before and after term *t*, using our preferred specification (Column 4 in Table 2). The plotted coefficients are from a single regression that includes a vector of leads and lags of clerk ideology. 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 in both panels 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.

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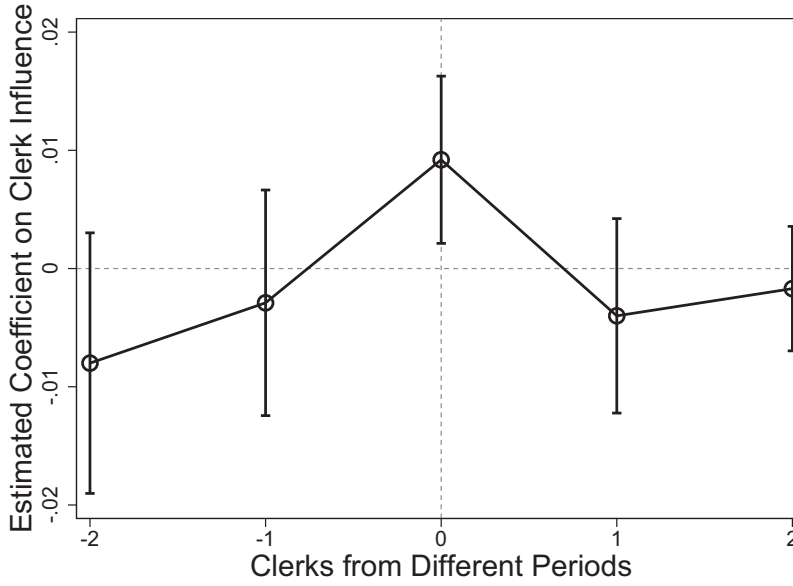


Figure 6. Placebo Test. *Note*: Bars reflect 90% confidence interval (clustered standard errors).

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 1 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.[27] 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

27. Section A2.6 of the Online Supplementary Appendix presents more information about this survey.

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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 by dropping cases argued between January through June.[28] 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.[29]

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. This reduces the number of justice-term observations to 204. The results provide evidence that our main findings are not driven by a bias in the early years.

6.2 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% 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.

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

29. An additional possibility addressed by this test is that the current term votes of a justice are influenced by that justice's prior-term clerks through the determination of which cases are granted certiorari. Although we do not observe the date at which cert is granted for a given case, the cases argued in January of a term or later are more likely to have been considered for cert by the current term's clerks. Note also that under our model, the fact that prior-term clerks influence current term votes through the cert process would not bias our results because the ideology of the prior-term clerks is a function of the justice's ideology from two terms prior, which, by assumption, is uncorrelated to subsequent evolutions in justice ideology.

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Table 6. Timing Assumptions

	Conservative vote		ΔConservative votes	
	(1)	(2)	(3)	(4)
A. Cases after December				
Clerk ideology	0.017***	0.011*		
	(0.006)	(0.006)		
ΔClerk 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	0.015***	0.010		
	(0.005)	(0.008)		
ΔClerk 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

Notes: 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.

6.2.1 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 cause 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 toward 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 are 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;[30] (3) generate predicted

30. We draw from the justice-level distribution rather than the justice-term level distribution to obtain a larger basis for sampling. A limitation of this approach is that it fails to

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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; and (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 1000 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.

6.2.2 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. (2017b), 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.[31] For this analysis, we use the Judicial Common Space (JCS) ideology measure of the prior judge.[32] 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.

account for changes in clerk hiring over the course of a justice's career. We repeated the analysis by drawing from the justice-year-level distribution and the algorithm converges on an estimated effect size of 0.025.

31. Kromphardt (2015) employs a similar approach to measure clerk ideology.

32. 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).

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Table 7. Investigating Selection

	Fill in Missing With		
	Imputed (1)	JCS Score (2)	Zeros (3)
ΔClerk ideology	0.009*	0.013**	0.011*
	(0.005)	(0.005)	(0.006)
Covariates			
Term FE	Yes	Yes	Yes
Justice FE	Yes	Yes	Yes
N	404	404	404
R^2	0.512	0.529	0.513
Mean conservative votes	0.468	0.468	0.468

Notes: 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 Online Technical Appendix for details of the imputation procedure. Column (2) reports the results using as a proxy for unobserved clerk ideology 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.

6.2.3 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-of-the-envelope adjustment to correct for this source of missingness. In particular, suppose that the mean ideology of the clerks who donate is ρ times greater than the ideology of the clerks who do not, $c_{jt}^{\mathrm{U}} = \rho c_{jt}$, where c_{jt}^{U} denotes the (unobserved) ideology of the clerks employed by justice j in term t, and $0 \leqslant \rho \leqslant 1$. In this case, the true ideology of the clerks hired in a justice-term, c_{jt}^{*}, is given by

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

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

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outcomes.[33] 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 $\rho \leqslant 1$, 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 are likely to moderately bias the size of our estimated coefficients toward zero but is unlikely to be driving our finding of a non-zero effect.

6.3 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—rather than clerk influence—may be driving 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 Table 8 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. This result is not surprising if changes in clerk ideology over time introduce classical measurement error into our measurement of the clerk's ideology in the year of the clerkship.[34]

33. The Online Technical Appendix provides this calculation.

34. The results for all the analyses in this paper are substantively the same when using the restricted sample from Table 8.

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Table 8. Influence of Clerk Ideology on Justice Voting After Restricting Donations to Those That Occurred within Five Years After (or before) the Clerkship

	Conservative vote		ΔConservative votes	
	(1)	(2)	(3)	(4)
Clerk ideology	0.016**	0.011*		
	(0.007)	(0.005)		
ΔClerk ideology			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
N	26,243	26,243	204	204
R^2	0.112	0.114	0.564	0.564
Mean conservative votes	0.507	0.507	0.474	0.474

Notes: 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.

Second, another concern is that the justice a clerk works for influences the evolution of the clerk's ideology in future years—for example, clerks who work for liberal justices might become more liberal after the clerkship.[35] 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 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. This data on clerks' identity were collected by Katz and Stafford (2010), who obtained information on clerks who worked for federal and district court judges during this period.

35. 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.

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They specifically collected the name, law school attended, and judge for 5057 circuit court clerks and 12,580 district court clerks. We used the same process to match these clerks to their political donations that we used for the Supreme Court clerks that we described in Section 4.1. The results of this analysis, presented in Supplementary Table A5, 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).[36]

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 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 clerk ideology in a justice-term as an instrument for

36. 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.

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Table 9. Influence of Clerk Ideology on Justice Voting Using Predicted Clerk Ideology as an Instrument for Clerk Ideology

	Mean clerk ideology First stage (1)	Conservative vote IV regression (2)
ΔPredicted clerk ideology	0.373**	
	(0.166)	
ΔClerk ideology		0.012
		(0.027)
Covariates		
Term FE	Yes	Yes
Justice FE	Yes	Yes
N	404	404
R^2	0.142	0.534
Dep. var. mean	−0.014	0.468

Notes: 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.

observed clerk ideology in the justice-term in a two-stage least squares regression. The results are reported in Table 9. 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.

6.4 Role of Clerk Gender

It is possible that the gender of a clerk directly influences how a justice votes, distinct from any effect stemming from the clerk's ideology (Kromphardt 2017). For example, having a higher fraction of female clerks may cause a justice to vote in a more liberal direction on cases associated with gender equality. Because female clerks are more likely to be politically liberal, it is possible that the effect we are attributing to clerk ideology is in actuality partially or entirely due to clerk gender. Table 10 investigates this possibility by estimating the first-difference specification in Column 4 of Table 2 and controlling for clerk gender. Columns 1 and 2 use the change in the percent of female clerks for all clerks in the justice-term, and Columns 3 and 4 use the change in the percent of female clerks for the sample of clerks whose political ideologies we observe. Columns 1 and 3 replace clerk ideology with clerk gender, and find a negative but impresiely estimate effect of the percent of female clerks. Columns 2 and 4 include both clerk ideology and clerk. In both columns, the effect of clerk ideology is similar to the effect estimated in the main specification, suggesting that gender is not confounding our estimates of the role of ideology.

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Table 10. Distinguishing Ideology and Gender

	Conservative vote			
	(1)	(2)	(3)	(4)
Percent of female clerks	−0.009	−0.005		
	(0.021)	(0.019)		
Percent of donating female clerks			−0.007	−0.006
			(0.013)	(0.013)
Clerk ideology		0.009**		0.009**
		(0.004)		(0.004)
Covariates				
Term FE	Yes	Yes	Yes	Yes
Justice FE	Yes	Yes	Yes	Yes
N	404	404	357	357
R^2	0.494	0.515	0.542	0.546
Mean conservative votes	0.468	0.468	0.474	0.474

Notes: Standard errors in parentheses.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors clustered by justice.

6.5 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 who were employed by retired justices. We then re-estimated our primary results while assigning these clerks to the active justice they spent part of their time working for. As Supplementary Table A6 shows, doing so does not substantively change our primary results.

7. Conclusion

In this article, we have studied whether law clerks affect how Supreme Court justices vote. We find that clerks exert a modest but statistically significant effect on how justices vote. Although the estimated effect is small overall, for major cases, and for cases where the justices are close to evenly divided, the influence of clerks is larger.

When interpreting our results, several factors suggest that 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

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suggests that accounting for that bias would inflate our estimated effect by up to 40%. To address this concern, one potential avenue for future research would be to account for the ideologies of clerks that have not made political donations by coding aspects of their resumes for ideological markers (e.g., membership in the *Federalist Society* or *American Constitution Society*).[37]

Second, our specifications assume that the influence of clerk ideology occurs at the justice–term level, with each clerk contributing 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.

Fourth, our finding that clerks influence justices through persuasion may suggest that justices are more susceptible to persuasion through other channels (e.g., oral argument, briefs) than is commonly believed. Alternatively, clerks may have a unique opportunity to persuade the justices given the institutional role they play and the amount of time they tend to spend with the justices.

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 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.

37. See Chilton and Posner (2015) for an example of research using this approach to code the ideologies of law professors.

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Supplementary Material

Supplementary material is available at *Journal of Law, Economics, & Organization* online.

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