Peer Effects on the United States Supreme Court

Richard Holden, Michael Keane and Matthew Lilley*

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Abstract

Using data on essentially every US Supreme Court decision since 1946, we estimate a model of peer effects on the Court. We consider both the impact of justice ideology and justice votes on the votes of their peers. To identify these peer effects we use two instruments that generate plausibly exogenous variation in the peer group itself, or in the votes of peers. The first instrument utilizes the fact that the composition of the Court varies from case to case due to recusals or absences for health reasons. The second utilizes the fact that many justices previously sat on Federal Circuit Courts. Those who served on the Circuit Courts for short (long) periods of time are empirically much more (less) likely to affirm decisions from their "home" court. We find large peer effects. Replacing a single justice with one who votes in a conservative direction 10 percentage points more frequently increases the probability that *each* other justice votes conservative by 1.6 percentage points. Further, a 10% increase in the probability that a given justice votes conservative leads to a 1.1 percentage point increase in the probability that each other justice votes conservative. This indirect effect increases the share of cases with a conservative outcome by 3.6 percentage points (excluding the direct effect of the new justice). In general, we find indirect effects are large relative to the direct mechanical effect of a justice's own vote.

*Holden: UNSW Business School, email: richard.holden@unsw.edu.au. Keane: UNSW Business School. Lilley: Harvard University Department of Economics, email: matthewlilley@fas.harvard.edu. We are grateful to Rosalind Dixon, John Friedman, Christine Jolls, Christopher Malloy, Emily Oster, Richard Posner, Jesse Shapiro, Andrei Shleifer, and Justin Wolfers for helpful discussions, and to seminar participants at Harvard, Harvard Law School and MIT.

1 Introduction

Economists have long been interested in the impact of one's social, educational, and workplace environment–and the characteristics of other agents in that environment–on one's own behavior and outcomes.[1] The presence of positive spillovers, or *peer effects*, in such settings would suggest a range of policy interventions that could improve educational and labor-market outcomes. However, the problem of identifying peer effects in the educational and workplace environments where they have typically been studied are formidable. There are several reasons for this.

First, as emphasized by Manski (1993), even identifying the relevant peer group is extremely difficult in most settings. Take, for example, the educational setting, and the question of how one's "peers" affect school performance. Who exactly are a student's peers? All other students in a particular class? All other students in the school (or grade)? Or are the relevant "peers" the subset of classmates who are the student's friends?, or who the student studies with?, or who form the student's "comparison group"? Does it also include neighborhood friends at other schools? Clearly these are difficult questions.

Second, as discussions in Manski (2000) and Moffitt (2001) make clear, plausible identification of peer effects requires (i) exogenous variation in the behavior of peers (with the peer group held fixed or at least randomly assigned) and/or (ii) exogenous variation in the peer group itself (e.g., random assignment). Given that peers are to some degree chosen, it is very difficult to find settings with plausibly exogenous variation in the peer group.[2] For example, consider the educational setting. Even if we could randomly assign students to classes, we cannot control their choices of who to be friends with, who to study with, or who to adopt as their performance comparison group. Furthermore, we cannot literally assign students randomly to classes because they (or their parents) have ways to circumvent these assignments, or even switch school, if they are unhappy with them. Similarly, if people chose peers or sort into peer groups (i.e., if the peer group is not assigned exogenously), it becomes very difficult to find interventions that exogenously shift the

[1]In the context of education, the concept of peer effects dates to at least the "Coleman Report" (Coleman et al. (1966)).

[2]Manski (1993) has particularly stressed the *Reflection Problem*—as people tend to chose peers who resemble themselves, there is typically a mechanical link between the characteristics of individuals and those of their peer group. This creates a great risk of falsely inferring that peer behavior affects own behavior, even if the causality actually runs the other way. See Manski (1993) and Manski (2000).

behavior of one or more peers while holding peer group composition fixed.

In this paper we argue that the United States Supreme Court provides a particularly good laboratory for estimating peer effects, for four key reasons: First, the relevant peer group of a justice can be clearly defined as the group of eight other justices who sit on the same Court. Second, if we view the whole set of justices as the peer group, the endogenous group selection problem does not arise, because justices have no choice over the identity of these peers. Furthermore, this peer group is of policy interest, as it can be altered by a well-defined policy lever (i.e., presidential nomination and Senate confirmation). Third, even though the full complement of peers is fixed (except in the infrequent instances when court composition changes) we will show there exists a highly plausibly source of case-to-case exogenous variation in any one justice's peer group. A little known fact, at least outside the legal community, is that many Supreme Court cases are decided by less than the full complement of justices. That is, justices are frequently absent from particular cases due to illness, recusals, and other random factors. This stochastic process creates variation in justices' peer group on a case-to-case basis that is clearly exogenous and not by choice.

Fourth and finally, the existence of "home court bias" generates a plausibly exogenous instrument that shifts the behavior of individual peer justices while the peer group is held exogenously fixed. Specifically, many Supreme Court justices previously sat on Federal Circuit Courts of Appeals. Epstein et al. (2009) find strong evidence that justices are highly inclined to rule in favor of their respective home circuit court, even conditional on ideology and other factors, and we find the same effect. This provides us with a compelling instrument to identify peer effects.[3]

Together, these four facts mean that the question "How does the ideology or voting behavior of a justice's peers affect his/her own vote?" is well-posed, as the peer group is well defined, it is not self-selected, and it is subject to plausibly exogenous variation. And in addition, the behavior of peers is subject to plausibly exogenous variation induced by home court bias. Furthermore, this question is of policy relevance, because it helps to predict the impact of any potential Supreme Court appointment on the overall voting behavior of the Court.

[3]Another formidable obstacle to identifying peer effects is that the externalities created by peer effects should presumably be internalized by the market's price mechanism or, failing that, by firms, or even governments. Only when none of these three institutions internalize the externality can one hope to observe it in equilibrium outcomes. We argue that the Court provides a rare example of a case where no such mechanism is operative.

This stands in sharp contrast to the question "How does a student peer group affect his/her grades?," which we would argue is not well-posed because the relevant peer group is not clear, it is almost certainly self-selected, and it is not clear how exogenous variation in such self-selected peer groups could be generated, nor what policy lever we could use to alter them.[4] And, given the relevant peer group is unclear, it is difficult if not impossible to find sources of exogenous variation in the behavior of relevant individual peers (while holding the peer group fixed).

In addition to being an ideal laboratory for studying peer effects, the composition of the Supreme Court and the rulings it makes are also of intrinsic interest, given their impact on important legal and social questions. Furthermore, understanding the extent to which justices with a particular ideological standpoint can influence the votes of other justices is important for understanding the optimal strategy for an administration in nominating justices. This, in turn, speaks to the characteristics and design of legal institutions.

It is particularly important to understand how existence of peer effects would affect the ideal presidential Supreme Court appointment. As Court decisions depend on majority voting, in the absence of peer effects the median justice will be pivotal, and case outcomes will reflect her position. It is thus tempting to think that the ideal appointment is one that shifts the median justice closest to the view of the President.

However if peer effects exist then voting decisions of justice j can be affected by the ideological position of justice i, and thus the Court's disposition will not merely be a function of the median justice's ideal point. In general, there is a disjuncture between a justice's *ideological ideal point* and their *effective ideal point*, with the latter including the impact of peer effects. More specifically, if peer effects are a function of ideological positions, then the effective ideal point of justice j depends on the ideological positions of the other justices. This suggests that the President, in choosing a nominee, should consider not only her ideological position, but also her ability to affect

[4]To play devil's advocate, one could argue the Court is no different, because a justice may choose to only care about the opinions of a subset of other justices, just as a student may choose a study group or "comparison group." But, in contrast to the classroom peer case, we argue that this doesn't vitiate interest in the effect of the entire set of peers on justice behavior. This is because we know the entire set of justices—and only that—can be altered by new appointments to the Court. In contrast, in the education example, if it were true that students were not influenced by their entire class, but only by their chosen friends or "comparison group," it would vitiate interest in the impact of class peers. Education researchers would instead seek out interventions that might influence the "comparison group," and their interest would shift to evaluating effects of those interventions.

the Court's rulings through her impact on other justices.

In this paper we look at two types of peer effects: how peer ideology affects a justice's voting behavior, and how actual peer votes affect a justice's voting behavior. Following the literature, we will refer to these two types of peer effects as "exogenous" and "endogenous" peer effects, respectively (see, e.g., Moffitt, 2001):

First, in Section 3, we consider "exogenous" peer effects that operate through the mean ideology of peers. To do this we first measure justice ideology by estimating a linear probability model of justice votes as a function of case characteristics and justice fixed effects (i.e., ideological positions). This utilizes a detailed coding of the votes in our dataset as being either conservative (1) or liberal (0) in orientation. Then, in a second stage, we add peer ideologies as additional explanatory variables in a model of voting behavior.[5] Unlike some other courts, Supreme Court cases do not involve random assignment of justices, and there is relatively slow turnover of justices. However, the Court composition does change frequently due to recusals and absences, which provide a plausibly exogenous source of peer variation from case to case. Using this approach, we find clear evidence of ideology-based exogenous peer effects. In particular, we find that replacing a single justice with one who votes conservative 10 percentage points more frequently increases the probability that *each* other justice votes in the conservative direction by 1.63 percentage points (on average).

Alternatively, peer effects may also be "endogenous," meaning they operate through the actual votes cast by peer justices, not their ideology *per se*. In that case, identifying a true peer effect requires exogenous variation in voting propensity across justices – i.e. a variable which directly affects how a given peer justice votes in a given case, but not the votes of other justices, except through the vote of the directly-affected peer. For this purpose, we utilize the fact that justices who have previously served on a Circuit Court of Appeals have a strong tendency to vote differently when a case comes from their "home" court, rather than any other Circuit Court. This "home court bias" provides us with an instrument with the above mentioned properties.

[5]One potential problem with estimating justice ideology from votes is that if peer effects exist, the ideology estimates are biased due to contamination from other justices, which in turn means the constructed peer ideology measures are contaminated by a justice's own ideology. However, as discussed in Section 3.6 and Appendix B, our estimation strategy is robust to this concern.

Thus, we also consider models that include both "exogenous" and "endogenous" peer effects, corresponding to the two terms in the most general "structural" model of peer effects discussed in Moffitt (2001) equation 16. As Moffitt (2001) and Manski (1993) note, in the reduced form of this structural model, own votes depend on own ideology, peer ideology, and the exogenous factor (i.e., home court bias) that shifts peer votes conditional on ideology. In the absence of endogenous peer effects, the exogenous factor that shifts peer votes drops out of the reduced form. Thus, testing for significance of the "home court" bias of peers in the reduced form is a simple test for the existence of endogenous peer effects (a test that should not be too sensitive to the exact functional form through which peer votes affect the own vote in the full structure). When we estimate this reduced form (see Section 3.8) we find both peer ideology and the peer home court bias variables are significant, implying both exogenous and endogenous peer effects are present.

Hence, in Section 4, we estimate structural models with both exogenous and endogenous peer effects. Here, we rely on both recusals and home court bias as sources of exogenous variation in peer ideology and peer votes. In our preferred model, we find that a percentage point increase in the proportion of peers casting conservative votes in a case makes a justice 0.9 percentage points more likely to vote conservative.[6]

Finally, we examine whether the peer effects that we find actually change pivotal votes, and hence case outcomes, or if they merely affect the size of the majority. If peer effects merely push a decision from 6-3 to 5-4, or *vice versa*, then they are of limited practical interest.[7] We again utilize the home court instrument, except that variables are now aggregated at the case level, and we consider how a single justice's vote affects the collective voting behavior of their peers. We find strong evidence that peer effects can be pivotal. A single justice becoming 10% more likely to vote conservative increases the share of cases with a conservative outcome by 3.6 percentage points–excluding the direct or mechanical effect of that justice–and reduces the share with a liberal outcome by 3.2 percentage points.

[6]To see this note that a 1% point shift in the average peer is the same as an 8% point shift for one justice. Multiplying that by 1.25 provides the effect of giving 1 justice a 10% point shift. Thus, the effect is 0.9% points times 1.25 = 1.1% points.

[7]Of course, the credibility of the Court, and how political it looks, is an important issue, and is plausibly affected by the size of the majority in a case. 5-4 decisions breaking along the lines of the party of the appointing President, for instance, may be seen as particularly political and this could be damaging to the image of the Court.

To highlight the magnitude and importance of the effects we estimate, one can consider the implied impact of replacing retired justice Anthony Kennedy with Justice Brett Kavanaugh. Using "Judicial Common Space" (Epstein et al. (2007)) measures of ideology plus the fact that Kavanaugh was nominated by a Republican President, we use the set of justices with JCS scores and our own ideology scores to produce an appropriately scaled estimate of Kavanaugh's ideology. Using our peer effect estimates we estimate that replacing Justice Kennedy with Justice Kavanaugh would have made *each* other justice only 0.3% more likely to vote conservative on a given case (the effect being muted here because Kavanaugh has a forecast ideology very similar to Kennedy's point estimate). In contrast, if Merrick Garland had succeed Justice Scalia, we estimate that would have made each other justice 5.1% more likely to vote liberal on a given case.

We are certainly not the first authors to consider the issues of judicial ideology and peer effects. Many political science and legal scholars have argued about whether Supreme Court justice decision making is largely driven by justices' own narrow policy preferences, or whether justices are also constrained by higher legal principles, such as deference to precedence and judicial restraint (Bailey and Maltzman (2011)), and political constraints, such as public opinion and executive discretion over compliance (Carrubba and Zorn (2010)). There is a significant empirical literature estimating the ideological position of judges and justices on measures that encapsulate both viewpoints. For instance, Martin and Quinn (2002) develop a dynamic item response model and estimate justice ideal points that can be time-varying via Markov Chain Monte Carlo methods, and Martin et al. (2005) use the Martin-Quinn method to estimate the median Supreme Court justice on Courts dating from 1937. If one thinks that peer effects operate through the characteristics of judges, then understanding judicial ideology is a necessary first step to study them, as well as being (arguably) of interest in its own right.

Perhaps closer to our paper is the literature on panel effects on lower courts. A large literature considers peer effects (often referred to as "panel effects") on U.S. Circuit Courts of Appeals.[8] Different authors emphasize different channels, such as: deliberation, group polarization, or aversion to dissent (Epstein et al. (2011)). Fischman (2015) argues that peer effects are best understood by reference to peers' votes rather than characteristics, and reanalyzes 11 earlier papers on Circuit

[8] For three notable examples, in addition to those mentioned below, see Revesz (1997), Miles and Sunstein (2006) and Posner (2008).

Court "panel" voting, as well as new data.[9] He finds that–across the board–each judge's vote increases the probability that a given judge votes in the same direction by approximately 40 percentage points. Boyd et al. (2010) considers the impact of female judges and, using Rubin (1974)'s "potential outcomes" approach, only finds strong effects for sex discrimination cases, suggesting an information channel is operative rather than alternative theories of influence.[10] Finally, Epstein and Jacobi (2008) suggest that the power of the median justice is due to bargaining power, not personality. They claim that ideological remoteness of the median justice gives them a greater range of the ideological spectrum over which they are pivotal.

Relative to this large literature, we see our contribution as threefold. One, we focus on the United States Supreme Court rather than Federal Circuit Courts of Appeals. Two, we analyze a simple and intuitive voting model using a novel identification strategy for both the ideological channel and the vote channel. And three, we focus on both peer effects and their impact in altering case outcomes.

Once one is convinced that peer effects exist, a key question, of course, is what is driving them. As we mentioned above, in the context of lower courts, several possibilities have been raised, including: deliberation, group polarization, and aversion to dissent. We return to the question of what is driving the effects we find in this paper in our concluding remarks, where we also offer estimates of our effects by issue area.

The remainder of the paper is organized as follows. Section 2 discusses the data we use. Section 3 contains our analysis of the ideological channel for peer effects, while Section 4 analyzes the ideological and voting channels jointly. Section 5 focuses on case outcomes, rather than just the peer effects themselves, and Section 6 contains some concluding remarks.

[9] He replaces the characteristics of panel colleagues with their votes, so the votes are endogenous, but colleague characteristics can be used as an instrument for colleague votes, assuming that they have no direct causal effect.

[10] See also Peresie (2005).

2 Data on Supreme Court Votes

We use data from the Supreme Court Database.[11] This database contains a wide range of information for almost the entire universe of cases decided by the Court between the 1946 and 2013 terms.[12] It provides a rich array of information for each case, including the case participants, the legal issue area the case pertains to, the court term in which the case was heard and opinions were issued, and it further identifies the winning party and overall vote margin. Particularly relevant for the analysis in this paper, the data includes the identity and voting decision of each justice, for each case in which they were involved, such that decisions of individual justices, and their relationship with the identity and voting decisions of the peer justices, can be analyzed. For almost all cases, votes are identified according to their ideological disposition, categorized as either liberal or conservative, with codification following an explicit set of rules, with the exceptions being for cases without any clear ideological underpinning, or occasions where a justice is absent or recused from voting. Finally, it also contains identifying data including case and vote identification numbers, and citation numbers used in official reports.

These data are augmented with additional information on each justice from the U.S. Supreme Court Justices Database developed by Epstein et al.[13] In particular, this provides information on which, if any, Circuit Court of Appeals a justice previously served on, and the length of their tenure on that court. This turns out to be useful as justices sometimes hear cases that come from a court they previously worked on (their "home court"). Our model accounts for bias towards (or against) this "home court."

In its entirety, the data provide information on 116 362 votes (including absences and recusals) from 12 981 cases. Restricting attention to the relevant subset of votes used in this paper (excluding absences, recusals and votes issued in cases without any discerned ideological direction), the data contains 110 729 votes with identified ideological direction[14] from 12 779 cases,

[11] Harold J. Spaeth, Lee Epstein, et al. (2014). Supreme Court Database, Version 2014 Release 1. `http://supremecourtdatabase.org`

[12] For example, non-orally argued cases with *per curiam* decisions are not included unless the Court provided a summary, or one of the justices wrote an opinion.

[13] Epstein, Lee, Thomas G. Walker, Nancy Staudt, Scott Hendrickson, and Jason Roberts. (2013). The U.S. Supreme Court Justices Database. `http://epstein.wustl.edu/research/justicesdata.html`

[14] A small number of cases result in tied votes, following which the votes of individual justices are typically not made public. Provided that the case had a lower court decision with stated ideological direction, so that the case is known to

three quarters of which involve a vote by all nine serving justices. Considering directional votes, the distribution of votes by ideological direction is closely balanced, with 48% being issued in the conservative direction. In contrast, the majority (55%) of lower court decisions in cases reviewed by the Supreme Court are in the conservative direction.[15] This reversal is symptomatic of a strong tendency towards overturning lower court decisions; in the dataset 58% of votes made by justices and 60% of Supreme Court opinions are in the reverse direction to the source court's decision. This tendency towards overturning is a natural consequence of the Supreme Court's operations; since it reviews only a small fraction of cases and chooses which cases to hear, there is a natural tendency towards selecting to hear cases in which a preponderance of justices believe (it is likely that) an incorrect decision had been made by the relevant lower court.

Table 1 breaks down these aggregate proportions across several stratifications of the data. Of the 11 high-level legal-issue-area categories in the database with a nontrivial number of votes in our sample,[16] the distributions of vote ideology over the entire 1946-2013 range of court terms vary from 29% conservative for Federal Taxation cases to 60% conservative in Privacy cases. Separating instead by the Circuit Court of Appeals that previously heard the case (for the $\sim$60% of cases that source from such a court) the conservative share of votes ranges from 43% for cases from the Seventh Circuit to 54% for Ninth Circuit cases.[17] There is a larger degree of variation in vote ideology proportions across justices, with conservative vote share ranging from 22% for William O. Douglas to 72% for Clarence Thomas (see Table 12 in Appendix A for details), while Figure 1 further illustrates how the conservative vote share has varied over time.

3 Exogenous Ideology-Based Peer Effects

In our first model, we assume exogenous peer effects. That is, we assume peer effects work directly through ideological positions, with the preferences of justice j directly influenced by the

have ideological relevance, the vote direction for each justice is coded as 0.5 by convention.

[15]There are a small number of cases with directional Supreme Court votes but unspecified lower court vote direction. This accounts for 1% of directional Supreme Court votes.

[16]There are another 4 issue area categories which collectively make up less than 0.1% of the sample, for 15 issue area categories in the entire database.

[17]The Ninth Circuit is often considered as being strongly liberal, which recalling the Supreme Court's endogenous case selection and its overall tendency towards overturning the decisions it reviews, is consistent with this high conservative vote share.

Table 1 – Descriptive Statistics for Directional Votes

	Votes	Cases	Vote Direction *(Cons. %)*	Lower Court *(Cons. %)*	Overturn *(%)*
Total	110,729	12,779	47.58	55.03	58.18
Legal Issue Area					
Criminal Procedure	22,549	2,585	52.12	63.07	60.23
Civil Rights	18,435	2,112	44.87	53.47	58.71
First Amendment	9,895	1,140	45.92	56.66	56.25
Due Process	4,975	577	42.57	53.65	59.84
Privacy	1,483	169	60.35	30.21	57.38
Attorneys	1,122	130	43.23	52.05	60.34
Unions	4,387	506	45.25	57.53	55.87
Economic Activity	21,447	2,500	42.28	48.82	57.20
Judicial Power	17,041	1,976	58.32	54.18	58.33
Federalism	5,805	670	43.65	56.66	58.23
Federal Taxation	3,415	394	29.49	56.78	52.71
Circuit Court					
Federal	937	107	46.21	43.00	62.82
First	2,125	246	47.01	40.82	51.40
Second	8,107	934	48.35	50.70	54.85
Third	5,008	575	51.54	49.84	54.21
Fourth	4,471	512	45.96	60.88	55.40
Fifth	7,907	914	43.49	65.12	60.88
Sixth	5,558	644	47.59	50.55	60.17
Seventh	5,523	645	42.97	59.07	58.63
Eighth	4,046	465	45.30	48.60	57.94
Ninth	11,835	1,359	54.30	38.27	62.80
Tenth	3,153	367	51.03	51.22	60.01
Eleventh	2,203	247	44.80	67.68	57.10
D.C.	6,961	818	52.15	51.13	59.46

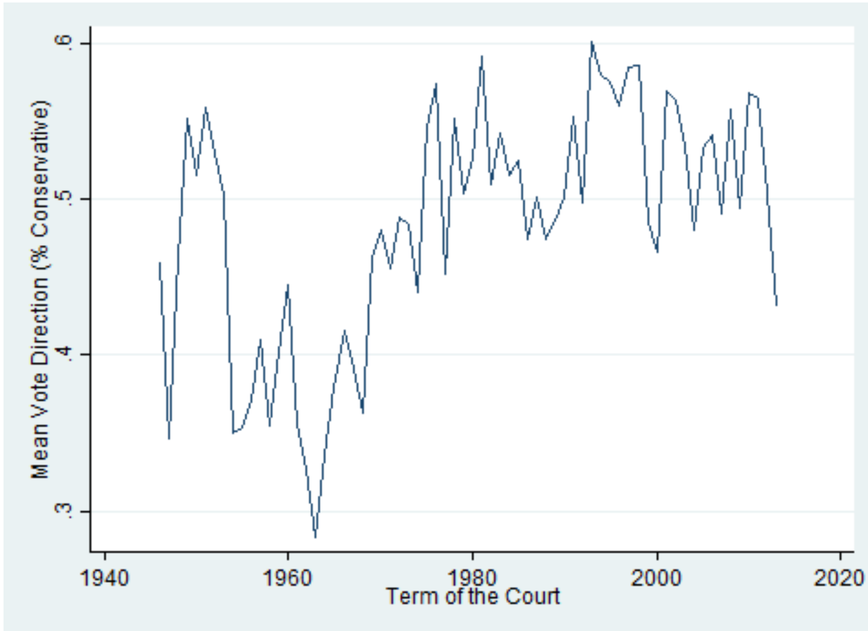


Figure 1 – Evolution of Conservative Vote Share by Term

ideological positions of the other justices $\{i\}\backslash j$. In the terminology of Manski, this is a contextual peer effect since justice ideology is predetermined with respect to their interactions with other justices. Under this mechanism, the voting decisions of justice i gravitate towards (or are repelled from) positions consistent with the ideology held by other justices (regardless of how those other justices actually vote in the same case).

In order to estimate the effect of peer ideology on the voting decisions of a justice, a two-step procedure is utilized. This is motivated by the need to first generate estimates of justice ideology. These individual ideology measures are then combined in order to construct measures of peer ideology. Finally, the peer effect estimation can be undertaken.

More specifically, the first step involves estimation of a linear probability model[18] of justice votes as a function of a set of case characteristics along with justice fixed effects. The fixed effect for each justice provides an estimate of his/her ideal point in the ideological spectrum. By virtue of the linear probability model framework, the estimated justice coefficients are interpretable as the fraction of cases in which the respective justice will make a conservative (rather than liberal) vote.[19] These justice coefficients can then be extracted and used to create proxies for peer ideology,

[18]The panel data structure with a predominance of dummy variables in the estimated model favors OLS estimation.
[19]Strictly speaking, we should say the justice coefficients capture the "fraction of cases in the excluded dummy

including but not limited to the mean ideological position of contemporaneous peers.

In the second step, these peer measures are added as an additional explanatory variable to the first-stage regressions. Nonzero coefficients on peer ideology indicate the presence of peer effects (rejecting the null hypothesis of the absence of peer effects). We estimate several specifications with different sets of controls for case and justice characteristics. In order to prevent peer variables from containing information about the case that is not present in the covariates,[20] for each specification the peer variables utilized in the second stage are those constructed from the analogous first-stage regression (that is, with the same set of covariates in both stages). Concluding that this two-step procedure yields unbiased estimates of peer ideology effects presents us with several econometric challenges, which are discussed in detail below.

3.1 Empirical Specification

Let j denote justice, c denote case and t denote year. The ideological direction of the vote by each justice present in each case, d_{jct}, is either conservative (1) or liberal (0). Define u_{jct} as the net utility that a justice derives from voting conservative rather than liberal. Then,

$$d_{jct} = \begin{cases} 1 & \text{if } u_{jct} \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

In our baseline model, the hypothesized utility function (also interpretable as the probability that a justice will issue a conservative vote) is of the form

$$\begin{aligned} u_{jct} = & \alpha_j + \gamma_c + l_c + \delta_t + lc\_dec_c \times \beta_1 + I\left[j \in app_c\right] \times \left[\beta_2 + \beta_3 \times app\_yrs_j\right] \\ & + lc\_dec_c \times I\left[j \in app_c\right] \times \left[\beta_4 + \beta_5 \times app\_yrs_j\right] + \varepsilon_{jct} \end{aligned} \tag{2}$$

where α_j is a justice fixed effect, γ_c is a fixed effect for the Circuit Court of Appeals (if any) that previously heard the case, l_c is a fixed effect for the legal issue area the case pertains to, and δ_t is a

categories where the justices issued conservative votes." However, the choice of excluded categories is an innocuous normalization, as the difference in the justice coefficients between any two justices does simply reflect the overall difference in the conservative vote proportion between those two justices.

[20] We subsequently use an instrumental variables approach to further address any such problem.

fixed effect for the court term that captures systemic drift in the ideology of the court over time.[21] lc_dec_c is the ideological direction of the decision made by the lower court, which the Supreme Court is reviewing. Further, $I\left[j \in app_c\right]$ is an indicator for whether the case sourced from a Circuit Court of Appeals for which the justice previously served, and app_yrs_j is the number of years that the justice previously served on a Circuit Court of Appeals (if any). These latter two variables are interacted with the decision of the lower court.

Subsequent specifications add further precision to the model. Since justices may conceivably have differing ideological preferences across different issue areas (that is, a single ideological dimension may not fully characterize justice ideological preferences) a second specification incorporates justice by issue area fixed effects α_j^l (replacing α_j and l_c). A third specification further adds issue area by term fixed effects δ_t^l to account for any differential systematic (across justices) ideological drift by issue area (replacing δ_t). The fourth specification further allows justice ideology to vary across time, by having justice by issue area by natural court[22] fixed effects $\alpha_j^{l,nc}$ (replacing α_j^l). The precise rationale for these specifications, in terms of the exogenous variation in peer ideology that they capture to identify peer effects, is discussed in detail in Section 3.5 below.

3.2 First Stage Results - Justice Ideology Estimates

The four specifications of the linear probability model outlined in Section 3.1 are estimated by OLS. Standard errors are clustered by case to account for unobserved case characteristics providing a common within-case shock to the votes of all justices. Given the purpose of extracting proxies for ideology, it is desirable that the specifications yield stable ideology measures. Table 2 shows the correlations between different measures, weighting equally by directional votes. The correlations vary from 0.70 to 0.99, and are particularly high between models where ideology is estimated for the same unit (such as justice-issue pairs in Models 2 and 3). Further, the potential empirical relevance of any peer ideology influences is inherently restricted by the influence of own ideology on voting decisions. If votes are not substantially driven by ideology, peer effects based on the transmission of ideology are unlikely to have meaningful effects. However, the model esti-

[21]Since there is no anchor on, or exact measure of, the ideology of cases heard over time, term dummies account for systematic changes in justice ideology net of changes in the ideological composition of cases heard.

[22]A natural court is a period during which no personnel change occurs on the court.

mates shown in Table 3 demonstrate that justice ideology is an extremely important determinant of votes; in each specification the justice fixed effects have substantial explanatory power over vote direction after controlling for all other model covariates, with marginal contributions to model R^2 of between 0.0805 and 0.1314.

Table 2 – Ideology Measures Correlation Matrix

	Model 1	Model 2	Model 3	Model 4
Model 1	1.0000			
Model 2	0.8708	1.0000		
Model 3	0.8716	0.9855	1.0000	
Model 4	0.7040	0.8168	0.8206	1.0000

For Models 2, 3 and 4 where justice ideology differs by issue area or natural court, ideology scores are demeaned within these groups to remove level differences between models that occur because the specifications have different dummy variables and thus omitted categories.

While most of the model coefficients are not of particular interest, several interesting results are worth a brief discussion. First, the coefficients for a conservative (liberal) lower court opinion (compared to the omitted category of an indeterminate lower court ideological direction) being negative (positive) reflect the tendency of the Supreme Court to overturn many decisions that it reviews. Second, a consistent pattern of *home court bias* is evident.

Previous service on a Circuit Court of Appeals (a justice's *home court*) affects how a justice votes when hearing a case sourced from that court (i.e., when they are *at home*). Justices who had previously served on a Circuit Court of Appeals and had relatively short tenure (8-10 years or less) are less likely to overturn the lower court's decision in a *home court case*. However this bias diminishes with home court tenure, and justices with long Circuit Court tenures are instead more likely to overturn lower court decisions when hearing a case sourced from their home court.

Table 3 – First Stage Results - Justice Vote Direction (Conservative %)

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Conservative LC	-0.061	-0.074*	-0.089	-0.092
	(0.041)	(0.043)	(0.059)	(0.061)
Liberal LC	0.085**	0.070	0.054	0.051
	(0.041)	(0.043)	(0.059)	(0.061)
Justice Home Court	0.108***	0.121***	0.123***	0.126***
× Conservative LC	(0.031)	(0.029)	(0.028)	(0.028)
Justice Home Court	-0.157***	-0.138***	-0.139***	-0.140***
× Liberal LC	(0.032)	(0.031)	(0.031)	(0.032)
Justice Home Court Tenure	-0.012***	-0.012***	-0.012***	-0.012***
× Conservative LC	(0.004)	(0.003)	(0.003)	(0.003)
Justice Home Court Tenure	0.016***	0.014***	0.014***	0.014***
× Liberal LC	(0.003)	(0.003)	(0.003)	(0.003)
Justice FE	Yes	No	No	No
x Issue Area	No	Yes	Yes	No
x Natural Court	No	No	No	Yes
Circuit Court FE	Yes	Yes	Yes	Yes
Issue Area FE	Yes	No	No	No
Term FE	Yes	Yes	No	No
x Issue Area	No	No	Yes	Yes
R-squared	0.1446	0.1753	0.2101	0.2378
Δ R-squared (Justice FEs)	0.0805	0.1111	0.1071	0.1314
Observations	110729	110729	110729	110729

Δ R-squared is the marginal explanatory power of justice ideology on vote direction, measured as the increase in model R-squared collectively due to the justice fixed effects (or justice by issue area fixed effects). * p<0.10, ** p<0.05, *** p<0.01

3.3 Second Stage Empirical Specification - Estimating Exogenous Peer Effects

Ideally, estimating the effect of the average ideology of a justice's peers would involve adding a variable $\overline{a^{l}_{\text{-}j}}$ measuring the average peer ideology to the specification in Equation 2, yielding

$$\begin{aligned} u_{jct} = & \alpha_j + \gamma_c + l_c + \delta_t + \beta_p \times \overline{a^{l}_{\text{-}j}} + lc\_dec_c \times \beta_1 + I\left[j \in app_c\right] \times \left[\beta_2 + \beta_3 \times app\_yrs_j\right] \\ & + lc\_dec_c \times I\left[j \in app_c\right] \times \left[\beta_4 + \times \beta_5 \times app\_yrs_j\right] + \varepsilon_{jct} \end{aligned} \tag{3}$$

However since justice ideology is unobservable, the peer variable that we actually utilize is the proxy $\overline{\widehat{\alpha^{l}_{\text{-}j}}}$ constructed as the average fixed effect (i.e. ideological position) of the concurrently serving justices, obtained in the first stage. The estimate of β_p measures strength and direction of peer effects. A positive coefficient indicates that judges are pulled towards the ideological position of their peers. A potential problem with using ideology estimates calculated from voting behavior is that if peer effects exist, our first-stage ideology estimates are contaminated by the ideology of other justices. This, in turn, means our constructed peer ideology measures are contaminated by a justice's own ideology. However, as discussed in Section 3.6 and Appendix B, this contamination is constant for a given composition of the court, and thus nets out with appropriately specified fixed effects.

3.4 Identification of Exogenous Peer Effects - Testing the Exogeneity of Recusals

The key difficulty in identifying peer effects in the context of the Supreme Court is that there is very little structural panel rotation. For example, unlike other courts, cases do not involve random assignment of a subset of justices, and further, the cohort of justices evolves only slowly over time. Intuitively, these features complicate the task of separating peer effects from joint ideological drift of justices over time. However, while cases before the Supreme Court are generally heard by the full panel of justices, justice absences provide a natural source of variation in the peers voting on a given case. As noted in Section 2, at least one justice is absent in roughly 1/4 of all cases (2 196 of 12 779). While official reasons for absence are in general not publicly stated, typical reasons include illness, not being confirmed to the Court at the time oral argument took place (for mid-term appointments), or recusal if a justice has heard the case on a lower court, argued it in a previous role as US solicitor general, or owns stock in a firm affected by the case. Our identification strategy primarily leverages this case-by-case variation in court composition to isolate the effect of peer ideology on justice votes.

To justify this, it is necessary to address concerns regarding whether justice absences are sufficiently random. If justices' participation decisions are correlated with case unobservables (that relate to vote probabilities, such as the latent ideological disposition of the case), then variation in court composition is tainted by selection bias. In such a setting, the latent vote probabilities would be correlated with the ideology of absent justices, inducing a correlation (in the reverse direction) between the vote probabilities and the ideology of peers voting on a case. Concerns about potential selection bias are more acute if justices exercise substantial discretion in choosing whether to be absent, as their decision may in part causally depend on case characteristics unobservable to the econometrician. We pursue two approaches to dealing with the possibility of selection bias in this setting. One is to implement an estimation procedure that is robust to non-random selection into absence. The second is to demonstrate that, upon analysis, there does not appear to be any correlation between unobserved vote probabilities and the ideology of absent justices, such that absences are as-good-as random.

Our first approach is to construct placebo tests which separate selection bias from the true peer effect of altering the composition of peers involved in a case. The variation in Court composition is particularly useful in that it allows the effect of peers to be considered both when they are active (voting on a case) and absent (recused). Intuitively, direct causal peer effects from a justice's ideology are plausible when justices participate and vote in a case. As peers compete for influence, each should be given greater weight the fewer peers present, such that the mean ideology of active peers is a sensible measure of the peers a justice faces.[23] Consistent with this, any peer effect should be sharply attenuated or eliminated when a justice does not vote (i.e. if recused, it would be considered improper for them to discuss the case with the other justices).[24] On the other hand,

[23]Conversely, if the influence of each peer does not decline at least proportionately as the number of peers increases, one obtains the odd implication that very large groups should exhibit near unanimity in making decisions, so long as positive peer effects exist.

[24]This does not preclude non-transitory peer effects of absent justice ideology, for example where a justice's ideology influences their peers' viewpoint or manner of thinking in an enduring manner. However, permanent peer effects like

suppose absences are strategic, and that there is a correlation between unobserved case characteristics (latent vote probability) and absent justice ideology. Then, the ideology of absent justices provides information about such unobservables, and allowing for multiple justices to be absent, the coefficient on their mean ideology is the natural measure of the extent of selection bias.

It is important to note that, within a given cohort, the mean ideology of active and absent peer justices will tend to be negatively correlated by construction. The two peer measures are not, however, collinear, as the absence of a particular justice causes the ideology of active peers to change differentially for each remaining justice, even though the absent peer measure is common, because each of their remaining peers (the set of which is unique to the justice) receives a higher weighting overall.[25] This mechanical correlation suggests an additional estimate of causal peer effects as the effect of variation in active peer ideology holding the ideology of absent peers constant, whereas the richer placebo test null hypothesis of no selection bias is that controlling for the ideology of active peers, absent justice ideology should not affect votes.

To test these hypotheses, for each of the four first-stage model specifications, three peer variables are created as the average ideology of (1) all other peers, (2) other justices active in a case, and (3) the justices absent from a case (set to zero if no justices are absent). Equation (3) is then estimated using each of these peer measures in turn, plus the further specification jointly testing the effect of active and absent peer ideology.[26] Then, peer effects can be identified by the coefficient on active justice ideology, while the absent peer ideology regressions operate as placebo tests to detect the presence of selection bias due to endogenous recusals. By comparing the coefficients on the different peer measures, the appropriateness of using recusal-based variation in peer ideology to isolate peer effects can be established.

Our second approach to addressing the issue of selection bias involves a direct empirical analysis of whether the ideology of absent justices is related to unobserved case characteristics. Previ-

this will be largely absorbed by justice and term FE, and thus will not be captured by a measure of absent peer ideology, nor in general identified by these tests. Thus our method at best captures only some of the channels through which peer effects may operate.

[25]Separately identifying the active and absent peer measures is further aided by a handful of permanent changes in court composition. For example in periods between the death of a justice and their replacement being appointed, the composition of active justices is altered but the departed justice is not considered absent (as they are no longer a member of the court at the time a case is considered).

[26]Since most cases involve no absent justices, the specifications containing the absent peer ideology variable also include a dummy indicating whether any justices are absent.

ous analysis of the empirical determinants of justice absences on the Supreme Court, such as Black and Epstein (2005) and Hume (2014), suggests that many absences are largely non-discretionary. Examples include oral arguments occurring prior to a justice being appointed to a court, extended illness,[27] or a case having already been heard by a justice while serving on a lower court. These absences are either plausibly independent of the affected cases, or well explained by observable characteristics (e.g. the pertinent circuit court of appeal a case come from). However, for the remaining more discretionary absences, it is harder to give this guarantee *a priori*; a justice with a lesser illness or a debatable conflict of interest may consider the ideological nature of a case before deciding whether to recuse themselves. Accordingly, while these papers do not provide evidence that absent justice ideology is correlated with latent vote probabilities, they cannot rule this out either.

Accordingly, our starting point in considering the potential for selection bias is to measure the relationship between unobservable case characteristics and the ideology of absent justices, restricting attention to cases where at least one justice is absent. Although unobservable case characteristics are, by definition, not directly observable, their effect on votes can be proxied by calculating the mean vote residual in the first stage model estimates, as the residual term captures the systematic effects of all uncontrolled for case characteristics in addition to any idiosyncratic shocks.[28] For a given case, a unit change in the mean vote residual is equivalent to switching the case from having a unanimous liberal vote to a unanimous conservative vote. A regression of the mean absent justice ideology on the mean case residual over the full sample period yields a statistically insignificant coefficient of -0.024 (SE 0.015). This suggests that absent justice ideology has little relationship with case ideology, with a fairly tight bound (around zero) on the degree of selection bias. The 95% confidence interval rules out more than a 5 percentage points difference in the average ideology of absent justices between two cases with the same observables but opposite unanimous votes due to unobservables.

It is important to note that non-random occurrence of absences alone is not sufficient to produce endogeneity bias; this additionally requires that the ideology of the absent justice(s) be non-

[27]For example, Justice Douglas was absent for a large number of consecutive cases during the 1949 term following a horse accident.

[28]The following analysis uses the results from Model 1.

random and correlated with counterfactual vote probabilities. Indeed, intuition suggests that justices should be more loathe to recuse themselves from cases they perceive as more important or likely to be divisive, irrespective of how they plan to vote. Indeed, Black and Epstein (2005) finds that recusals are less frequent in (typically higher-stakes) cases where the underlying issues have generated disagreement among different lower courts, and more likely in (typically lower-stakes) cases pertaining to statutory (rather than constitutional) interpretation. Consistent with this logic, cases yielding unanimous verdicts appear somewhat over-represented among cases with eight justices active and one absent justice.[29]

A further test utilizes the fact that approximately one-quarter of the cases with an absent justice (721 of 2 917) involve multiple absences. If the ideology of absent justices is related to unobserved case characteristics, then in cases with multiple absences the absent justices should share similar ideological inclinations. Supposing that k of N justices are absent in a case, the obvious thought experiment is the extent to which, given the ideology of $k-1$ absent justices, the ideology of the final absent justice out of the remaining $N-k+1$ justices can be predicted. This is calculated by regressing the ideology of each absent justice in turn against the mean ideology of the other absent justices, controlling for the mean ideology of the $N-k-1$ remaining justices (that is, the $N-k$ active justices plus the final absent justice, as this is akin to the remaining pot from which justice are selected without replacement).

However, selection bias is not the only plausible reason for absent justices to share similar ideology. Absence rates are elevated in a justice's first term on the court, such that multiple justices being appointed in quick succession, as occurs several times in the data, is responsible for a substantial number of multiple absences. Justices appointed in quick succession also are typically nominated by the same President, and thus tend to have similar ideology. Accordingly, the above regressions are conducted separately for first term absences (where this mechanism is at play) and all other absences (thus a cleaner measure of endogenous absence). Indeed, consistent with this hypothesis, the coefficient on the mean ideology of other absent justices is 0.40 (SE 0.05) for first term absences, whereas the test for selection into absence for the remaining sample yields a minuscule coefficient of -0.0002 (SE 0.11). This suggests that outside of justices' first term, where a

[29] 34.9% of 9 justice cases yield unanimous verdicts, and another 10.7% are 8-1.

clear and exogenous mechanism is at play, absent justice's ideologies are approximately orthogonal to each other. Again, this points towards justice absences being an as-good-as random source of variation in Court composition.

3.5 Second Stage Results - Estimating Exogenous Peer Effects

The estimation results for each of the four second-stage models are shown in Table 4. The results for the first model, where the peer measure is based on justice fixed effects controlling for term, are shown in the first panel of Table 4. The inclusion of term fixed effects allows changes in the ideological composition of the Court to be separated from joint ideological drift of justices over time (which may occur due to changing norms, beliefs and preferences of society), and ensures the latter are not mislabeled as peer effects.

The first column reports results using the mean ideology of all peers to measure peer effects. Since the all peer measure is based off justice fixed effects, for a given justice it is constant for all cases in a year, except due to infrequent cohort changes arising from mid-year appointments. While changes in the cohort of justices produces variation in a justice's ideology relative to their peers over time, it does so in a common way for all continuing justices.[30] Accordingly the all peer measure is close to collinear with the combination of term and justice fixed effects, which yields the very imprecise coefficient estimate shown in column (1).

In contrast, using our preferred active peers measure, identification of peer effects comes primarily from within-term variation in peers due to recusals. This specification, presented in column (2), yields a substantial and tightly estimated active peer coefficient of 1.311. This implies, for example, that replacing a justice with another who votes in the conservative direction 10 percentage points more frequently on average would increase the conservative vote probability of all other justices by 1.64 percentage points, generating a cumulative 0.13 extra conservative votes by the peer justices per case (i.e., $0.0164 \times 8 = 0.13$).

In Table 4 column (3) we see the absent peers measure yields a small and marginally significant negative estimate, but this disappears when we jointly including both the active and absent

[30]Since in constructing a mean ideology of other justices, each involves replacing the retiring justice's ideology estimate with the new justice's score.

Table 4 – Peer Ideology Second Stage Results - Justice Vote Direction (Conservative %)

Model 1: Justice, Term FE				
	(1)	(2)	(3)	(4)
	Vote Direction	Vote Direction	Vote Direction	Vote Direction
Mean All Peer Justices	-0.789			
	(0.968)			
Mean Active Peer Justices		1.311***		1.468***
		(0.371)		(0.511)
Mean Absent Peer Justices			-0.162*	0.038
			(0.085)	(0.120)
R-squared	0.5527	0.5531	0.5529	0.5531
Observations	110729	110729	110729	110729

Model 2: Justice by Issue Area, Term FE				
	(1)	(2)	(3)	(4)
	Vote Direction	Vote Direction	Vote Direction	Vote Direction
Mean All Peer Justices	0.390**			
	(0.154)			
Mean Active Peer Justices		0.562***		0.583***
		(0.129)		(0.138)
Mean Absent Peer Justices			-0.029	0.027
			(0.068)	(0.070)
R-squared	0.5689	0.5691	0.5687	0.5691
Observations	110729	110729	110729	110729

Model 3: Justice by Issue Area, Term by Issue Area FE				
	(1)	(2)	(3)	(4)
	Vote Direction	Vote Direction	Vote Direction	Vote Direction
Mean All Peer Justices	0.030			
	(0.800)			
Mean Active Peer Justices		1.245***		1.838***
		(0.275)		(0.305)
Mean Absent Peer Justices			0.015	0.157***
			(0.046)	(0.051)
R-squared	0.5869	0.5875	0.5870	0.5877
Observations	110729	110729	110729	110729

Model 4: Justice by Issue Area by Natural Court, Term by Issue Area FE				
	(1)	(2)	(3)	(4)
	Vote Direction	Vote Direction	Vote Direction	Vote Direction
Mean All Peer Justices	2.959			
	(2.444)			
Mean Active Peer Justices		1.990***		1.868***
		(0.251)		(0.415)
Mean Absent Peer Justices			-0.354***	-0.032
			(0.057)	(0.098)
R-squared	0.6014	0.6028	0.6025	0.6028
Observations	110729	110729	110729	110729

Models estimated with associated set of covariates used in analogous first stage regression, see Table 3. Peer variables are constructed using the first stage justice coefficients estimates. * p<0.10, ** p<0.05, *** p<0.01

peer measures (see column (4)). Comparing columns (2) and (4) we see that inclusion of the absent peers measure causes the coefficient on active peer ideology to increase slightly to 1.468. This suggests the negative coefficient on absent peers in column (3) is merely an artifact of the Court's average ideology being relatively stable within term, such that the ideologies of absent and active justices tend to be negatively correlated. It is also worth noting that the standard errors of the active and absent peer variables only increase by about 50% when both are included together, compared to when each is included separately (compare columns (2) through (4)). This illustrates that there is not a high degree collinearity between these two variables (for reasons we discussed in Section 3.4).

The second panel of Table 4 show results from the second model which utilizes a richer specification where justice ideology is allowed to vary by legal issue area. Since the term fixed effects are common across issue areas, this allows the peer variables to gain identification through differential variation in the ideology of peers by issue areas over time when justices are replaced by new appointees (since the common component of issue-area specific changes is differenced out by the term dummies). An alternate framing is that changes in the cohort of justices produces variation in the ideology of peers, and while this is common amongst continuing peers, it nonetheless differs by issue area. Using this richer model of ideology, the all peers measure now yields a significant peer effect coefficient of 0.390 (see column (1)).

Our preferred active peers measure, which gains additional identification from recusal-driven variation in peers, gives an estimate of 0.562 (see column (2)). For the thought experiment of replacing a single justice with another who votes in the conservative direction 10 percentage points more frequently, the latter estimate implies an increase of 0.7 percentage points in conservative vote probability (and thus $0.007 \times 8 = 0.056$ additional conservative votes per case). Further, the placebo test in column (3) yields a tightly estimated insignificant coefficient on absent peers. And, in column (4), we see that the coefficient on active peers changes little when the absent and active peer coefficients are jointly estimated.

Since Model 2 incorporates justice ideology (and thus peer measures) that differ by issue area, but only a single set of controls for term, it is vulnerable to the criticism that peer effects identified off changes in Court composition are not well distinguished from issue-area-specific ideological drift over time. Given exogenous ideological drift specific to an issue area, new justice appointments will on average have voting records and thus estimated ideology that captures this drift. Thus for issue areas where idiosyncratic (i.e. issue specific) ideological drift is pertinent, average peer ideology measures for cases of that issue area will tend to co-move with ideological drift and voting propensities, upwardly biasing the peer effects estimates.

Our third model controls for such issue-area-specific ideological drift via inclusion of term-by-issue-area fixed effects. The results are displayed in the third panel of Table 4. Analogously to the first model, the term-by-issue-area dummies soak up almost all variation in the all peers measure, so the associated coefficient is very imprecisely estimated. However the active peer measure, which is identified through within-year-and-issue-area variation in ideology of a justice's voting peers across cases due to recusals, yields a positive and significant peer effect coefficient of 1.245. By contrast, the placebo measure of absent justices yields a precisely estimated coefficient that is insignificant and near zero. These results change slightly under joint estimation of the effects of active and absent peers; the estimated effect of active peers is nontrivially higher at 1.838 while the coefficient on absent peers is rendered significant albeit relatively small. It is unclear whether this final result is indicative of a statistical artifact or captures a real but relatively small peer effect of justices even when not voting on a case. However, as we noted in Section 3.4, the model in column (4) can be interpreted as estimating the true peer effect while using the absent peer variable to control for selection effects.

The results for the fourth model, which allows the ideology of each justice to change over time (specifically, by natural court) for each issue area, are displayed in the final panel of Table 4. Allowing justice ideology to vary over time addresses any concern that the results could be confounded by non-systematic ideological drift, such as polarization where conservative and liberal justices move towards the extremes. Also, if justice ideology does vary idiosyncratically over time, this final specification may suffer from less attenuation bias because it better captures contemporaneous ideology. As above, the all peers coefficient is imprecisely estimated. The active peers measure, identified off the same source of variation as in Model 3 except that peer ideology measures are specific to the natural court, yields a significant and even larger point estimate of 1.990. In column (3) the absent peer measure is significantly negative at -0.354. However this appears to be merely an artifact of the negative correlation between the ideology of absent and active peers; when estimated jointly the coefficient on the absent peer measure is near zero and insignificant, while the active peer estimate is largely unchanged at 1.868.

3.6 Accounting for Potential Endogeneity & Measurement Error in Justice Ideology

While the results in Section 3.5 are collectively strongly indicative of substantial positive ideology-based peer effects, there are two notable issues with the estimation procedure. The first is that the justice fixed effects from the first stage are used to construct the peer ideology measure utilized in the second stage. But if peer effects are present, then the first stage is misspecified. As a result, each justice's own ideology measure will be contaminated by her peers' ideology. This, in turn, means that the peer ideology measures we construct will be contaminated by a justice's own ideology (see Appendix B for a detailed derivation). However, as shown in Appendix B, when we do fixed-effects estimation in the second stage, the justice j-specific effect that potentially "contaminates" the peer measure washes out. This is because the contamination is invariant across observations for a given justice. Nevertheless, we show that the measurement error in the peer ideology measure generates a relatively minor (downward) scale bias. Furthermore, using ideology estimates introduces random measurement error in our peer ideology measure, which should generate attenuation bias in the second-stage peer effect estimates. This means our findings regarding the magnitude of peer effects are likely conservative.

A second issue is that the ideology estimates constructed from the first stage estimates are based on each justice's full voting record, rather than being limited to their previous votes. This approach is practical, as the longer the voting history the ideology variables are based upon, the less noisy a proxy it should be, reducing attenuation bias caused by measurement error. But this means the peer ideology measures are not predetermined in a temporal sense. If future votes reflect a predetermined ideological propensity, this is not an issue. But a failure of strict exogeneity will arise if there is ideological drift over time due to past cases and decisions.[31]

[31] Note also that, in finite samples, individual votes have a non-vanishing effect on the justice ideology estimates. Unobserved characteristics of the contemporaneous case thus affect the justice coefficients in the first stage, causing the peer measures to be positively correlated with unobserved case characteristics in the second stage. While this effect is very slight if a justice is observed to vote on many cases, it nonetheless produces upwardly biased coefficients for the all and active peer ideology measures.

Given these two potential problems, the obvious approach is to instrument for the peer effect variable using a predetermined (to Supreme Court tenure, and thus voting behavior) measure of justice ideology. Segal-Cover scores (Segal and Cover (1989)) are estimates of justice ideology based on textual analysis of newspaper editorials between nomination by the President and the Senate confirmation vote. Thus, these ideology measures predate any of the justice's Supreme Court votes.[32,33] While Segal-Cover scores are at best noisy proxies for true justice ideology, they should be independent of the measurement error in our vote-based ideology measures, because they are based on pre-Court tenure observables. However, as we show in Figure 2 Segal-Cover scores are strongly correlated with our model-estimated justice ideology scores, with an even tighter relationship between the mean Segal-Cover score and our peer ideology estimates (since averaging over multiple justices reduces noise).[34]

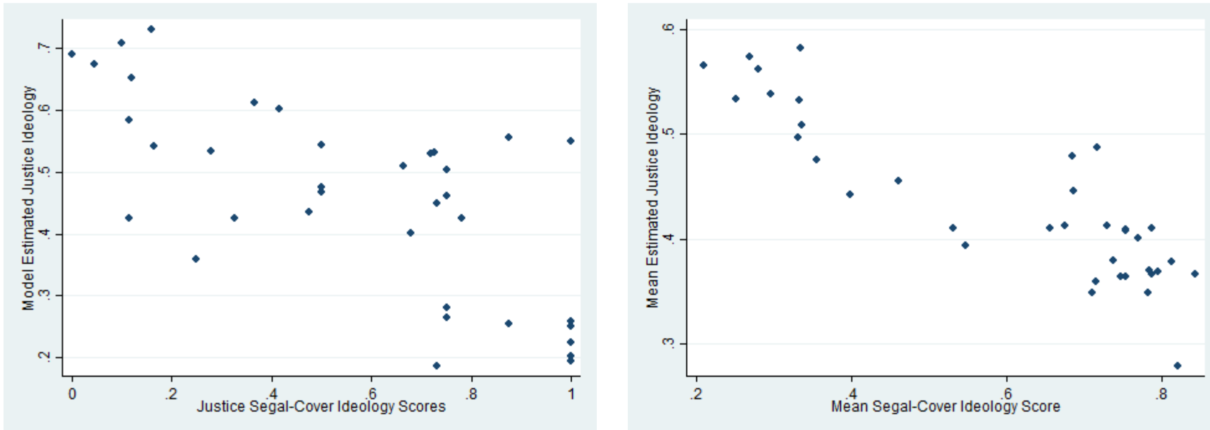
Accordingly we re-estimated the peer effect regressions in Table 4 using the mean Segal-Cover score of justice peers (all others, active peers, and absent peers in turn as appropriate) as an instrument for their true ideology. Before discussing the results, note that the use of Segal-Cover scores as an instrument involves the identifying assumption that the pre-Court tenure perceived ideology of justices only affects how their peers vote through their own true ideology (note that this is much more credible in specifications with time-based controls for ideological drift). For our first model specification, where justice ideology is common across all issue areas, it is sufficient to use a single Segal-Cover score variable as the instrument. However, in the specifications with justice ideology differing by legal issue area and/or natural court, we construct instruments using Segal-Cover scores interacted with issue area and court dummies.

The instrumental variable (2SLS) estimates are shown in Table 5. These results are generally consistent with the OLS estimates shown before. Peer effects are consistently found to be positive and of meaningful magnitude, in particular for the active peer measures where identifi-

[32] Formally, the coding from editorial text to ideology score was undertaken much later when Segal and Cover developed these scores, and the coding process involves some subjectivity (it does not, for example, follow a simple decision rule). However the scores remain plausibly exogenous to subsequent voting behavior of justices.

[33] Three of the justices in the sample sat on the court for several months as recess appointments before being nominated and confirmed by the US Senate through normal procedures, so their Segal-Cover scores, which stem from this later nomination, are not truly predetermined to all their votes. However the scores still predate the vast majority of their votes (98-99%), and the results are robust to adjusting the recess votes.

[34] Note these correlations are negative, because Segal-Cover scores are coded on a spectrum of 0 (conservative) to 1 (liberal), the reverse orientation to the voting propensity measure used in this paper.



(a) Ideology by justice

(b) Ideology by mean peer

Figure 2 – Relationship between Segal-Cover ideology estimates and Model (2) ideology estimates

cation comes from changes in Court ideology due to recusals. The results are generally consistent with what we found above–peer effects are positive and substantial– albeit the point estimates are slightly lower and less precise. This suggests that the bias introduced by measurement error in the ideology variable (potential attenuation bias) are relatively small. The placebo specifications testing peer effects of absent justices again find effects relatively close to zero, largely statistically insignificant, and of unstable sign.[35] Thus the IV results provide additional evidence of strong positive peer ideology effects.

3.7 Case Selection Bias and Term Fixed Effects

As justices select which cases the Supreme Court will hear, a potential source of bias - in models that exclude term fixed effects - is that the characteristics of cases chosen may depend on justice ideology, due to an underlying strategic objective. For example, a natural strategic aim of a majority coalition of justices with similar ideology is to enshrine their own preferences in precedent (or move precedent in their preferred direction). Winning cases thus becomes an instrumental goal. The appointment of a new justice that shifts the majority balance to some coalition may make them more willing to take on cases that are more ideological (in their favored direction) and thus offer a greater prospect of setting important precedent. By definition, these more ideological cases

[35]As before, the estimates using the all peers measure, where peer effects are less convincingly identified–and the exclusion restriction is less plausible–are very noisy.

Table 5 – Peer Ideology IV (Segal-Cover) Results - Justice Vote Direction (Conservative %)

Model 1: Justice, Term FE

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Mean All Peer Justices	-1.704			
	(1.375)			
Mean Active Peer Justices		1.304***		1.239*
		(0.497)		(0.692)
Mean Absent Peer Justices			-0.160	-0.012
			(0.117)	(0.160)
First Stage F-Statistic	356	1112	934	
Observations	110729	110729	110729	110729

Model 2: Justice by Issue Area, Term FE

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Mean All Peer Justices	0.222			
	(0.245)			
Mean Active Peer Justices		0.411*		0.518**
		(0.220)		(0.227)
Mean Absent Peer Justices			-0.027	0.017
			(0.108)	(0.110)
First Stage F-Statistic	865	735	85	
Observations	110554	110554	110554	110554

Model 3: Justice by Issue Area, Term by Issue Area FE

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Mean All Peer Justices	-1.921*			
	(1.162)			
Mean Active Peer Justices		0.811**		1.351***
		(0.396)		(0.422)
Mean Absent Peer Justices			0.028	0.143**
			(0.064)	(0.064)
First Stage F-Statistic	67	103	214	
Observations	110554	110554	110554	110554

Model 4: Justice by Issue Area by Natural Court, Term by Issue Area FE

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Mean All Peer Justices	1.273			
	(2.631)			
Mean Active Peer Justices		1.483***		1.900***
		(0.313)		(0.519)
Mean Absent Peer Justices			-0.344***	0.034
			(0.081)	(0.130)
First Stage F-Statistic	3351443	2.91e+10	334321	
Observations	110372	110372	110372	110372

Models estimated with associated set of covariates used in analogous OLS regression, see Tables 3 & 4. Peer variables are constructed using the first-stage justice coefficients estimates. Segal-Cover peer measure instruments are constructed from justice Segal-Cover scores. * p<0.10, ** p<0.05, *** p<0.01

are harder than usual for such a grouping to win - i.e., the more ideological a case is, the more likely is any given justice to vote in the opposite ideological direction.[36] If such endogenous case selection does exist, the resulting *case selection bias* will bias estimates of peer effects downwards if one fails to control for term fixed effects. This is because movements of the Court's ideological composition in one direction will change the distribution of cases heard, moving the average vote of continuing justices in the opposite direction.

In order to shed light on whether this case selection mechanism is important, we consider the relationship between the mean Segal-Cover score of justices sitting on a natural court and case characteristics that are known to be viewed as particularly conservative or liberal.[37] If case selection effects exist, then reviewing a larger number of conservative (vis a vis liberal) lower court decisions is behavior that would intuitively be consistent with a comparatively liberal Court. Figure 3 reveals a strong relationship as hypothesized, with more liberal Supreme Court cohorts (high average Segal-Cover scores) mostly reviewing conservative lower court opinions, and vice versa.

This analysis reveals an important reason to control for term fixed effects in the models in Sections 3.5 and 3.6. To the extent that case selection is governed by the justices jointly, irrespective of whether a justice will ultimately be recused, case selection effects will be common (at least by issue area) within a natural court.[38] Term dummies capture this effect, so the peer effect coefficients we report in Tables 4 and 5 are not biased by endogenous case selection.

3.8 A Simple Reduced Form Test for Endogenous Peer Effects

In Section 4 we will estimate a structural model of peer effects, including both exogenous ideology-based peer effects and also endogenous peer effects that operate through the votes of other justices (see equation 4). But first, we present a simple reduced-form test for whether endogenous peer effects exist. In the structural model of Section 4, we will continue to use recusals as an exogenous

[36]Implicit in this idea is that if a majority wins all cases by too large a margin, they could have chosen harder targets and still been successful.

[37]Note that, if observable case characteristics are impacted in one direction, it is plausible that this will be true of unobservable case characteristics also.

[38]This does not require that a justice who will ultimately recuse themselves from the case still participate in selecting the case to be heard, but rather that their recusal does not change the probability that the case is selected to be heard.

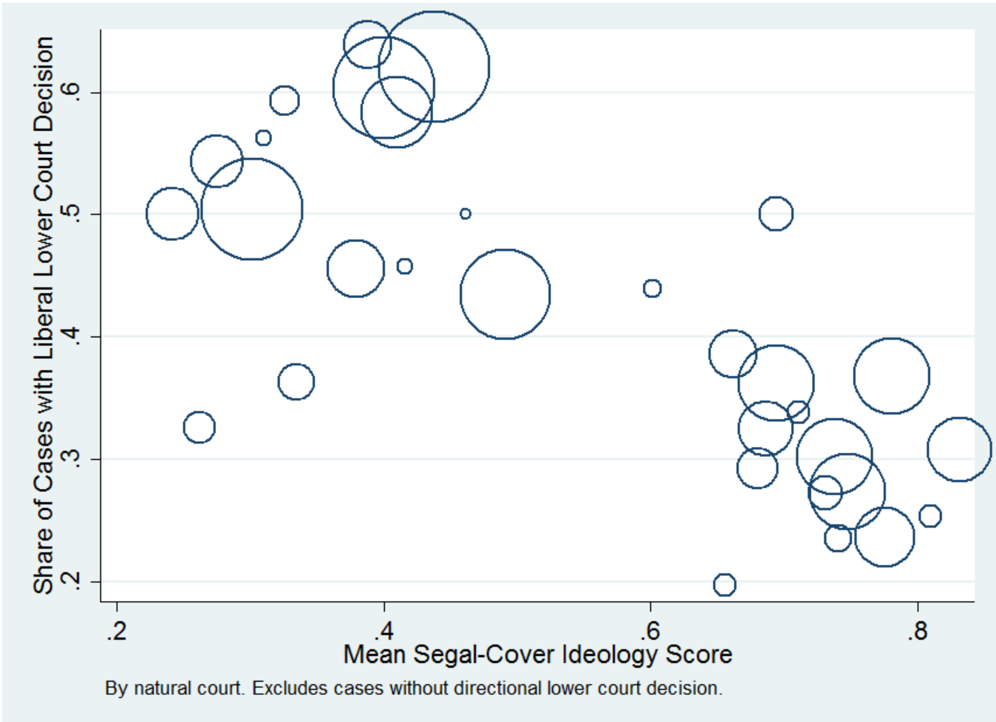


Figure 3 – Endogenous Case Ideology Selection

shifter of peer ideology, while also using "home court bias" as an exogenous instrument that shifts peer votes independently of peer ideology and case characteristics. In this section we estimate the reduced form of that model, obtained by substituting out for peer votes in equation 4 using their determinants. In practice, this is equivalent to simply entering the "home bias" instrument directly in equation 3. If the "home bias" term is significant in the reduced form, we will conclude that endogenous (vote-based) peer effects exist.

To be more precise, in Section 3.2 we showed that justices who have previous service on a Circuit Court of Appeals vote differently when hearing cases sourced from that court. In particular, those who had short Circuit Court tenures are less likely to overturn the lower court decision in such cases, whereas those with longer tenures are more likely to do so. To capture this in the reduced form, we add to equation 3 variables for the share of other justices *at home* $\frac{1}{N-1}\sum_{j\neq i} I[j \in app_c]$ and the average length of home court tenure per justice $\frac{1}{N-1}\sum_{j\neq i}(I[j \in app_c] \times app\_yrs_j)$ in a case (where the denominator counts both home and *away* justices). If these variables are jointly significant in the reduced form we take it as evidence that endogenous peer effects exist.

Table 6 reports estimates of the key variables of interest in the reduced from. We report only results for the model with justice and term fixed effects (called Model 1 previously). Interestingly, comparing the results here with those in Table 4 first panel, we see that the coefficients on peer ideology are essentially unchanged by the addition of the peer home court measures. Nevertheless, the peer home court measures are jointly highly significant. As it implausible that the home court variables would affect a justices vote directly, rather than indirectly through peer votes, this is strong evidence that endogenous peer effects exist. The sign pattern is consistent with the idea that the votes of home court peers dampen the propensity of the Supreme Court to *overturn* lower court verdicts, as the justices appear to show some deference to peers from the lower court. But the coefficients on the home court variables are not all individually significant, and it appears that the effect is stronger if the lower court decision was in the liberal direction.

Finally, note that the significance of peer ideology in the reduced form may arise *either* because exogenous peer effects exist, *or* because peer ideology affects a justice's own vote through its effect on peer votes (i.e., an endogenous peer effect). Thus our reduced form results could be consistent with a structural model that contains *both* exogenous and endogenous peer effects or one that contains only the latter. We must estimate the structural model in equation 4 of the next section to sort out these two explanations.

4 A Model with Both Exogenous and Endogenous Peer Effects

Here we extend the analysis of Section 3 to allow for endogenous vote-based peer effects, in addition to exogenous ideology-based peer effects. If peers affect the votes of their colleagues through their own votes, then the votes of justices and their peers are jointly determined on a case-by-case basis, as in Fischman (2015). This fits within the framework of Manski's endogenous peer effects (Manski (1993)). Of course, this does not preclude the existence of exogenous peer effects operating through peer ideology. Thus, in this section, we attempt to jointly estimate the effects of both the votes and ideology of peers.

Table 6 – Reduced Form Peer Effects - Justice Vote Direction (Conservative %)

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction	(4) Vote Direction
Mean All Peer Justices	-0.788			
	(0.956)			
Mean Active Peer Justices		1.317***		1.488***
		(0.371)		(0.512)
Mean Absent Peer Justices			-0.161*	0.042
			(0.085)	(0.120)
Share of Peers at Home				
× Conservative LC	0.121	0.129	0.120	0.131
	(0.170)	(0.170)	(0.170)	(0.170)
× Liberal LC	-0.477***	-0.488***	-0.478***	-0.489***
	(0.178)	(0.178)	(0.178)	(0.178)
Peer Mean Years at Home				
× Conservative LC	-0.032	-0.032	-0.031	-0.033
	(0.027)	(0.027)	(0.027)	(0.027)
× Liberal LC	0.070***	0.071***	0.071***	0.071***
	(0.021)	(0.021)	(0.021)	(0.021)
R-squared	0.1454	0.1463	0.1458	0.1463
Observations	110729	110729	110729	110729

* p<0.10, ** p<0.05, *** p<0.01

4.1 Empirical Specification and Vote Endogeneity

To estimate the effect of the votes of peers on a justice's own vote, a similar specification to Equation (3) is used, except that in addition to the reduced form peer ideology channel, peer effects are also captured by any of several variables reflecting the mean vote of (some subset of) other justices $\overline{d_{\text{-}j,ct}}$ in the same case.

$$\begin{aligned} u_{jct} =& \alpha_j + \gamma_c + \delta_t + l_c + \beta_p^v \times \overline{d_{\text{-}j,ct}} + \beta_p^i \times \widehat{\overline{\alpha_{\text{-}j}^l}} + \beta_1 \times lc\_dec_c + I\left[j \in app_c\right] \times [\beta_2 + \beta_3 \\ & \times app\_yrs_j] + lc\_dec_c \times I\left[j \in app_c\right] \times \left[\beta_4 + \beta_5 \times app\_yrs_j\right] + \varepsilon_{jct} \end{aligned} \tag{4}$$

This equation again includes justice and term fixed effects to control for systematic variation in vote ideology propensities across justices and time, and to account for case selection bias. However, unlike previously, focus is given to the simpler specification without justice by issue area and term by issue area fixed effects.[39]

[39] The "home bias" instrument used for votes (see below) is by definition unrelated to issue area or term, and empirically the correlation appears small, such that the results are robust to adding these controls.

In equation 4 β_p^i captures the effect of changes in the ideology of a justice's peer cohort, while β_p^v captures the relationship with peers votes, with a positive coefficient indicating that justices are inclined to vote in accordance with their peers. However, OLS estimates of β_p^v cannot be interpreted as a consistent estimate of endogenous peer effects, as votes are jointly determined. Unobserved case characteristics which affect the ideological position of a case drive the votes of both a specific justice and their peers, yielding an omitted variable bias in the OLS estimates. Since these unobserved case characteristics include almost everything material to the case, the vote of peers provides substantial information about the nature of the case.[40] Recalling that in the full sample 37% of cases involve a unanimous vote, even the vote of a single justice has very substantial predictive power over how other justices vote.

Very strong correlations can exist between votes, irrespective of the existence of peer effects. Table 7 documents these strong correlations, showing the OLS estimates from regressions of vote direction on three different measures of peer votes. Column 1 uses the mean vote direction (proportion conservative) of other justices in the case. Columns 2 and 3 explore the predictive power of the votes of *home* justices in *home court* cases; defined as those sourced from the Circuit Court of Appeals on which the justice previously served. Column 2 shows the estimated relationship between a justice's own vote and the mean vote of "home" justices in the same case.[41] As the relationship between a justice's own vote and the votes of home justices should be stronger when they are more numerous, Column 3 considers the relationship between a justice's own vote and the net vote direction of other home justices (i.e., the number of home justices issuing conservative votes minus liberal votes), divided by the total number of all peer justices present in the case.[42] As expected, each of these regressions reveals a strong relationship between a justice's own vote and the votes of peers. But due to endogeneity bias this provides no insight into the existence of peer effects.

[40]Recall that the observed case characteristics include the legal issue area, the lower court decision, the Circuit Court of Appeals (if any) that the case stems from, and the term in which the case is heard by the Supreme Court. Conditioning of these variables leaves much of the variation in case vote outcomes unexplained, implying that unobserved case characteristics are very important determinants of votes.

[41]Since this is by convention set to zero in cases where no home justices are present, such as any case not from a Circuit Court of Appeals, a dummy variable is added to indicate the presence of another home justice.

[42]For example, if there is a single home peer justice, and they vote liberal, this variable is $-1/8$. If there are three home peers, of which two vote liberal and the other conservative, the variable is also $-1/8$. If there are two home peers, and both vote liberal, it is $-2/8$.

It is interesting that in Table 7 column 1 the mean ideology of active peers has a negative association with a justice's vote given the control for the vote of peers. A simple explanation is that a justice's vote may reflect either their own ideology or unobserved case characteristics. If a conservative justice is observed to vote liberal, this conveys far more information about unobserved case characteristics than if the justice voted in their typical ideological direction. By comparison, the positive coefficients on peer ideology in Section 3 occurred when not holding peer votes constant. Likewise, in Columns 2 and 3 the peer vote measures capture only the votes of home peers, and thus the coefficient on peer ideology remains positive and similar in magnitude to the Section 3 estimates.

Table 7 – Peer Vote Effects OLS - Justice Vote Direction (Conservative %)

	(1) Vote Direction	(2) Vote Direction	(3) Vote Direction
Peer Vote Mean	0.861***		
	(0.003)		
Home Peer Vote Mean		0.444***	
		(0.014)	
Net Home Peer Vote Mean			1.466***
			(0.050)
Mean Peer Ideology	-0.452***	1.306***	1.282***
	(0.062)	(0.361)	(0.360)
Circuit Court FE	Yes	Yes	Yes
Justice FE	Yes	Yes	Yes
Issue Area FE	Yes	Yes	Yes
Term FE	Yes	Yes	Yes
R-squared	0.7252	0.5704	0.5697
Observations	110729	110729	110729

Ideology measure is mean of active peers from Model 1. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

4.2 Instrumental Variables Estimation Results

To identify true peer vote effects it is necessary to isolate exogenous variation in voting propensity across justices. This requires a variable which directly affects how a justice votes in a given case, but has no plausible rationale for affecting the votes of others except through the vote of the directly affected justice (see Moffitt (2001)). While observed case characteristics do produce variation in peer votes across cases, they also have a direct effect on a justice's own vote, so they do not provide valid instruments.

More fruitfully, we can exploit the fact mentioned in Section 3.2, that justices who have previous service on a Circuit Court of Appeals vote differently when hearing cases that are sourced from their home court. In particular, justices who had short tenures on a Circuit Court of Appeals are on average less likely to overturn a lower court opinion, while the reverse is true for justices with long home court tenures. For example, Justice Kennedy, a conservative, who served on the stereotypically liberal 9^{th} Circuit for 12 years, exhibits a strong bias against his home court. Similarly, Chief Justice (then Judge) Warren Burger, who is famous for the extent to which he clashed with liberals on the D.C. Circuit Court over his 13 year tenure (Greenhouse (2007)), also exhibits a negative home bias. More generally, it seems plausible that deference to colleagues is relatively quick to form, but enmity (or independence) takes time, and this may drive the observed pattern. Figure 4 documents this tendency by plotting the differential in the rate at which justices overturn decisions in cases from their home court compared to all other cases, against the duration of home court tenure, for each of the 19 justices who previously served on a Circuit Court of Appeals.

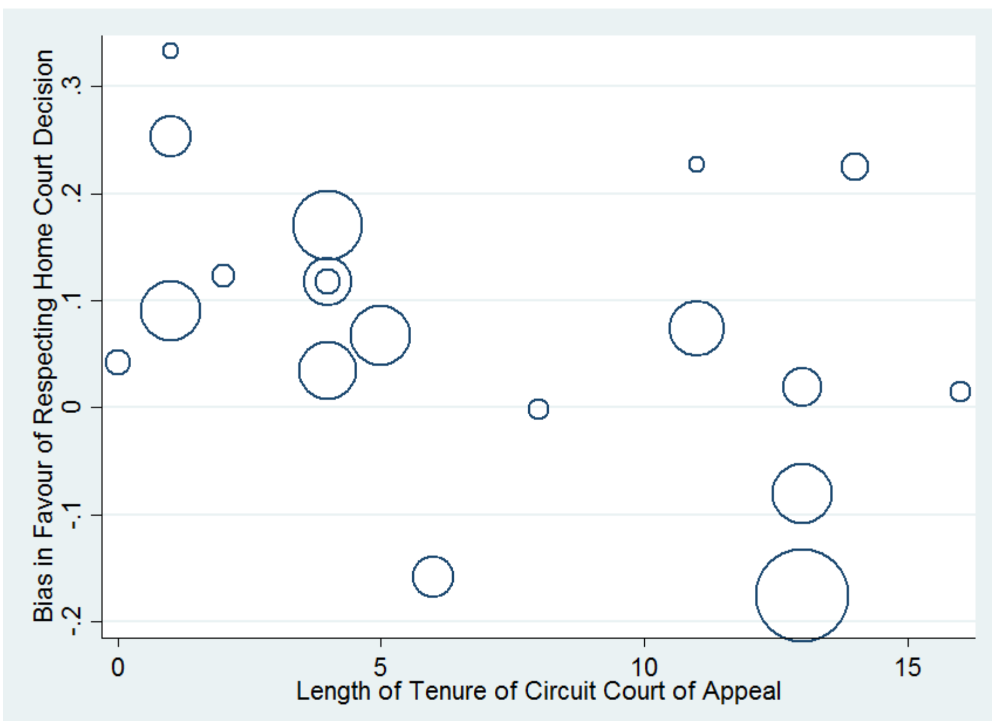


Figure 4 – Home Court Bias in Overturn Rate of Lower Court Decisions

It is thus possible to consistently estimate Equation (4) by Two-Stage Least Squares, using the share of other justices *at home* $\frac{1}{N-1}\sum_{j\neq i} I[j \in app_c]$ and the average length of home court tenure per justice $\frac{1}{N-1}\sum_{j\neq i}\left(I[j \in app_c] \times app\_yrs_j\right)$ in a case (where the denominator counts both home and *away* justices) as instruments for the votes of peer justices. In order to capture the effect of home justice votes on the ideological direction of a justices own vote, the two home justice variables are interacted with the ideological direction of the lower court opinion.[43] This method relies on the (plausible) exclusion restriction that a justice's vote is affected by the presence of home justices and the length of their home tenure only through the votes of the home justices (directly) and the votes of other away justices (indirectly, through the potential peer mechanism).

To address any possibility that the instruments are contaminated by selection of which justices are present and vote in respective cases, two different specifications of the instruments are considered. These both utilize the share of other justices at home and the average length of home court tenure per justice, but in one specification the instruments are defined using the justices active in each respective case, while the other uses all justices on the Supreme Court.[44]

Consistent with Figure 4, the first stage results in Table 8 show there is a strong relationship between the home justice variables and voting propensities. The pattern of justices with short (long) home tenure being respectively less (more) likely to overturn lower court decisions (indicated by the +, -, - + pattern of the four coefficients) is evident irrespective of whether all, or only active justices, are considered. Indeed comparing the top and bottom halves of Table 8), we see that the estimates of the effects of home court justice votes on own justice votes is little affected by whether all or only active justices are used to construct the home bias instruments. The mean of active peer ideology is also significant in the first stage.

The second stage instrumental variable (2SLS) estimates exploit the variation in justice votes driven by home court affiliation to estimate the extent to which a justice's vote is causally affected by the votes of their peers. The IV estimates, presented in Table 9 are surprisingly similar to the OLS estimates in Table 7.[45] Thus, the high correlation between justice votes is not solely due to

[43]This is only for liberal and conservative lower court decisions. In cases where the lower court opinion is not of specifiable direction, overturning the lower court is not well defined.

[44]If there are no selection effects to be concerned about, the former specification is more intuitive since the endogenous variable can only utilize the votes of active justices.

[45]As expected, instrumenting causes the standard errors of the estimated peer vote coefficients to increase by factors of about 5 to 10. But the IV estimates of the peer vote coefficients in 9 are nevertheless highly significant. And the point estimates are not very different from the OLS results in Table 7.

Table 8 – Peer Vote Effects IV First Stage - Peer Vote Measures

	Peer Vote Mean		Home Peer Vote Mean		Net Home Peer Vote Mean	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Peers at Home						
× Conservative LC	0.211		0.385*		0.178**	
	(0.169)		(0.211)		(0.090)	
× Liberal LC	-0.582***		-1.403***		-0.280***	
	(0.176)		(0.229)		(0.094)	
Peer Mean Years at Home						
× Conservative LC	-0.039		-0.085***		-0.025***	
	(0.026)		(0.030)		(0.009)	
× Liberal LC	0.079***		0.217***		0.050***	
	(0.020)		(0.025)		(0.008)	
Share of Active Peers at Home						
× Conservative LC		0.289*		0.470**		0.198**
		(0.173)		(0.237)		(0.093)
× Liberal LC		-0.579***		-1.420***		-0.304***
		(0.177)		(0.256)		(0.095)
Active Peer Mean Years at Home						
× Conservative LC		-0.054**		-0.085***		-0.027***
		(0.026)		(0.031)		(0.009)
× Liberal LC		0.070***		0.231***		0.053***
		(0.021)		(0.027)		(0.009)
R-squared	0.6886	0.6886	0.5853	0.5870	0.0841	0.0885
Observations	110729	110729	110729	110729	110729	110729
First Stage F-Statistic	5.369	5.284	25.032	26.180	23.051	24.506
First Stage P-Value	0.000	0.000	0.000	0.000	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.01

unobserved case characteristics, but seems to be due at least in part to endogenous peer effects working through the actual votes of peer justices.

The magnitude of peer effects implied by the IV estimates is sizeable and of practical significance for each of the peer measures mentioned above. Columns 1 and 2 show that, holding all else equal, a percentage point increase in the proportion of peers issuing a conservative vote in a case makes a justice 0.9 percentage points more likely to vote conservatively. In the typical full panel case (with 8 peer justices), this means that a single peer experiencing a 10 percentage point increase in conservative vote probability yields a direct effect of 1.1 percentage points on each other justice.

Columns 3 to 6 focus explicitly on the effect that the votes of home justices have on their peers. A percentage point increase in the proportion of home peers who issue a conservative vote in a case makes the votes of their peers on average 0.3 percentage points more conservative. Accordingly, in cases with a single home justice, switching their vote has a 30 percentage point effect on peer votes. While this effect may seem large, it is actually smaller then the effect that Fischman (2015) finds for peer votes on circuit courts. The final two columns allow the peer effect of an additional home justice being in a case to be calculated; such a change produces a one-eighth change in the net home peer vote mean variable, and thus has a 14 percentage point effect on the conservative vote probability of peers.[46]

Importantly, the mean active peer ideology variable is also significant in the structural models in Table 9. Thus we find that *both* exogenous and endogenous peer effects are significant. It is interesting that the coefficient on active peer ideology in the models in Table 9 columns (3)-(6) are all close to 1.30. This is very similar to the peer ideology effects reported in the 2nd column of the top panels of both Table 4 and Table 5, where we only allowed for exogenous peer effects, and it is also very similar to the reduced form estimate of the peer ideology effect in Table 6. In all these specifications, the votes of away peers are not held constant, so it is unsurprising that the coefficient on peer ideology remains positive and of similar magnitude. But with the votes of away justices held fixed, as in Table 9 columns (1)-(2), a more conservative ideology of peers

[46]By virtue of the specification, the effect of a home justice switching the ideological direction of their vote is assumed to be twice as large.

mechanically signals unobserved case characteristics that are less favorable for a conservative outcome, for reasons discussed earlier. Thus we prefer the models in Table 9 columns (3)-(6), because in these specifications the active peer ideology coefficient is not driven negative by this mechanical effect.

Table 9 – Peer Vote Effects IV Second Stage - Justice Vote Direction (Conservative %)

	(1)	(2)	(3)	(4)	(5)	(6)
	Vote Direction		Vote Direction		Vote Direction	
Peer Vote Mean	0.894***	0.877***				
	(0.037)	(0.041)				
Home Peer Vote Mean			0.338***	0.301***		
			(0.067)	(0.063)		
Net Home Peer Vote Mean					1.291***	1.122***
					(0.272)	(0.249)
Mean Peer Ideology	-0.520***	-0.485***	1.307***	1.308***	1.285***	1.288***
	(0.092)	(0.102)	(0.362)	(0.363)	(0.361)	(0.361)
Observations	110729	110729	110729	110729	110729	110729
Home Peer Instruments	All	Active	All	Active	All	Active
First Stage F-Statistic	5.369	5.284	25.032	26.180	23.051	24.506

Ideology measure is mean of active peers from Model 1. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

4.3 The Total Effect of a Justice's Vote in Models with Endogenous Peer Effects

When considering endogenous effects, it is possible that initial shocks to voting propensities are propagated from justice to justice. In fact, we can show that different propagation mechanisms, which amount to differing peer effect mechanisms, can yield a common average peer effect coefficient. For insight, consider the following stylized examples, with a single home justice experiencing a shock to her vote propensity. Now let λ be the direct effect of one justice's vote on the vote of the other justices, scaled down by the number of peers. We shall refer to this as the *direct effect*. We consider three natural possibilities of how the direct effect translates into the total effect on the vote of a justice.

First, it may be that the vote of a justice affects each other justice only directly, with no propagation through the votes of other justices. This occurs when justices provide information to each other; each receives a signal which determines initial voting propensity and is made public to the others. Given the signals of peers, the vote probability of the individual justice is a sufficient statistic for her signal. This signal can affect the vote probability of each peer justice, but have no subsequent spillovers, because any vote changes by the peer justices are understood to be in response to the initial justice's signal and thus provide no additional information. In such a context, an initial shock of magnitude k to the home justice's vote probability shifts the vote probability of each peer by $\frac{\lambda k}{N-1}$, with no multiplier effect occurring. The lack of multiplier effects means that the home peer vote variable changes by a large amount relative to the mean peer vote measure, limiting the coefficient on home peer votes. In expectation the peer vote mean variable for away justices shifts by $\frac{(N-2)\frac{\lambda k}{N-1}+k}{N-1}$, so the average peer coefficient is

$$\beta_p^1 = \frac{\lambda}{\frac{N-2}{N-1}\lambda + 1}.$$

Given our estimate of $\beta_p = 0.874$ and that $N = 9$, this implies a λ of 3.7. This implies a direct effect of a given justice's vote on the vote probability of any other justice of 3.7/8=0.46, under this (perhaps implausible) hypothetical.

Second, suppose that indirect propagation does occur. For example, in addition to the direct peer effect arising due to the shock experienced by the home justice, suppose justices further respond equally strongly to the induced changes in the votes of their other peers. However, suppose that the home justice experiences no indirect peer effects reflecting back on themselves; as above their initial change in vote probability is a sufficient statistic for the information content they provide. Then an initial shock of magnitude k to the home justice's vote probability produces a direct effect of $\frac{\lambda k}{N-1}$ on the vote probability of each peer, which is then multiplied by $(1 - \frac{N-2}{N-1}\lambda)^{-1}$ through the indirect propagation mechanism. Compared to the first propagation mechanism, the mean peer vote variable changes by a large amount relative to the home peer vote measure, with the multiplier effects amplifying the coefficient on home peer votes. In expectation the peer mean vote variable for away justices shifts by

$$\left((N-2) \times \frac{1}{1-\frac{N-2}{N-1}\lambda} \times \frac{\lambda k}{N-1} + k\right) \Big/ (N-1)$$
$$= \frac{k}{(N-1)-(N-2)\lambda},$$

so the average peer coefficient is

$$\begin{aligned}\beta_p^2 &= \frac{\lambda}{1-\frac{N-2}{N-1}\lambda} \Bigg/ \left(\frac{N-2}{1-\frac{N-2}{N-1}\lambda} \times \frac{\lambda}{N-1} + 1\right) \\ &= \lambda.\end{aligned}$$

Third, suppose that indirect propagation does occur for all justices, including the justice initially experiencing the shock. Then an initial shock of magnitude k to the home justice's vote probability produces a direct effect of $\lambda k/(N-1)$ on the vote probability of each peer, with an immediate reflection on the home justice of $\lambda \times \dfrac{\lambda k}{N-1}$. These effects are then amplified by a factor of $\left(1-\lambda \times \frac{\lambda+N-2}{N-1}\right)^{-1}$. In expectation the total effect on the peer mean vote variable is

$$\begin{aligned}&\left((N-2) \times \frac{\lambda k}{N-1} \times \frac{1}{1-\lambda \times \frac{\lambda+N-2}{N-1}} + \frac{\lambda^2 k}{N-1} \times \frac{1}{1-\lambda \times \frac{\lambda+N-2}{N-1}} + k\right) \Bigg/ (N-1) \\ &= \frac{k}{(N-1) \times \left(1-\lambda \times \frac{\lambda+N-2}{N-1}\right)}\end{aligned}$$

for away justices and

$$\frac{\lambda k}{(N-1) \times \left(1-\lambda \times \frac{\lambda+N-2}{N-1}\right)}$$

for the home justice who experiences the initial shock. Where the average peer coefficient β is identified off variation in the peer vote mean variable for away justices, it is given by

$$\begin{aligned}\beta_p^3 &= \frac{\lambda}{1-\lambda \times \frac{\lambda+N-2}{N-1}} \Bigg/ \left((N-2) \times \frac{\lambda}{N-1} \times \frac{1}{1-\lambda \times \frac{\lambda+N-2}{N-1}} + \frac{\lambda^2}{N-1} \times \frac{1}{1-\lambda \times \frac{\lambda+N-2}{N-1}} + 1\right) \\ &= \lambda.\end{aligned}$$

Thus, in both case 2 (which we might call "partial reflection"), and case 3 (which we might call "full reflection") we find that $\beta_p = \lambda$. Technically, adding reflection back to the home justice scales up the effect of each justice on each other justice proportionally, leaving the solution to the fixed point problem unchanged.

4.4 Addressing the Potential Exogeneity of Home Court Status

A natural concern with using the home court status of justices as an instrument for justice voting propensity is that the cases which the court hears are chosen by justices. Hence a justice's previous tenure on a Circuit Court of Appeals may affect the nature of cases that are chosen to be heard from their prior court, relative to other courts. For example, it seems plausible that the same bias that leads justices to have an increased (decreased) propensity to overturn decisions from their home court could also lead them to advocate disproportionately for (against) the Supreme Court reviewing decisions from their home court to begin with.

Crucially, were a case selection bias of this form to exist, it is far from clear that this would bias the IV estimates upwards. First, consider a justice biased towards the home court, who may try to prevent home cases from being reviewed by the Supreme Court. Intuitively, their lobbying to prevent cert being granted is most likely to be successful for cases with below average ex ante overturn probability (based on case characteristics and facts).[47] Selecting out these cases would thus increase the average overturn propensity observed for home cases that reach the Supreme Court, and falsely look like a negative peer effect. Conversely, suppose a justice biased against their home court desires to have additional cases from their home court reviewed by the Supreme Court. Since the Supreme Court has a disproportionate tendency to overturn lower court decisions, it is plausible that the marginal home case that the justice may persuade the Supreme Court to hear has lower than average overturn probability, by virtue of it not otherwise being reviewed.[48]

Moreover, the data regarding the frequency of cases from each Circuit Court of Appeals fails to show any clear link to the presence of home justices. Considering each Circuit Court in turn, Figure 5 separates cases into three groups; cases (irrespective of whence they are sourced) where no justice with previous tenure on the considered Circuit Court is on the Supreme Court, and then those with short and long home tenure justices from the considered court respectively (note that

[47]This may be tempered by the home justice having greater incentive, and thus investing greater effort, to prevent cases with high overturn probability from being reviewed.

[48]This effect may be weak since the Supreme Court chooses to hear only a small proportion of cases over which it has jurisdiction, even when it would counter-factually view the lower court as having made an incorrect decision. The qualifier that justices may focus their lobbying on cases with higher perceived overturn probability also applies.

the latter two can be present simultaneously). For each group, we report the share of cases from the respective Circuit Court. In general, the relative frequency of cases from each Circuit Court is similar regardless of the presence of a justice with tenure from that same court, or the length of that tenure. The most notable exception is an artifact of a consistent increase in the share of cases from the relatively liberal 9th Circuit over time, combined with Justice Kennedy, who had previously served on the 9th circuit, being on the Supreme Court from 1988 until recently.

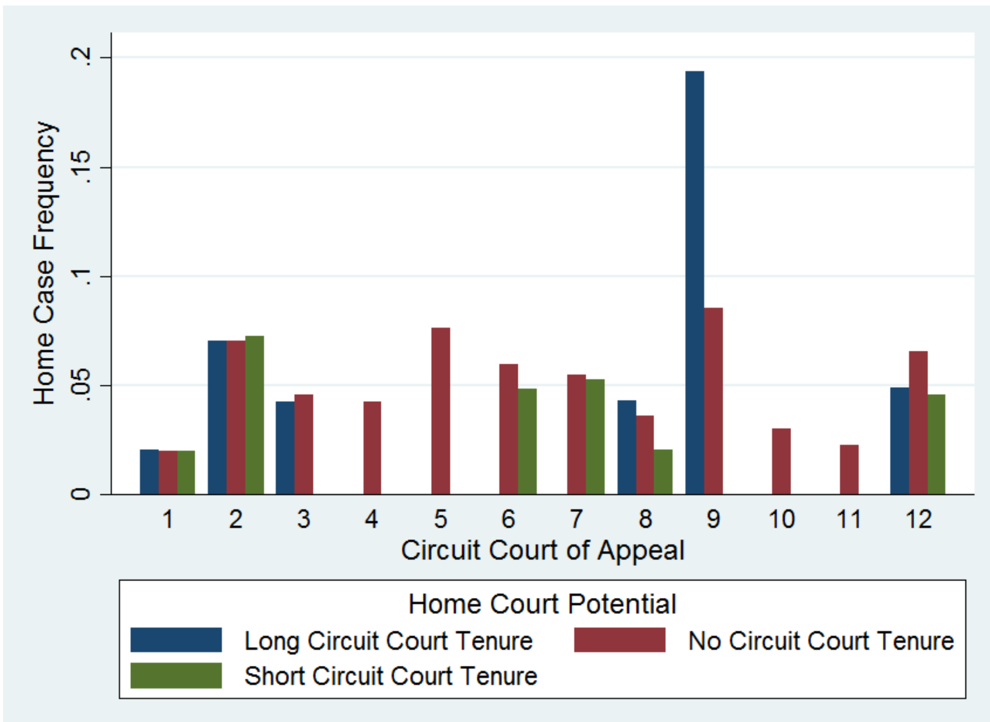


Figure 5 – Home Court Effect in Selection of Circuit Court of Appeals Cases

It is also worth restating that the possible concern that the first stage relationship could be an artifact of recusal behavior does not appear to be merited. As shown in Table 8, the relationship between length of home court tenure and propensity to overturn the Circuit Court's decision holds irrespective of whether all home justices are considered, or only those actively participating in each respective case. This suggests the IV estimates are not being driven by justice decisions about whether to participate or be recused from a case providing information about unobserved (to the econometrician) case facts.

5 The Effect of Peer Votes on Case Outcomes

Sections 3 and 4 have provided strong evidence that peers affect votes. However, if these peer effects do not change pivotal votes, and thus alter the direction of case outcomes by switching majority decisions, they are of diminished practical interest. Accordingly it is necessary to establish whether the peer effects documented above are a general phenomena, or only affect votes in cases that are not tightly decided, such that case majority outcomes are not altered.

A natural first pass is to consider the distribution of the number of votes by Supreme Court justices to overturn the lower court decision of different cases. Since short (long) tenure home justices have reduced (increased) propensities to overturn lower court decisions, we would expect cases with short tenure home justices to have fewer justices on average overturning the lower court decision than those with long tenure home justices, with cases with no home justices falling somewhere in-between. Figure 6 shows the cumulative distribution of the number of overturn votes for these three groups of cases, defining long home tenure as more than 8 years, and restricting the sample to cases voted on by a full panel (9 justices) and including at most one home justice. The number of overturn votes in long home tenure cases first-order stochastically dominates that in short tenure cases, while the distribution for cases with no home justice mostly falls between. The magnitude of the difference is substantial, and stable across the distribution, with lower court decisions overturned 9 percentage points more often in the long home tenure cases.

Notwithstanding the lack of controls, Figure 6 does not in itself tell us anything about peer effects, since it does not disentangle the change in the home justice's own vote from the votes of the other (*away*) justices. Accordingly, Figure 7 isolates the effect on the away justices by plotting the distribution of overturn votes in these cases once the home justice is excluded.[49] Again, the number of overturn votes for cases with a long tenure home justice first-order stochastically dominate those in short tenure home justice cases. But as expected, the exclusion of the home justice reduces the distance between the distributions, with a 4 percentage point difference in the proportion of cases with at least half of the away justices voting to overturn the lower court opinion.

[49] To make cases where there are no home justices comparable, the distribution is calculated by applying equal (1/9) weight to dropping each justice in turn. By comparison, 8-justice cases make a poor placebo group since there is a clear aversion in the data to producing tied votes, which distorts the shape of the cumulative distribution.

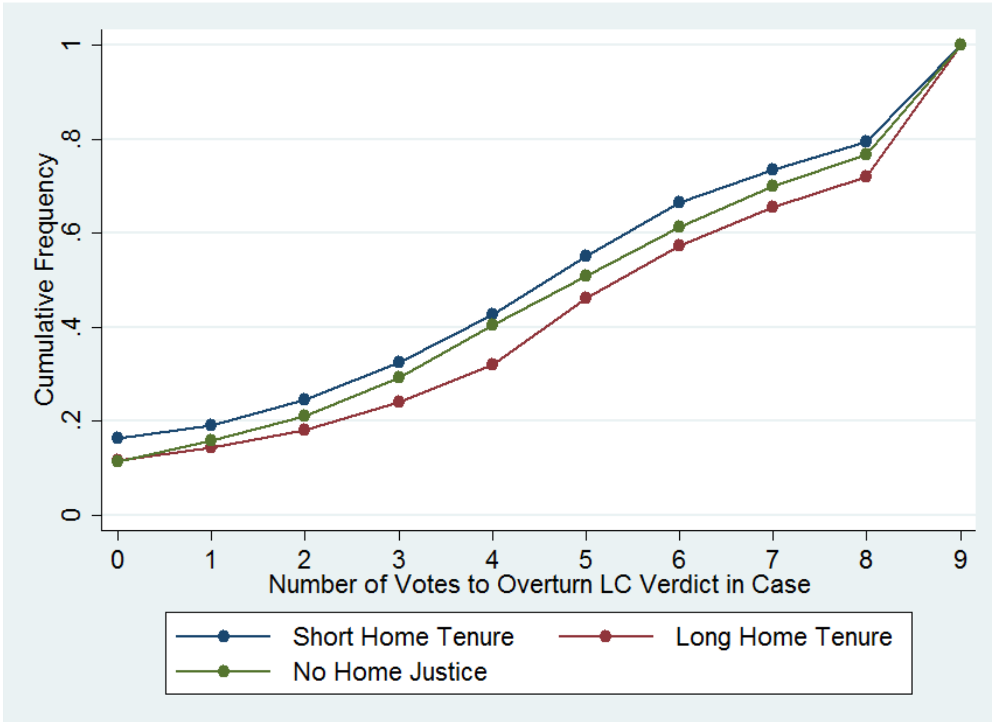


Figure 6 – Distribution of Overturn Votes by Presence of Home Justice

These results are indicative that the difference in Figure 6 is due to effects upon both the home justice's own vote propensity (consistent with the first stage IV results) and the distribution of overturn votes by peers. In particular, both of these figures suggest that peer effects operate at all levels of case closeness, rather than occurring only in one-sided cases.

5.1 Instrumental Variables Estimation Results

To determine whether the peer effects in case outcomes are statistically significant after controlling for covariates, it is possible to use a similar procedure to that discussed in Section 4.2, except that now variables are aggregated at the case level. In particular, all the regression analysis in Sections 3 and 4 considered the effect of peer votes and/or ideology on the votes of a single justice. But identifying whether peer effects change pivotal votes requires that we consider a single justice and analyze how their vote and/or ideology affects the collective voting behavior of their peers. Disentangling peer effects from the mechanical effect of a justice's own vote on the majority outcome requires excluding the vote of the justice whose perspective is taken. As in Section 4,

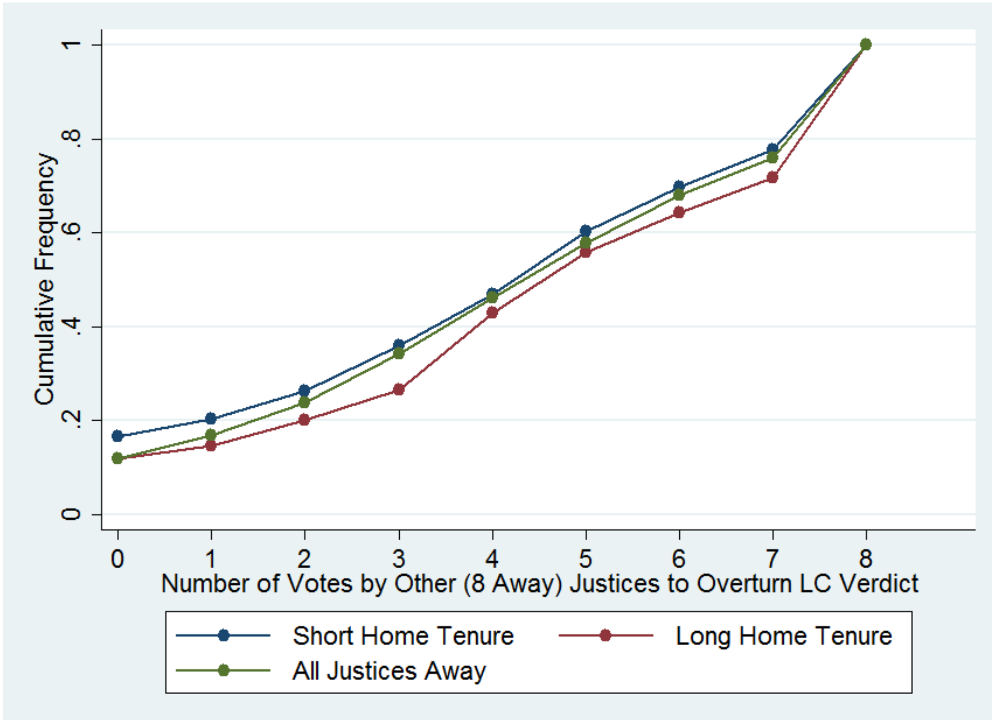


Figure 7 – Distribution of Overturn Votes of Other (Away) Justices by Presence of Home Justice

a justice's own vote is jointly determined with the votes of peers, so the home circuit court variables are again used as instruments which identify exogenous variation in votes. However, due to the change in perspective, such that the individual justice's vote is the regressor of interest, the pertinent instruments are the home court status of this individual justice. Correspondingly, the home court status of peers can be used as an additional control variable to explain variation in the measures of peer voting behavior.

Using this approach, the effects of a justice's own vote on several different measures of the collective votes of peers are considered. First, the number of conservative votes by other justices in a case serves to verify the existence of peer vote effects when analyzed at the case level, and also is potentially informative about the extent of propagation. However it sheds no light on the case circumstances (i.e., types of cases) under which peer effects operate. Second, in order to measure whether peer effects change pivotal votes, we define a case's opinion as *potentially conservative* or *potentially liberal* if, not counting a justice's own vote, enough others (for example, at least four out of eight) vote in the pertinent direction that such a opinion outcome is mathematically possible.[50]

[50]Thus it is possible that a case can have both conservative and liberal potential from the perspective of some justice

Considering the potential outcome variables, rather than the overall majority outcome, enables us to estimate the effect of a justice's own vote on the decision's direction, excluding the mechanical effect of her own vote.

Because we do not want the dependent variables in our regressions to be influenced in a mechanical way by recusals or the number of home justices, we restrict the sample to cases with a full panel of justices, and either zero or one justice at home. In cases with a home justice, full weight is given to the home justice's observation (such that the dependent variable is based only on the votes of the away justices). In cases where all justices are away each is given equal (1/9) weight. This means that in total, each case is weighted equally, and there is no arbitrariness in which justices are included in the dependent or independent variables.

Letting $d_{\text{-}j,ct}$ represents the pertinent collective vote measure of the peer justices and d_{jct} the vote of the justice whose perspective is taken, we estimate regressions of the form:

$$\overline{d_{\text{-}j,ct}} = \alpha_j + \gamma_c + \delta_t + \beta_p \times d_{jct} + l_c + \beta_1 \times lc\_dec_c + \varepsilon_{\text{-}j,ct} \tag{5}$$

A few details of the estimation are worth noting. Since outcomes are considered at the case level (except for the exclusion of the single justice whose perspective is taken) and only full panel cases are considered, the natural control for justice cohorts would be to include fixed effects for the natural court. But these are nearly perfectly collinear with term and are hence omitted.[51] Since the justice whose perspective is taken (and whose vote is thus not included in the dependent variable) varies across (and within) cases for a given cohort, the dependent variable is mechanically affected by the excluded justice's ideology. This is addressed by the inclusion of justice fixed effects. Note that explicitly controlling for excluded justice's estimated ideology to capture justice peer ideology effects is redundant as this is perfectly collinear with the justice fixed effects. Finally, the home circuit court peer variables are dropped since by construction only the votes of away justices are included in the dependent variable.

The results for these analyses are shown in the first column of Table 10, with each row corre-

when they are voting, with their own vote deciding the actual decision direction.

[51] Recall most of the within-year variation in justice cohort is due to recusals, which are excluded since only full panel cases are considered here.

sponding to one of the three alternative outcome measures. The first row presents the coefficient for the effect of the home justice's vote direction on the number of conservative votes by other justices. This result is consistent with previous individual-level analysis; a single (implicitly home) justice switching their vote changes the net votes of their (away) peers by approximately 2.8 votes collectively. While this effect may seem very large, it is not inconsistent with the large effect of peer votes estimated by Fischman (2015) for federal circuit courts. This result highlights the empirical importance of peer effects; the indirect effect of a justice's vote on the total vote outcome through the votes of their peers is several times stronger than the direct mechanical effect of the justice's own vote.

Rows 2 and 3 report the results for the conservative and liberal potential outcome measures (respectively). The home justice switching their vote from liberal to conservative has a substantial peer effect in the same direction, increasing the share of cases with a conservative outcome potential[52] by 36 percentage points and reducing the share with liberal potential by 32 percentage points. This implies that home justice votes do have a large effect on case outcomes.

A second set of regressions expand the sample of cases considered to all cases with a full panel of justices, such that cases with more than one home justice are included, and each justice observation is weighted equally. For each observation the dependent variables are again constructed using the votes of all the other peer justices in the respective case. The specifications considered are as above, except that since the dependent variables can incorporate the votes of home justices, the home circuit court peer variables for the peer justices are added as controls, as in:

$$\begin{aligned}\overline{d_{\text{-}j,ct}} =&\alpha_j + \gamma_c + \delta_t + l_c + \beta_1 \times lc\_dec_c + [\beta_2 + \beta_3 \times lc\_dec_c] \times \frac{1}{N-1} \sum_{i \neq j} I[i \in app_c] \\ &+ [\beta_4 + \beta_5 \times lc\_dec_c] \times \frac{1}{N-1} \sum_{i \neq j} (I[i \in app_c] \times app\_yrs_i) + \beta_p \times d_{jct} + \varepsilon_{\text{-}j,ct}\end{aligned} \tag{6}$$

The estimates from these specifications are shown in Column 2 of Table 10. In each case the results are similar to those with the restricted sample in Column 1, and again provide strong evidence that peer effects shift pivotal votes. According to the point estimates, an individual jus-

[52] For example, shifting the vote of other justices from 3-5 or less to 4-4 or more.

tice switching their vote from liberal to conservative increases the probability that the other justices collectively vote in a manner that produces conservative outcome potential by 32 percentage points, and decreases liberal outcome potential by 40 percentage points.

Table 10 – Peer Vote Effects on Verdict Direction Outcomes IV

Coefficients on Own Vote Direction

	(1)	(2)
Dependent Variable	0-1 Home Justices	Any # Home Justices
Conservative Peer Votes	2.781***	3.037***
	(0.683)	(0.750)
Conservative Potential	0.361***	0.321**
	(0.128)	(0.152)
Liberal Potential	-0.323**	-0.397***
	(0.133)	(0.146)
Observations	67576	84267
First Stage F-Statistic	11.792	9.755

All regressions restricted to cases with a full panel (9) of justices. Each cell reports the IV coefficient estimate (and standard error) of the respective dependent variable on a justice's own vote direction. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

While both sets of estimates find large effects on the potential ideological direction of case opinions, a plausible argument can be made that these estimates are biased towards zero. This possibility arises because, due to the need to exclude a justice's own vote, cases can be categorized as having both conservative and liberal potential. Suppose justices do not care purely about the margin of victory, but attach additional importance to winning any majority. A justice who plans to issue a conservative (liberal) vote has great incentive to attempt to influence the votes of peers to achieve at least a 4-4 split, and hence *cause* a *potentially conservative (liberal)* outcome. Once this is achieved, however, they would have a lesser incentive to go further and *prevent* a *potentially liberal (conservative)* outcome (requiring a split of at least 5-3), because their desired majority outcome is achieved regardless due to their own vote. Such a mechanism would systematically reduce the between-group difference in the outcome potential measures and hence bias the estimated effect of the considered justice's vote downwards. Note that this relates to reduced effort (once a majority is achieved), and could arise even if justices exert full effort to try to win a majority.

6 Discussion and Conclusion

We have presented a variety of estimates suggesting that the ideology and voting behavior of a justice affects the voting behavior of other justices. Moreover, our estimates imply that these peer effects can be pivotal and thus affect case outcomes–and the magnitudes are meaningful.

This raises the question of why these effects exist and what drives them. As we mentioned in the introduction, a variety of explanations have been offered in the context of lower courts, including: deliberation, group polarization, aversion to dissent, or deference to expertise. It is challenging to provide compelling evidence distinguishing between these different channels. We do know, by virtue of the fact that peer effects can cause a change in outcome, that dissent aversion (not wanting be an outlier justice on a case) cannot be the whole story.

Having said that, it is not easy to distinguish between justices persuading each other, being deferential to each other on areas of expertise, or even some form of *horse trading*. We can, however, get some sense of whether Posner (2008)'s *deference effect* is at work. Under that hypothesis, roughly put, justices defer to other justices who have expertise in a certain area of law, other than for highly politicized issues. As Posner puts it: "The hotter the issue (such as abortion, which nowadays is much hotter than, say, criminal sentencing), the greater the explanatory power of the political variable."

In the following table, we estimate our peer effect coefficient (including justice by issue area and term by issue area fixed effects) separately for the 11 issue areas. To facilitate precise estimates, we estimate regressions of a justices own vote on the mean peer vote (along with the same control variables used previously in Section 4), but with ideology excluded. That is, we assume there are only endogenous peer effects. Identification for each issue area comes from using the mean active justice ideology estimates in that issue area as instruments for the mean vote of peers. A first thing to note is that the 11 issue areas are fairly coarse categories that typically include some "hot" issues and some less controversial ones. Second, some of the first stage F-statistics indicate weak instrument problems, and some of the standard errors are large (the Privacy and Unions coefficients, for instance, are almost completely uninformative).

Notwithstanding these issues, it is noteworthy that, relative to the average coefficient of about 0.6, the issue areas with stronger peer effects include: Attorneys, Economic Activity, Judicial Power and Federal Taxation, all of which are arguably on the "cooler" end of the political spectrum. Conversely, First Amendment, Civil Rights and Due Process have lower-than-average coefficients, and the areas are arguably on the "hotter" end of the political spectrum.[53]

Table 11 – Peer Effects by Issue Area - Justice by Issue Area, Term by Issue Area FE

	Coefficient	Standard Error	First Stage F-Statistic	Observations
Unions	-0.109	2.594	0.179	4387
Civil Rights	0.346	0.350	3.370	18435
Due Process	0.442	0.535	1.427	4975
First Amendment	0.547***	0.162	8.674	9895
Criminal Procedure	0.601***	0.077	29.143	22549
Economic Activity	0.674***	0.105	11.508	21447
Attorneys	0.724***	0.196	3.239	1122
Federal Taxation	0.760***	0.145	4.123	3415
Judicial Power	0.806***	0.067	10.020	17041
Federalism	0.853***	0.092	3.685	5805
Privacy	2.096	2.376	0.226	1483

Coefficients of mean peer vote on justice vote. Coefficients calcualted from separate regressions for each issue area, using ideology of active peer justices for that issue area as instrument. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

There may be less "deference to expertise" on the Supreme Court compared to Circuit Courts of Appeals because, given the types of cases the Supreme Court hears, it is less likely that a justice has particular expertise in the area. Justice Breyer and administrative law is an example of a justice with particularly relevant expertise, but it is not easy to think of a large number of such examples. Nonetheless, the "hot button" issue effect that Posner conjectures, and that we provide some evidence for, could still operate in the absence of deference to expertise. It could simply be that on "hot button" cases justices decide ideologically, and on other cases they are more persuadable by their colleagues.

It would be highly desirable to precisely estimate our coefficient for each "issue"–which is more granular than "issue area"–but given our identification strategy and the number of cases, it is not possible to draw meaningful conclusions from that exercise.

[53]We again emphasize caution in interpreting the Civil Rights and Due Process coefficients given the large standard errors.

Our estimates of peer effects also speak to the broader issue of the optimal strategy for a president nominating a justice. This requires balancing the proximity of the justice's ideology to that of the president, with the effect they will have on their peers. An immediate implication of this is that optimal nominations are "court specific" in the sense that they depend on the existing justices, as well as presidential preferences. Here it is necessary to note that optimal appointment will depend on heterogeneity or nonlinearity in peer effects. While our estimates are of the average proportional treatment effect of being exposed to a change in peer ideology or voting disposition, it is plausible that the ability of one justice to convince another diminishes as they become ideologically distant. Convincingly identifying these nonlinearities is difficult, and thus left for future work.

Since home court justices are so influential, an intriguing implication of our results is that–for given ideology–a President should prefer to appoint a circuit court judge. Furthermore, for a conservative president it would be ideal to select either a long-serving conservative judge from a liberal circuit court, or a short-serving conservative judge from a conservative circuit court.

Finally, the magnitude of the peer effects that we estimate implies that the indirect effect of a justice's vote on the outcome through the votes of their peers is several times larger than the direct mechanical effect of the justice's own vote. Thus, the replacement for a particularly liberal or conservative justice is particularly consequential in that it has the potential to have a large impact on case outcomes.

Appendices

A Justice Ideology Point Estimates

Table 12 orders the justices from the 1946-2013 period according to their estimated ideology, from most liberal to most conservative. The estimates from the first model in Section 3 are used to provide a single point estimate per justice. This model controls for term fixed effects and thus the justice estimates abstract from joint ideological drift in the views of justices and secular changes in the ideological composition of cases heard by the Supreme Court. Rather than ranking justice ideology in an absolute sense, this attempts to measure the ideology of justices relative to their social milieu. Alternatively, by accounting for time effects that would affect any justice serving in an equivalent context, these ideology scores are interpretable as estimating the relative ideologies of any set of justices had they counterfactually been on the Supreme Court at the same time.

B Peer Effect Measure and Justice Ideology

We remarked in the text that the justice ideology measures obtained in the first stage are contaminated by any peer effects of other justices, and that this in turn causes our peer ideology measures to be contaminated by a justice's own ideology. However, this contamination is washed out when we use fixed effects in the second-stage regressions. We now demonstrate this formally.

Let α_j^p and $\bar{\alpha}_{-j}^p$ denote our proxies for justice ideology and the peer variable, respectively. Now let votes, which are influenced by the true measures α_j and $\bar{\alpha}_{\text{-}j}$, follow

$$d_{jc} = \alpha_j + \beta\bar{\alpha}_{\text{-}j} + \varepsilon_{jc}.$$

If court composition is unchanged during the tenure of justice j then $\bar{\alpha}_{\text{-}j}$ is constant during her tenure. So if we estimate

$$d_{jc} = \alpha_j^p + \xi_{jc},$$

we will obtain (in large samples, by the Khintchine Law of Large Numbers (hereafter "KLLN"))

Table 12 – Justice Ideology Estimates

Justice	Ideology Estimate	Segal-Cover Score	Conservative Vote Proportion	Party of Appointing President
W. O. Douglas	0.1784	0.730	0.2154	Democratic
W. B. Rutledge	0.1864	1.000	0.2336	Democratic
F. Murphy	0.1940	1.000	0.2424	Democratic
T. Marshall	0.2176	1.000	0.2802	Democratic
W. J. Brennan	0.2438	1.000	0.2930	Republican
H. L. Black	0.2483	0.875	0.2820	Democratic
A. Fortas	0.2517	1.000	0.3082	Democratic
E. Warren	0.2583	0.750	0.2703	Republican
A. J. Goldberg	0.2735	0.750	0.2404	Democratic
J. P. Stevens	0.3508	0.250	0.3889	Republican
R. B. Ginsburg	0.3937	0.680	0.3863	Democratic
H. A. Blackmun	0.4174	0.115	0.4790	Republican
D. H. Souter	0.4174	0.325	0.4183	Republican
S. Sotomayor	0.4174	0.780	0.3712	Democratic
S. G. Breyer	0.4263	0.475	0.4160	Democratic
E. Kagan	0.4403	0.730	0.3963	Democratic
P. Stewart	0.4533	0.750	0.5046	Republican
T. C. Clark	0.4591	0.500	0.4764	Democratic
B. R. White	0.4680	0.500	0.5201	Democratic
F. M. Vinson	0.4967	0.750	0.5635	Democratic
F. Frankfurter	0.5022	0.665	0.5394	Democratic
S. Minton	0.5217	0.720	0.5688	Democratic
S. F. Reed	0.5242	0.725	0.5708	Democratic
H. H. Burton	0.5260	0.280	0.5669	Democratic
L. F. Powell	0.5327	0.165	0.6084	Republican
C. E. Whittaker	0.5353	0.500	0.5516	Republican
R. H. Jackson	0.5420	1.000	0.6157	Democratic
J. Harlan II	0.5471	0.875	0.5729	Republican
W. E. Burger	0.5755	0.115	0.6574	Republican
S. D. O’Connor	0.5927	0.415	0.6245	Republican
A. M. Kennedy	0.6042	0.365	0.6042	Republican
J. G. Roberts	0.6430	0.120	0.6126	Republican
W. H. Rehnquist	0.6659	0.045	0.7134	Republican
A. Scalia	0.6804	0.000	0.6793	Republican
S. A. Alito	0.6992	0.100	0.6653	Republican
C. Thomas	0.7221	0.160	0.7157	Republican

the following proxy

$$\alpha_j^p = \alpha_j + \beta\bar{\alpha}_{\text{-}j}.$$

We now construct the following:

$$\begin{aligned}
\bar{\alpha}_{\text{-}j}^p =& \frac{1}{N-1}\sum_{k\neq j}\alpha_k^p \\
=& \frac{1}{N-1}\sum_{k\neq j}\left(\alpha_k + \beta\bar{\alpha}_{\text{-}k}\right) \\
=& \left(\frac{1}{N-1}\sum_{k\neq j}\alpha_k\right) + \beta\left(\frac{1}{N-1}\sum_{k\neq j}\bar{\alpha}_{\text{-}k}\right) \\
=& \bar{\alpha}_{\text{-}j} + \frac{\beta}{N-1}\left(\frac{1}{N-1}\left\{(\alpha_2 + ... + \alpha_j + ... + \alpha_N) + (\alpha_1 + \alpha_3 + ... + \alpha_j + ... + \alpha_N) + ...\right.\right. \\
& \left.\left. + (\alpha_1 + ... + \alpha_j + ...\alpha_{N-2})\right\}\right) \\
=& (1+\beta)\bar{\alpha}_{\text{-}j} + \frac{\beta}{N-1}\left(\alpha_j - \bar{\alpha}_{\text{-}j}\right) = \left(1 + \frac{N-2}{N-1}\beta\right)\bar{\alpha}_{\text{-}j} + \frac{\beta}{N-1}\alpha_j.
\end{aligned}$$

This expression makes clear what is meant by saying that the peer effect measure is contaminated by a term due to the justice's own ideology (the $\frac{\beta}{N-1}\alpha_j$ term).

Now suppose that justice j is observed sitting on a number of different courts $g = 1, ..., G$, each with a different (but typically overlapping) group of $N-1$ other justices being concurrently appointed to the court. This allows the exposure of a particular justice to another to vary across cases and across justice pairs, while within a group g, composition of the court may still vary due to absences.

The true model is now

$$d_{jc} = \alpha_j + \beta\bar{\alpha}_{\text{-}j,g} + \varepsilon_{jc}.$$

If one estimates

$$d_{jc} = \alpha_j^p + \xi_{jc},$$

with a large number of cases serving alongside each peer justice, the KLLN implies that one obtains

$$\alpha_j^p = \alpha_j + \beta\left\{\pi_1^j\alpha_1 + \pi_2^j\alpha_2 ... + \pi_n^j\alpha_n\right\}$$

where π^j_k is the exposure weight of justice j to justice k, with $\sum_i \pi^j_i = 1$ and zero direct exposure to self, $\pi^j_j = 0$.

Let us now construct $\bar{\alpha}^p_{\text{-}j,c,g}$, the mean ideology that j faces in a case c with cohort g.

$$
\begin{aligned}
\bar{\alpha}^p_{\text{-}j,c,g} &= \frac{1}{N_c - 1} \sum_{k \neq j} \alpha^p_k I^c_k. \\
&= \frac{1}{N_c - 1} \sum_{k \neq j} \alpha_k I^c_k + \frac{\beta}{N_c - 1} \sum_{k \neq j} \left(\pi^k_1 \alpha_1 + \pi^k_2 \alpha_2 ... + \pi^k_j \alpha_j ... + \pi^k_n \alpha_n \right) I^c_k
\end{aligned}
$$

where I^c_k is an indicator for the presence of justice k in a case c. In the peer contamination term, the sum of exposure weights sum to $N_c - 1$, since $\sum_i \pi^k_i = 1$ for each i), hence the average peer contamination is divided by $N_c - 1$.

Note that the exposure of peers of j to the ideology of j can be separated out,

$$
\begin{aligned}
\bar{\alpha}^p_{\text{-}j,c,g} &= \frac{1}{N_c - 1} \sum_{k \neq j} \alpha_k I^c_k + \frac{\beta}{N_c - 1} \sum_{i} \left(\sum_{k \neq j} I^c_k \pi^k_i \right) \alpha_i && (7) \\
&= \frac{1}{N_c - 1} \sum_{k \neq j} \alpha_k I^c_k + \frac{\beta}{N_c - 1} \sum_{i \neq j} \left(\sum_{k \neq j} I^c_k \pi^k_i \right) \alpha_i + \frac{\beta}{N_c - 1} \left(\sum_{k \neq j} I^c_k \pi^k_j \right) \alpha_j && (8) \\
&= \frac{1}{N_c - 1} \left(\sum_{k \neq j} \alpha_k \left(I^c_k + \beta \sum_{i \neq j} I^c_i \pi^i_k \right) \right) + \frac{\beta}{N_c - 1} \left(\sum_{k \neq j} I^c_k \pi^k_j \right) \alpha_j && (9)
\end{aligned}
$$

Here the first term captures the true peer mean ideology, the second the contamination via total exposure of current peer justices (not j) to all justices except j who they ever coincided with on the court, while the final term captures each of the peers reflective exposure to j.

Now suppose we run the regression

$$
d_{jcg} = \gamma_j g + \theta \bar{\alpha}^p_{\text{-}j,g} + \omega_{jc}.
$$

where $\gamma_{j,g}$ are justice by group fixed effects in this second-stage estimation and θ is the key estimated parameter that captures peer effects.[54]

As this estimation incorporates justice by group fixed effects, let us de-mean (8) over the T

[54] For example, g here can categorize the intersection of issue area and natural court.

cases c within each cohort g by justice j pair.

$$\overline{\bar{\alpha}^p_{\text{-}j,c,g}} = \frac{1}{T}\sum_c \left(\frac{1}{N_c-1}\sum_{k\neq j}\alpha_k I^c_k\right) + \frac{1}{T}\sum_c \left(\frac{\beta}{N_c-1}\sum_{i\neq j}\left(\sum_{k\neq j} I^c_k \pi^k_i\right)\alpha_i\right) + \frac{1}{T}\sum_c \frac{\beta}{N_c-1}\left(\sum_{k\neq j} I^c_k \pi^k_j\right)\alpha_j$$

To make this tractable, assume that justice absences are independent and equally likely within g. This yields that each justice k is equally exposed to each other justice (π^k_i is common across k), including j whose peer mean is under consideration.[55]

It follows that the α_j term collapses to $\frac{\beta}{N-1}\alpha_j$, which is constant across cases within g, and thus drops out upon demeaning. Further, the change in direct peer ideology that occurs when justice absences occur always coincide with a change in contamination effects of $-\frac{\beta}{N-1}$ multiplied by the change in true peer ideology. Thus within group g variation in the peer mean proxy deflates the true peer mean ideology variation.

It follows that

$$\bar{\alpha}^p_{\text{-}j,c,g} - \overline{\bar{\alpha}^p_{\text{-}j,c,g}} = \left(1 - \frac{\beta}{N-1}\right)\left(\bar{\alpha}_{\text{-}j,c,g} - \overline{\bar{\alpha}_{\text{-}j,c,g}}\right),$$

and observe that the α_j drops out as claimed.

This leaves us with the fixed-effects regression:

$$d_{jcg} - \overline{d}_{jcg} = \theta\left(1 - \frac{\beta}{N-1}\right)\left(\bar{\alpha}_{\text{-}j,c,g} - \overline{\bar{\alpha}_{\text{-}j,c,g}}\right) + \left(\omega_{jc} - \bar{\omega}_{jc}\right), \tag{10}$$

where the first parenthetical term on the right-hand side is the *attenuation factor* and the second parenthetical terms is the "correct" regressor.

In large samples we obtain

$$\beta = \theta\left(1 - \frac{\beta}{N-1}\right),$$

and thus

$$\theta = \beta / \left(1 - \frac{\beta}{N-1}\right). \tag{11}$$

[55] This simplifies the math considerably, but has only second order numerical effects. Since absences are rare, even substantial correlation of absences and variation in frequencies produces little variation in π^i_k, and thus differences between aggregate contamination across different cases.

Therefore θ is consistent for β if $\beta = 0$, it is *attenuated* if $\beta < 0$ and it is *exaggerated* if $\beta > 0$. Note that tests for the existence of peer effects will still be consistent, as $\beta = 0$ under the null (see Wooldridge (2010, pp.158-160), where in his notation, $G = 0$ so 2SLS standard errors and test statistics are valid).

Note that in our case $N = 9$. So for example, if the true parameter β were equal to 1 then, in large samples, the $plim_{n\rightarrow\infty}$ of the fixed effects estimator of θ would be $8/7$ (as the attenuation factor is $7/8$). This illustrates the sense in which our fixed effects estimates of peer effects in the models where ideology is separately estimated by natural court, within which court composition is fixed except for absences, are slightly inflated.

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