Essays on the Federal Judicial Hierarchy

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Essays on the Federal Judicial Hierarchy

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Essays on the Federal Judicial Hierarchy

Abstract

This dissertation considers the multiple ways that the hierarchical structure of the U.S. federal judiciary constrains (or fails to constrain) judicial behavior. How much are lower court judges constrained by courts above them in the hierarchy and by their own colleagues? Political science research has considered the pressures under which federal judges operate, but has not come to a definitive conclusion about when and why these pressures affect lower court decision-making. The first paper considers whether judges on the Courts of Appeals respond to changes in the ideological compositions of the circuits on which they sit. I show that circuit judges are influenced by other members of their circuit; in fact, circuit and panel ideology are larger predictors of circuit judges' behavior than a judge's own ideology. I argue that this derives from an unusual institutional feature of the circuit courts, where circuit judges sitting on panels are bound by the precedential decisions of other panels. In the second paper, co-authored with Michael Olson, we investigate how changes in the composition of appellate court panels affects *district* court voting. District court judges face a much greater rate of review than do circuit court judges. We find that district court judges vote more liberally when they face more liberal circuits. Crucially, this is limited to district-years when the rate of appellate review is high; when it is low, district court judges are not affected by circuit ideology, suggesting that it is indeed the hierarchical structure of the federal courts that drives this responsiveness. In the third paper, I consider the relationship between the circuit courts and the Supreme Court by looking at resolved

circuit splits. This work suggests that the Supreme Court's ability to constrain lower court behavior is limited by the low rate of review of circuit cases; only when the rate of review increases (after the first case in a circuit split) do we see any congruence between circuit court behavior and Supreme Court behavior.

Contents

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Introduction

This dissertation explores how the institutional structure of the U.S. federal courts shapes lower court behavior. District court judges and circuit court judges both face hierarchical pressures from judges above them. Furthermore, circuit judges sitting in panels of three are able to issue precedents that are binding on future panels, which can lead to horizontal influence of circuit court judges on each other. Under what conditions do these pressures affect the decision-making of lower court judges? I approach these questions by developing a theoretical framework for thinking about the various pressures on lower court judges, collecting new data on circuit court decision-making, and developing new measurements of circuit ideology and agendas.

In the first paper, "Appellate Judges' Responsiveness to Changes in the Composition of Their Circuits," I consider the ways in which changes in the composition of the U.S. Courts of Appeals affect judicial behavior. While previous research has focused extensively on how the composition of three-judge panels affects judicial decision making, less is known about how judges respond to broader changes in the composition of their circuits. I find that judges do respond to such changes: as a circuit grows more conservative through the appointment process, individual judges on that circuit vote more conservatively. I further test the mechanisms behind this relationship, and I find evidence consistent with the hypothesis that this is a result of judges responding to precedent. The institutional rule that three-judge panels can issue binding precedents therefore induces responsiveness by

individual judges to changes in the ideological views of their circuits.

The second paper, "Appellate Court Influence over District Courts in the United States," co-authored with Michael P. Olson, considers the hierarchical pressure that circuit judges exert on district judges. District judges face the possibility of review and reversal by circuit judges. We consider across a broad range of cases whether district judges try to avoid reversal, and, if so, whether they do so for instrumental or non-instrumental reasons. We find evidence that district judges change their behavior when the composition of the circuit courts change and that this is driven by a desire to avoid reversal. However, we do not find evidence that district judges do this in order to reduce their workload or to improve their chances of promotion to a higher court.

Finally, I turn to another relationship within the judicial hierarchy in the third paper, "Does the Supreme Court Constrain Circuit Court Behavior?" While district judges face a relatively high probability of review and reversal by the circuit courts, circuit judges are rarely reviewed by the Supreme Court. This paper considers whether the low rate of review can account for the relative lack of responsiveness seen in circuit judges' decision making to changes in the Supreme Court's ideology. By looking at the development of resolved circuit splits (where multiple circuits considered the same legal issue) over time, I can see whether the first circuit to hear a case is more or less likely to agree with the Supreme Court than subsequent circuits, which face higher rates of review. I find that there is a difference between the first and subsequent circuits, suggesting that the low rate of review does affect circuit court behavior. Still, the difference is relatively modest, highlighting that circuits act with a great deal of independence even in the rare cases where Supreme Court review looms large.[1]

[1]The quantitative data used in this dissertation will be made available after an embargo at https://doi.org/10.7910/DVN/ZLOGYE, UNF:6:1uauSwIQTGHapL6M/CXJpw==.

1 Appellate Judges' Responsiveness to Changes in the Composition of Their Circuits

How much do changes in the composition of the federal bench affect the decision-making of the U.S. Courts of Appeals? It is well known that ideology predicts judicial voting (Segal and Spaeth 2002), and thus it is uncontroversial to state that new appointees may vote differently from the judges they replaced. However, that is not the only way in which changes in circuit composition can affect judicial behavior, because the departure or appointment of one judge may have *spillover effects* on the behavior of other judges. The circuit courts have strong collegial dynamics (Hettinger, Lindquist, and Martinek 2006); most cases are heard in panels of three, and judges' decision-making can be affected by their immediate copanelists (e.g., Boyd, Epstein, and Martin 2010; Hinkle 2017; Kastellec 2013; Kim 2009; Sunstein et al. 2006). Less well understood is how judges may be affected by changes in the composition of their circuits beyond just their copanelists on a given case. While some previous work has shown a relationship between circuit ideology and individual judges' voting (e.g., Cross 2007; Epstein, Landes, and Posner 2013), the mechanisms underlying responsiveness to circuit ideology have not been directly tested.

In this paper, I develop a theory of why judges may care about their circuit context. Crucially, this involves a judge's relationship to other *panels* on one's circuit, but previous measures of circuit ideology have not accounted for the panel decision-making process. I

employ a measurement strategy for circuit ideology that considers this institutional context, and I use this measure to test whether changes in circuit composition lead to within-judge changes in voting behavior. With this approach, I am able to test the theoretical bases for circuit responsiveness. In particular, I hypothesize that collegiality pressures lead judges to care not only about the views of their immediate colleagues on a panel, but also about the views of other judges on the circuit, given that judges will have repeated interactions with those circuit colleagues over time. However, I further hypothesize that judges may follow the preferences of their circuit for institutional reasons beyond collegiality: the views of other judges on one's circuit are likely to be reflected in circuit precedent, most of which is created by panels. The rule that future panels are bound by the decisions of previous panels thereby induces judges to respond to the changing views of their circuit colleagues.

I find that in panel decisions, judges respond to the compositions of their own circuits, both through responsiveness to copanelists (the well-known phenomenon of panel effects) and through responsiveness to the composition of the circuit as a whole. Furthermore, this is driven by issues that appear more frequently on the circuit courts' dockets; on less routine issues, the preferences of the circuit matter less. On these high prevalence issues, changes in circuit composition are more likely to be reflected in recent circuit precedent. Thus, the finding of responsiveness in this context is consistent with the hypothesis that circuit judges respond to changes in the ideological compositions of their courts in part because of precedents issued by their colleagues.

The finding that judges on panels respond to changes in the ideological composition of their circuits is reinforced by an analysis conducted using a novel dataset of resolved circuit splits. In those data, where concerns about agenda effects are lessened, we see that panel responsiveness to circuit ideology is greater than in typical cases.

Thus, I find that appointments to circuit courts create spillover effects, affecting the behavior of judges beyond the appointee. This has important implications, given the large

role circuit courts play in the American legal system and the power presidents have to affect the composition of these courts. Even one-term presidents can have a large effect, especially if aided by Congress. By 1980, Jimmy Carter had appointed over forty percent of federal judges then serving (Barrow, Zuk, and Gryski 1996, 85) despite making no Supreme Court appointments; in 2020, Donald Trump's 200th judicial appointment received much media attention.[1] Furthermore, about 50,000 cases commenced in the circuit courts in the twelve months leading up to September 31, 2020.[2] Given that the vast majority of circuit cases are not reviewed by the Supreme Court,[3] the circuit courts thus have the *de facto* final say on much of federal law. To understand the consequences of these appointments, we must understand not only how these judges are likely to vote, but the likely effect they will have on the behavior of the other judges with whom they serve.

The paper proceeds as follows. First, I present a theory of responsiveness by judges sitting on panels to changes in circuit court composition and develop a measure of circuit conservatism that takes into account the panel decision-making process of the Courts of Appeals. Then, I present empirical evidence that judges on panels respond to changes in the composition of their circuits, using the U.S. Courts of Appeals Database to investigate cases from 1965–2002, and test whether this relationship is driven by precedent. Finally, I employ a novel dataset of resolved circuit splits from 1985–2017 to test the robustness of the finding that judges respond to changes in circuit composition after taking greater account of agenda effects.

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[1] E.g., https://tinyurl.com/4vzx6jx8.

[2] https://tinyurl.com/2rr2426y.

[3] For example, in the October 2019 Term, the Supreme Court only granted 71 petitions for argument. See Feldman (2020).

1.1 Spillover Effects of New Judicial Appointees

Three First Circuit[4] judges with long careers on the bench provide instructive examples on how changes in circuit composition can have spillover effects on the behavior of other judges on that circuit. Bailey Aldrich, Frank Coffin, and Levin H. Campbell all served as either active or senior status judges for (almost) all of the 1970s, 1980s, and 1990s.[5] What changed from the 1970s to the 1990s in the First Circuit? Most notably, the composition of the court changed. There was never more than one active judge at a time on the court in the 1970s who was a Republican appointee, but a combination of the creation of new seats and retirements led to five of the six active judges being appointed by Republican presidents by 1990; then Bill Clinton only made two appointments to the court, one of whom replaced a fellow Democratic appointee. Thus, while no panel consisting entirely of active judges in the 1970s could be majority Republican, all such panels from early 1990 to early 1998 were majority Republican.

Was this change in the composition of the First Circuit associated with changes in voting for Aldrich, Coffin, and Campbell? All three of these judges show considerable conservative shifts in their voting records over time. Bailey Aldrich went from a 52% conservative voting record in the 1970s to 68% in the 1980s and 77% in the 1990s.[6] Frank Coffin had 57% conservative voting in the 1970s, 62% in the 1980s, and 77% in the 1990s. Levin H. Campbell shows a similar pattern: 59% in the 1970s, 61% in the 1980s, and 78%

[4]This circuit covers Maine, Massachusetts, New Hampshire, Puerto Rico, and Rhode Island.

[5]Bailey Aldrich was appointed by President Eisenhower in 1954 and served as an active judge through 1972 and had senior status through 2002. Frank Coffin was appointed by President Johnson in 1965, took senior status in 1989, and continued to serve until 2009. Levin H. Campbell was appointed by President Nixon in 1972 and took senior status in 1992. Biographical information on judges available from *History of the Federal Judiciary.* http://www.fjc.gov. Web site of the Federal Judicial Center, Washington, DC.

[6]From the U.S. Courts of Appeals Database (Songer 2008; Kuersten and Haire 2011). These data consist of a random sample of published opinions and are limited here to decisions on three-judge panels. This dataset is described further in the "Data and Methods" section.

in the 1990s.

Of course, a broader analysis is necessary to understand the spillover effects of changes in circuit composition. It is possible that the agenda before the First Circuit changed during this time, confounding the relationship between circuit composition and judicial voting. In addition, the above examples don't distinguish between two ways in which circuit composition could affect judicial behavior. Judges may vote more conservatively when their copanelists become more conservative, which is an example of the phenomenon of panel effects. Judges may also vote more conservatively when their circuit grows more conservative even after adjusting for panel composition, since circuits that are more conservative will develop more conservative lines of precedent and approaches to the law. In the analyses to come, this paper measures both of these ways in which circuit composition may affect judicial behavior.

1.1.1 How Can Changes in Circuit Composition Affect Judicial Voting?

There are three main channels through which a new circuit court appointee may affect circuit court voting. The first channel is direct: the new judge may vote differently from the judge he or she replaced. It is well known that Republican appointees vote more conservatively and Democratic nominees more liberally. Judicial behavior may also be influenced by judicial attributes beyond ideology, such as personality (Hall 2018) or life experiences (Glynn and Sen 2015). Thus, presidents can have a direct effect on the Courts of Appeals simply by appointing a judge whom they expect will vote in their preferred direction.

The next two channels are what I will call "spillover effects," where a judge affects judicial outputs through influencing the behavior of other judges.[7] The first kind of spillover

[7]I use the term "spillover effect" broadly to capture the idea that a new appointment can lead to spillover

effect is that the judge may shift the composition of the panels on which he or she sits. These "panel effects," the phenomenon where judges are influenced by the other judges with whom they sit on particular panels, have been well studied in the judicial behavior literature. Panel effects have been observed in particular issue areas with respect to gender (Boyd, Epstein, and Martin 2010), race (Kastellec 2013), and religion (Shahshahani and Liu 2017). Crucially, these effects exist across many issue areas with respect to panel party composition (Sunstein et al. 2006). In the Sunstein et al. (2006) data, Republicans on all-Republican panels voted liberally 31% of the time but they did so 46% of the time on majority-Democratic panels; Democrats on all-Democratic panels voted liberally 64% of the time but only 44% of the time when on majority-Republican panels. These are large effects: Democrats on majority-Republican panels look like Republicans on majority-Democratic panels. Thus, because of panel effects, a Republican (Democratic) judge will likely induce more conservative (liberal) voting behavior by his or her copanelists.

The second kind of spillover effect is that the judge may affect the behavior of other judges even when they do *not* sit together. The appointment of a judge to the U.S. Courts of Appeals may affect the behavior all other judges sitting on the circuit, even when the judge is not present on a panel.[8] I argue that this is in part due to the formal structure of the circuit courts. Circuit judges have power over other judges in their circuit, most importantly through the ability to issue binding precedents that other judges (in principle) must follow and secondarily through the ability to hear cases *en banc*. Circuit judges themselves

behavior on judges already present on the circuit, but responsiveness to judges sitting on the same panel may more precisely be thought of as a peer effect.

[8]To save space, I speak of this as the effect of appointments, but circuit composition can change both through appointments and departures. Indeed, it is possible that judges react more quickly to the ideological shift occasioned by a departure than the shift from the subsequent appointment, given that the incoming judge may for a time experience "acclimation effects" (Hettinger, Lindquist, and Martinek 2003) and thus vote less ideologically than other judges. While it is difficult to distinguish between the effect of departures and appointments given the relatively short time period on average between a departure and subsequent appointment, this would be an interesting area for future research.

have identified the views of both copanelists and other non-copanelist judges as important in their decision-making. In Howard's (1981/2014, 151) study, 22 of 35 judges interviewed considered the views of copanelists to be "very important" and 12 of 35 "moderately important," while 7 of 35 judges thought that the views of other judges were "very important" and 18 of 35 "moderately important."

Yet the extant empirical evidence is mixed on whether judges respond to circuit ideology beyond panel effects. Broscheid (2011), looking at case outcomes in panel decisions from the U.S. Courts of Appeals Database (Songer 2008; Kuersten and Haire 2011), does not find a significant effect of circuit ideology in cases from 1993–2000. However, other studies, using an individual judge's vote as the dependent variable and employing different data and methods, have found such effects (Cross 2007; Epstein, Landes, and Posner 2013). These studies have not directly considered whether these effects operate within judge, however, which is important to understanding whether the relationship is causal.[9] Furthermore, the mechanism behind these effects, if present, remains unclear. Judges may care about the views of others on their circuits for multiple reasons (e.g., Epstein, Landes, and Posner 2013, 183). One is that judges may wish to maintain positive relationships with their colleagues; it is well understood that collegial norms affect circuit judges' behavior (Hettinger, Lindquist, and Martinek 2006). This is likely a primary mechanism involved in panel effects, often referred to as "dissent aversion." In particular, writing a dissent creates work for the majority opinion author, who must spend additional time responding to the dissent's arguments.[10] However, given that judges engage in repeated interactions with judges on their circuit, a desire to maintain collegial relationships may lead a judge to care

[9]Cross (2007) includes a measure of judicial ideology and Epstein, Landes, and Posner (2013) subsets to Democratic and Republican judges, however, which suggests that the effects they find are not simply a result of judges being likely to share ideologies with the broader circuit from which they are drawn.

[10]Epstein, Landes, and Posner (2011) presents empirical evidence that workload pressures affect the decision to dissent.

about the views of other judges on the circuit, even beyond copanelists.

Judges may also be directly anticipating a reviewing court's eventual resolution of the issue. To the extent circuit judges experience reversal aversion, we may see it in response both to changes in the ideological composition of the Supreme Court and to changes in the composition of one's own circuit, which can rehear a panel's decision *en banc*. However, there are reasons to expect that this is not a primary mechanism. Klein and Hume (2003) finds little evidence in favor of the hypothesis in the context of potential reversal by the Supreme Court, which may be because the Supreme Court hears such a small percentage of appellate court cases.[11] Much like Supreme Court review, *en banc* review is rare and likely not a top-of-the-mind concern for judges in routine cases (Bowie, Songer, and Szmer 2014), although the possibility of review likely has a greater impact on opinion crafting than on voting (Boston 2020; Hinkle 2016). Thus, while reversal aversion may affect behavior in a subset of cases where the probability of review is unusually high, I expect that it is not a primary driver of observed responsiveness of circuit judges to changes in the composition of their own circuits.

However, there is a particularly important mechanism that likely underlies much judicial responsiveness to circuit preferences: precedent. Published panel decisions are conventionally treated as precedential under the "law of the circuit" rule (Mead 2011), and thus any novel legal issue that a judge contributes to resolving at the panel level will affect the behavior of judges on other panels that are attempting to follow circuit precedent. While scholars have debated the role precedent or other legal factors play at the Supreme Court, which faces no hierarchical pressures (e.g., Bailey and Maltzman 2011; Gillman 2001; Richards and Kritzer 2002; Spaeth and Segal 1999), precedent plays a larger role at

[11]Howard (1981/2014, 165) notes that 11/35 justices he interviewed thought the "anticipated response of the Supreme Court" was "not important" when precedent was unclear while only 4/35 said it was "very important."

lower levels of the judicial hierarchy (Hinkle 2015). One would be hard-pressed to find a judge who would say that circuit precedent does not matter. In Howard's (1981/2014, 151) classic study, 26/35 circuit judges interviewed said that the closest circuit precedent is "very important" even when "precedents are absent or ambiguous" and none said it was "not important." Since precedent will partly reflect the preferences of the judges on the panel that created it, following precedent will lead to responsiveness to the preferences of other judges on one's circuit.

An important observable implication of this argument is that responsiveness to circuit precedent should not be identical in all types of cases. In the types of cases that routinely appear before the circuit courts, precedential opinions are issued more often. Thus, if this is a primary mechanism driving circuit responsiveness, we should see greater responsiveness to circuit preferences when judges are considering issues that appear with greater prevalence before one's circuit. On these high-prevalence issues, precedent should be more likely to shift in a rightward (leftward) direction as the circuit shifts to the right (left), since judges who are voting at least partially attitudinally will author precedential opinions that partly reflect their ideological views.[12] On low-prevalence issues, a change in circuit composition is unlikely to lead to noticeable changes in circuit precedent, simply because fewer precedential opinions will be written.

An alternative to the precedent hypothesis is the idea that judges are motivated by a general desire not to introduce unnecessary inconsistencies into the law; one judge stated that "[n]ext to resolving the dispute before us, it's the most important thing we have to do" (Klein 2002, 24). When faced with a novel issue, judges who wish to avoid creating an inconsistent body of law may defer to what they think other judges would do over their

[12]Of course, those opinions themselves will partly bound by precedent. But as long as judicial decision-making is partly attitudinal as well as precedential, a judge can know that a conservative shift in the composition of the court will lead to more conservative precedents on average.

own preferences. Importantly, if a preference for consistency were driving judicial behavior more than precedent, we should see responsiveness to circuit composition in a different set of cases. While precedent likely affects circuit court behavior on issues that arise frequently on a court's docket, a desire for consistency is more likely to operate on infrequent issues where there is less direct precedent but in which a judge does not want to run afoul of broader principles that inform a court's decision-making.

Judge O'Scannlain, Sovereign Immunity, and the ADA: An Example of How Circuit Precedent Binds

A concrete example helps show how circuit precedent can affect the behavior of an ideologically opposed judge. We'll consider Judge Diarmuid O'Scannlain of the Ninth Circuit and the question of whether sovereign immunity limits suits under the Americans with Disabilities Act (ADA).

The Eleventh Amendment generally protects states from suits under the doctrine of "sovereign immunity." However, Congress can abrogate that sovereign immunity under Section 5 of the Fourteenth Amendment. After the ADA was passed in 1990, courts had to decide whether plaintiffs could sue states under that act. The Supreme Court in the period following the passage of the ADA started developing a more conservative doctrine on state sovereign immunity and Congress's powers under the Fourteenth Amendment.[13] In fact, the Supreme Court held that Congress did not abrogate state sovereign immunity under Title I of the ADA.[14]

However, the Ninth Circuit took a different approach. The Ninth Circuit is a liberal

[13] See, e.g., *Seminole Tribe of Florida v. Florida*, 517 U.S. 44 (1996), *City of Boerne v. Flores*, 521 U.S. 507 (1997), and *Florida Prepaid Postsecondary Education Expense Board v. College Savings Bank*, 527 U.S. 627 (1999).

[14] *Board of Trustees of the University of Alabama v. Garrett*, 531 U.S. 356 (2001).

circuit and it was getting more liberal during the mid to late 1990s.[15] The Ninth Circuit in this period consistently ruled that another part of the ADA, Title II, *did* abrogate state sovereign immunity, allowing suits against states,[16] rejecting arguments that the Supreme Court's precedents required a different result.[17]

Enter Judge Diarmuid O'Scannlain, a conservative Reagan appointee and an outlier on the Ninth Circuit. He tried unsuccessfully to get the Ninth Circuit's precedent reheard *en banc*.[18] Nevertheless, even though he argued that the Ninth Circuit was out of step with the Supreme Court, he dutifully followed Ninth Circuit precedent when sitting on panels.[19]

This example illustrates several phenomena. First, judges take circuit precedent very seriously (Hinkle 2015). O'Scannlain strongly disagreed with the Ninth Circuit's approach and yet voted in accordance with precedent. Second, this can't always be explained solely by collegiality. If O'Scannlain simply wanted to maintain positive working relationships with his liberal colleagues, it is unlikely he would have written so extensively about why his colleagues were wrong. Third, the Supreme Court is only a limited check on outlier circuits. When it applies, Supreme Court precedent controls,[20] but the high court simply

[15] See Figure 1.2 later in the paper for a quantitative measure of this.

[16] See, e.g., *Clark v. California*, 123 F.3d 1267 (9th Cir. 1997), *Dare v. California*, 191 F.3d 1167 (9th Cir. 1999), *Hason v. Medical Bd. of California*, 279 F.3d 1167 (9th Cir. 2002)

[17] The Ninth Circuit, in a related issue, also held that states that accept funding under the Rehabilitation Act waive their sovereign immunity. *Douglas v. California Dept. of Youth Authority*, 271 F.3d 812 (9th Cir. 2001).

[18] *Hason*, 294 F.3d 1166 (O'Scannlain, J., dissenting from denial of rehearing *en banc*); *Douglas v. California Dept. of Youth Authority*, 285 F.3d 1226 (9th Cir. 2002) (O'Scannlain, J., dissenting from denial of rehearing *en banc*).

[19] *Miranda B. v. Kitzhaber*, 328 F.3d 1181, 1191 (9th Cir. 2003) (O'Scannlain, J., concurring); *Phiffer v. Columbia River Correctional Institute*, 384 F.3d 791, 793 (9th Cir. 2004) (O'Scannlain, J., concurring); see also *Vinson v. Thomas*, 288 F.3d 1145, 1156 (9th Cir. 2002) (O'Scannlain, J., dissenting) (dissenting on the merits of the disability discrimination claim, but accepting circuit precedent on sovereign immunity).

[20] Research suggests that lower court judges do attempt to follow Supreme Court precedent, especially when the Supreme Court signals the importance of a precedent (Hansford and Spriggs 2006; Masood, Kassow, and Songer 2019).

cannot decide as many issues as the circuits do. While the Ninth Circuit's precedent on suits under Title II of the ADA may have appeared somewhat inconsistent with the Supreme Court's general approach to state sovereign immunity, that *particular* issue had not been resolved by the Supreme Court. Thus, judges on the Ninth Circuit who supported panel precedent faced no direct hierarchical command to overturn it, and judges who opposed it (such as O'Scannlain) could not use the Supreme Court as a reason not to follow it. Finally, not all important issues are resolved by the Supreme Court or by the circuit *en banc*. O'Scannlain was unable to get the Ninth Circuit to reverse its course *en banc*, and the Supreme Court took up the issue only partially.[21] Thus, the views of O'Scannlain's fellow judges on Ninth Circuit panels ended up controlling.

Judicial Institutions and Responsiveness to Circuit Composition

In this paper, I use a novel measure of circuit ideology to investigate whether judges sitting on panels respond to their circuit's composition, and if so, what mechanism drives that responsiveness. First, in general, I hypothesize that judges on more conservative circuits should vote more conservatively, holding panel composition constant:

H1 (Circuit composition hypothesis). Judges on more conservative circuits will vote more conservatively.

However, *when* we see responsiveness to the circuit as a whole depends on which mechanisms are operative. First, we can help distinguish between precedent and a preference for consistency by seeing if responsiveness depends on the type of case the panel is hearing. If responsiveness to the circuit is concentrated in the types of cases that arise frequently before the circuit courts, it is likely a function of precedent, since appellate panels are making

[21] The Supreme Court only addressed the issue in the context of courts and prisons. *Tennessee v. Lane*, 541 U.S. 509 (2004); *United States v. Georgia*, 546 U.S. 151 (2006).

precedential decisions routinely in those types of cases. However, if such responsiveness is concentrated in novel or infrequent types of cases, it is more likely driven by a generalized preference for consistency, since it is less plausible that other panels are routinely issuing precedents that would be binding on the current panel. Thus, it is important not only to take into account the issue that is being considered in each case, as that may affect the baseline propensity to vote conservatively, but also whether that issue appears frequently or infrequently before the circuit courts.

H1a (Precedent hypothesis). Judges will respond to circuit composition on issues for which there is likely to be applicable precedent because they are more commonly considered by the court.

H1b (Consistency hypothesis). Judges will respond to circuit composition on issues that are novel or infrequent.

Second, responsiveness to one's circuit may be driven in part by collegiality as well as by precedent or preferences for consistency in the law. If collegiality concerns are present beyond panel effects, they likely stem from the repeated interactions one has with one's circuit colleagues.[22] Thus, responsiveness to changes in circuit composition should be driven by judges who actually hail from the circuit, rather than judges visiting from outside the circuit or district judges who are temporarily serving on circuit panels. Furthermore, if collegiality is a prominent motivator, we should see greater responsiveness in smaller circuits, where judges have more frequent interactions.

H2a (Collegiality hypothesis – repeated interaction). Judges from a given circuit when sitting on appellate panels will vote more conservatively if the circuit becomes more con-

[22]It does not, however, stem from interactions with non-copanelists on the specifics of a particular case; there is a strong norm in the Courts of Appeals against involving those outside the panel in a case (Bowie, Songer, and Szmer 2014, 56).

servative, but judges visiting from other circuits will not. Judges will respond to circuit composition more strongly in smaller circuits.

Finally, the collegiality concerns driven by dissent aversion are always present in three-judge panels.[23] Thus, I hypothesize that as one's copanelists become more conservative (holding circuit ideology constant), one's propensity to cast a conservative vote increases, consistent with the literature on panel effects. This should hold across a wide range of issues and judges.

H2b (Collegiality hypothesis – dissent aversion). Judges will vote more conservatively when they sit on panels with more conservative copanelists.

1.2 Measuring the Ideological Composition of Circuit Courts

The most natural way to measure responsiveness to other judges is to use a measure of ideology; however, this poses particular problems when studying the U.S. Courts of Appeals. While it is simple to come up with a measure of Supreme Court conservatism, any attempt to measure the conservatism of a circuit from voting behavior will run into serious issues, since most cases are heard in three-judge panels and most are unanimous. Furthermore, to avoid circularity, we need a measure that is not derived from the cases under study.

To avoid these problems, we can use Judicial Common Space (JCS) scores (Epstein et al. 2007) to construct measures of circuit conservatism. These measure the ideologies of individual judges as a function of presidential and senatorial ideology. While the inclusion

[23] As this is not the principal focus of this paper, I subsume all the possible motivations for panel effects under "collegiality" and do not attempt to unpack them. However, multiple motivations may drive panel effects. For a sophisticated examination of this problem, see Hinkle, Nelson, and Hazelton (2020).

of presidential ideology is obvious, copartisan home state senators also have an important role in the appointment process (Giles, Hettinger, and Peppers 2001).[24] Thus, nominees are assigned the average of the Common Space scores of the home state senators who are copartisans with the appointing president, if any; if there are no copartisan senators, the nominees are assigned the score of the appointing president instead.[25] Since JCS scores for circuit judges are not based on voting behavior or the courts' agendas, they avoid the difficulties in estimating circuit judges' ideologies from votes.

How can we aggregate JCS scores, which are assigned to individual judges, to the circuit level? Since circuit courts almost always decide cases in panels of three, the measure of circuit conservatism ought to mimic that decision-making process. Importantly, several of the possible mechanisms by which circuit composition could affect voting by individual circuit judges (precedent, preference for consistency, and collegiality) involve the circuit judge's relationship to other potential *panels*, not to the circuit sitting *en banc* (which is a rare occurrence). Only reversal aversion directly involves a relationship between a judge sitting on a panel to the full circuit. Precedent reflects the weight of panel decisions much more than *en banc* decisions given the latter's rarity.[26] Preference for consistency involves

[24]The norm of senatorial courtesy has historically given some power to outpartisan home state senators in preventing a nominee from being confirmed, but it is infrequently invoked; copartisan home state senators have additional power through their influence on who is nominated in the first place (Steigerwalt 2010).

[25]For district court judges who were not later elevated to the circuit courts, I employ the measures compiled by Boyd (2015a), which use the same procedure. Since Boyd's scores are based on Common Space scores through the 113th Congress, I use the version of the JCS scores for circuit court judges based on the same data. For any judges casting votes not included in either the JCS or Boyd datasets, such as judges sitting by designation from the Court of International Trade, I have constructed scores using the same procedure. For retired justices of the Supreme Court who do not have JCS scores as circuit judges, I assigned their JCS scores as Supreme Court justices as of the last Term they served. For judges who served in Congress and cannot otherwise have their scores calculated, I use their Congressional scores (Lewis et al. 2021).

[26]Epstein, Landes, and Posner (2013, 183) explains this dynamic well: "Court of appeals judges also are more tethered to precedent than Supreme Court Justices are, and as a circuit shifts to the right (or left) the minority judges will find themselves bound to precedents increasingly being created by the majority because it is a larger majority and therefore dominates more panels; and panel decisions are the source of most circuit-level (as distinct from Supreme Court) precedents because en banc proceedings are rare."

concerns about binding future panels under the "law of the circuit" doctrine; future *en banc* sittings are not bound by that doctrine. Finally, collegiality reflects the judge's anticipation of serving on future panels with his or her colleagues. Thus, to test these mechanisms, one needs a measure of ideology that reflects this panel decision-making process.

Therefore, I constructed a new measure of *Circuit Conservatism* as follows. For every month between 1965 and 2017, I generated all possible three-judge panels from the active judges in each circuit and computed the median JCS score of each of these panels.[27] The measure of circuit conservatism, which is assigned by circuit-month, is the mean conservatism of all these possible panels. Thus, this measure can be understood as the mean panel conservatism for each circuit-month. This measure has the additional benefit of reflecting the way that judges will experience the ideology of their own circuits, as judges likely learn this information through repeatedly sitting on panels with other judges and observing their ideological attitudes (or through reading the opinions that panels issue). It builds upon previous work, which has looked at the partisan composition of the circuits (e.g., Epstein, Landes, and Posner 2013) or used the JCS median (e.g., Cross 2007) by using a more precise measure of judicial preferences than partisanship and by taking into account the panel decision-making context of the Courts of Appeals.[28]

We cannot consider the effect of circuit conservatism without also considering copanelist conservatism. Thus, I include the ideology of one's copanelists by taking the mean

[27] 1965 is the starting year because there is no Common Space score for Hoover, yet one Hoover-appointed judge without a senatorial courtesy score (Joseph Chappell Hutcheson, Jr.) served as an active judge until the end of 1964. This also helps alleviate concerns that JCS scores may not do as good a job of capturing judicial ideology in the 1950s and earlier. To figure out which judges were active judges in a given circuit-month, I relied on the Attributes of U.S. Federal Judges Database (Gryski and Zuk 2008; Gryski, Zuk, and Goldman 2008) and the *History of the Federal Judiciary.* http://www.fjc.gov. Web site of the Federal Judicial Center, Washington, DC.

[28] Broscheid (2011) has the closest analogue to this measure that I have found in the literature, taking 1,000 random samples of three-judge panels by circuit-year from 1993 to 2000 to compare the distributions of panels across circuits. I build upon this work by extending the time frame, looking at circuit-month rather than circuit-year, using all possible panels rather than a random sample, and most crucially by using it as an explanatory variable for circuit judges' behavior.

of their JCS scores, a standard measure in this context (Collins and Martinek 2011). Also, since a judge's own views are likely correlated with that of the panel and the circuit, we need to take that into account as well. I will both estimate models that include a judge's own JCS score as a predictor, as well as models that employ judge fixed effects in order to consider within-judge changes in circuit and copanelist conservatism; this is discussed further below.

As measured here, changes in circuit ideology are entirely driven by appointments, since scores are assigned at the time of appointment and do not change over the course of a judge's tenure; therefore, the best way to interpret this circuit conservatism measure in the context of this study is as the effect that appointments to circuit courts have on panel decision-making. Figure 1.1 shows the change in the average conservatism measure across the geographic circuits from the beginning to end of each presidential term.[29] Turnover in the circuit courts is affected by presidential elections because some judges retire for personal reasons and thus leave the bench during presidencies of the other party (Barrow and Zuk 1990). Here we see that while the courts become more conservative during Republican terms and more liberal during Democratic terms, presidents have had varied success in moving the ideological balance of the circuit courts. For example, despite Carter's being a one-term president, his appointees had a large effect.

Because presidents will not be equally successful at making appointments to all courts, there is interesting across-circuit variation in this measure as well as within-circuit variation across time. Nevertheless, it is worth noting that only 26% of the variance in this measure is between circuits; most is within circuit over time. Both within-circuit and between-circuit variation can be seen clearly in Figure 1.2, which presents for illustrative purposes the conservatism measures for the Fifth Circuit and the Ninth Circuit from 1965–2017;

[29]The Federal Circuit is excluded on account of its highly specialized caseload. For presidential success by circuit, see Figure A.1 in the Appendix.

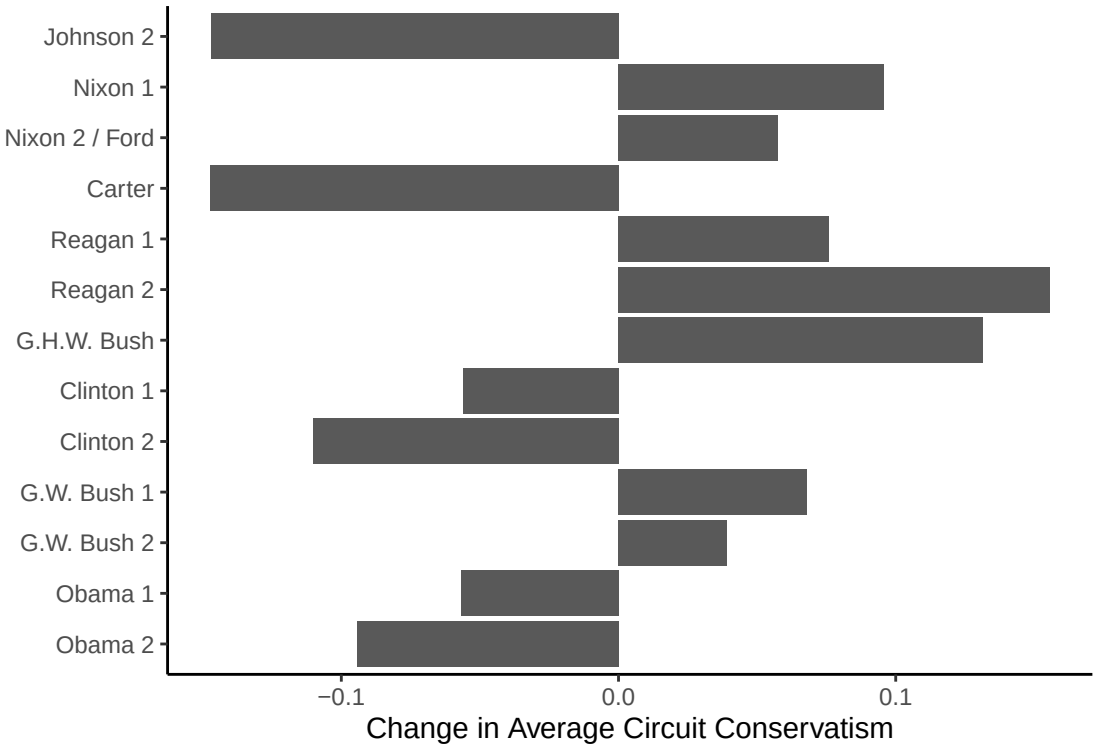

Figure 1.1: Change in average conservatism across all circuits by presidential term

Figure 1.2: Conservatism measures for the Fifth Circuit and Ninth Circuit, 1965–2017

Figure A.2 in the Appendix plots all the circuits.[30] The difference between the conservative Fifth Circuit and the liberal Ninth Circuit in the modern period is obvious, but the within-circuit variation is interesting as well. The Fifth Circuit became much more conservative in the early 1980s; less abruptly, the Ninth Circuit became more moderate as a result of the Reagan and Bush appointments and only became more liberal again during Clinton's presidency.

Measuring spillover effects is further complicated by the fact that circuit court judges sitting on panels may be influenced by the composition of two possible courts: their own circuits and the Supreme Court (Kastellec 2011). These pressures could potentially push in opposite directions.[31] Thus, I will include year fixed effects to take into account the

[30]The measures are by month; the figure plots each measure as of January.

[31]For example, a judge on a panel in the relatively liberal Ninth Circuit faces the following conundrum:

conservatism of the Supreme Court and other shared time trends across judges.

1.2.1 Case Data

The U.S. Courts of Appeals Database (Songer 2008; Kuersten and Haire 2011),[32] collects a random sample of published appellate court decisions from 1925-2002. Using the measure of circuit conservatism derived from Judicial Common Space scores (Epstein et al. 2007) as our independent variable of interest, we can see whether there is a relationship between conservative voting by judges sitting on panels and the ideological composition of the circuit hearing the case.

Figure A.3 in the Appendix shows judicial voting on panels by circuit-decade from 1965–2002, the years for which we have the circuit conservatism measures. It is well known that voting behavior differs from circuit to circuit (Songer, Sheehan, and Haire 2000), and Figure A.3 confirms that conservative voting varies significantly across decades and circuits. In Figure A.4 in the Appendix, we see that the correlation between conservative voting and average circuit conservatism, aggregated by circuit-decade, is 0.48. Of course, circuit conservatism could be a good predictor of circuit voting for several reasons; most notably, the preferences of the judges who actually voted will be correlated with the views of the circuits from which they were drawn. Even if there are spillover effects from the circuit, they may be entirely driven by panel effects. Thus, I estimate models at the level of individual votes in cases in order to distinguish among these effects.

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a more conservative decision may be inconsistent with her own circuit, but a more liberal decision is more likely to be inconsistent with the Supreme Court.

[32] Available at http://www.circuitcourtsdata.com/download/.

1.3 The Effect of Circuit Composition in Panel Decisions

The dependent variable in all the models I present is the ideological direction of a judge's vote.[33] Data from *en banc* decisions are excluded, so every vote is a vote in a case heard by a panel. 1 is a conservative vote and 0 is a liberal vote; those votes which were classified as mixed in the U.S. Courts of Appeals Database are assigned the value 0.5.[34] Most cases in the Database have one vote per judge, although some have two, when the case is categorized as having two issues; if two issues are noted in the Database, I included the votes on both issues. Votes are cast by judges (j) who in turn sit on panels (p); the main attributes of panels are the circuit from which they are drawn (c), the time at which they are making a decision (t), and the issue category to which the legal dispute belongs (i). The principal independent variable of interest is *Circuit Conservatism*. The circuit conservatism measure for each vote is the circuit conservatism of the circuit hearing the case, since that is the circuit whose precedents could potentially affect decision-making.[35] Cases are included from 1965 onward, where the Database includes 30 cases per circuit-year and where JCS scores can be computed for all active judges. To take into account panel effects, I also include a predictor of *Copanelist Conservatism*. The main model

[33] I obtained judge names by matching the judge codes in the U.S. Courts of Appeals Database to the Attributes of U.S. Federal Judges Database (Gryski and Zuk 2008; Gryski, Zuk, and Goldman 2008). Where possible, I manually investigated and corrected observations where this matching process appeared to fail, as when it led to judges with missing names, judges casting votes before appointment or after death, or duplicate judges within a single case-issue.

[34] Votes for which the ideological direction was classified as unclear are excluded. However, judges who cast unclear votes are still included in the calculation of copanelist conservatism for any other judges who cast clear votes. In Table A.2 in the Appendix, I treat mixed votes as unclear rather than assigning them 0.5; the results are similar to those presented in the main text.

[35] Occasionally judges sit on panels outside their home circuits, so some of the within-judge variation in circuit conservatism is between-circuit, although it is mostly within-circuit.

specification is as follows:

$$\textit{Conservative Vote}_{jp[c,t,i]} = \beta \textit{Circuit Conservatism}_{ct} + \gamma \textit{Copanelist Conservatism}_{jp} + \alpha_j + \tau_t + \iota_i + \varepsilon_p$$

This is an extension of the common two-way fixed effects model. α_j and τ_t are fixed effects by judge and year, respectively. To take into account agenda effects, ι_i is a dummy variable for the legal issue being considered by a case. Since errors are likely correlated at the panel level, standard errors are clustered by case. H1 predicts that $\beta > 0$ and H2b predicts that $\gamma > 0$. I will also present the results of other models to test whether the results are dependent on model specification. I test the subhypotheses H1a, H1b, and H2a by adding interaction terms to the model, as discussed further in subsequent sections.

Table 1.1 presents linear models with standard errors clustered by case predicting conservative votes by judges sitting on panels from 1965-2002, taking into account the spillover effects from other judges on the circuit and from copanelists.[36] We see in Model 1 that circuit conservatism, which reflects the average conservatism of panels on the circuit, and copanelist conservatism, reflecting the collegial context of an individual case, have strong effects of similar magnitudes. This is particularly notable given that this model takes into account a judge's own ideology. In fact, the coefficients for circuit conservatism (0.17, s.e. 0.03) and copanelist conservatism (0.15, s.e. 0.03) are larger than the coefficient for a judge's own ideology (0.06, s.e. 0.01). Table A.4 in the Appendix shows that while using the circuit JCS median as a measure of circuit conservatism recovers the directionality of the effect, the magnitude is diminished, fitting with my theoretical argument that most mechanisms of responsiveness to the circuit come through relationships to the circuit

[36]Linear models are presented because of the presence of the fixed effects in Models 2-4; Table A.3 presents results from an ordered logit model similar to Model 1. Case was the most conservative choice for clustering in Model 4 among case, case type, judge, circuit-decade, and no clustering.

sitting in panels, not the full circuit.[37]

Table 1.1: Predicting conservative voting with circuit conservatism and copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.17***	0.15***	0.14***	0.13**
	(0.03)	(0.04)	(0.05)	(0.05)
Copanelist conservatism	0.15***	0.15***	0.15***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Constant	0.61***			
	(0.005)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

***p $<$ .01; **p $<$ .05; *p $<$ .1

Models 2–4 are estimated within-judge in order further to address the possibility that judges may vote in line with their circuit's preferences simply because of shared attitudes among federal judges driven by their shared appointment process. In addition, Model 3 adds case type fixed effects to address concerns about possible changes in the courts' agendas as well as year fixed effects. These "case types" are included in the U.S. Courts of Appeals Database and categorize what each case was about, or for cases with two issues, what each issue was about. The year fixed effects capture any shared time trend among the

[37]Furthermore, Table A.9 shows that the relationship between the median JCS score on a circuit is declining in magnitude with a circuit's proclivity to take up cases *en banc*, which is inconsistent with a reversal aversion mechanism.

judges across all case types, which takes into account important factors such as the composition of the Supreme Court, the partisan composition of the national government, national public opinion, and so on.[38] This is a particularly demanding specification because much of the variation in circuit conservatism occurs over time within circuit. Finally, Model 4 replaces the separate year and case type fixed effects with a combined year-case type fixed effect. While this means that the variation being explained is now mostly within the largest case types, it is a way to take into account differential changes that occur over time across different case types. The estimated effects change little across the different specifications. We see that even in the most rigorous specification, changes in circuit conservatism are associated with changes in voting behavior (0.13, s.e. 0.05), supporting H1. We also continue to see strong panel effects in all these models. Both through panel effects and through the effect of judges who are not copanelists, the composition one's circuit affects judicial behavior.

To test whether the responsiveness to circuit conservatism we saw in Table 1.1 is driven in part by the repeated interactions judges have with their circuit colleagues (H2a), in Table A.6 in the Appendix I interact the circuit conservatism and copanelist conservatism variables with a binary variable indicating whether the judge comes from another court.[39] Model 1 in Table A.6 shows that circuit responsiveness is higher for judges sitting on their home circuits than for judges hailing from other courts. Model 2 in Table A.6 adds an interaction between copanelist conservatism and whether a judge is from another court, since the mechanism driving responsiveness to copanelists (dissent aversion) should *not* depend on what court a judge comes from, as the greater workload caused by writing a dissent ex-

[38] In Table A.5 in the Appendix, I present models that explicitly include a measure of Supreme Court conservatism. We see no evidence that circuit judges respond to broad ideological changes in the Supreme Court.

[39] This includes judges from other circuits, judges from district courts, judges from specialized courts such as the Court of International Trade, and retired Supreme Court justices.

ists regardless of whether judges are visiting from another court. In Model 2, we continue to see that responsiveness to the conservatism of the circuit where the case is being heard declines for judges from other courts, but responsiveness to copanelists does not.[40] However, we see in Table A.7 that responsiveness does *not* depend on circuit size, which pushes back against the idea that this can all be explained by collegiality, since smaller circuits involve more frequent interactions between judges.[41]

In all these models, we see a general responsiveness to circuit conservatism, supporting H1. In addition, this responsiveness is limited to judges who are not from another court (consistent with H2a) but is not driven by smaller circuits (inconsistent with H2a). The strong effect of copanelists across all models supports H2b. However, to test the precedent (H1a) and preference for consistency (H1b) mechanisms requires further analyses, presented below.

1.3.1 Responsiveness to Circuit Conservatism and Issue Prevalence

We see an overall responsiveness of judges sitting on panels to the conservatism of the circuit as a whole (H1). An important question is whether this responsiveness persists across different issues that the courts face. In particular, we can distinguish between issues that come up frequently before the U.S. Courts of Appeals and infrequent issues. If responsiveness to one's own circuit is limited to frequent issues, that suggests that the most likely mechanism is through well-developed circuit precedent (H1a). On the other hand, if responsiveness is limited to infrequent issues, it suggests a very different mechanism. In that case, judges may be looking to the views of other judges on their circuit in order to

[40] While I hypothesize that these dynamics are a function of collegiality, it is worth noting that judges from other courts may be less familiar with circuit precedent (Howard 1981/2014, 190); thus, we cannot say that this result shows that collegiality *rather than* precedent is driving responsiveness to circuit preferences.

[41] This lack of a relationship also holds when considering the probability of a judge serving with any particular copanelist (Table A.8).

avoid deciding novel issues in a way that would be inconsistent with the circuit's overall approach to deciding cases (H1b).

In order to investigate this question, we need good estimates of how often issues come before the circuits. While the case type variable in the U.S. Courts of Appeals Database reflects the substantive topics of the cases and is a strong predictor of voting, it does not map cleanly onto legal issues. For example, there are many different case types for different substantive crimes (e.g., murder, robbery, narcotics). But the legal issues coming up on appeal in criminal cases may not have to do with the type of crime, but rather with issues surrounding sentencing or criminal charges that transcend substantive crimes (e.g., conspiracy, aiding and abetting).

I develop the following strategy to measure issue prevalence. The U.S. Courts of Appeals Database has variables for the most-cited and second-most-cited provisions of the U.S. Code in the Westlaw headnotes for the case. By using these variables, we can a) subset the data to statutory cases, b) assign each case to its most cited statutory provision, and, most importantly, c) calculate the prevalence of the statutory provision in the database to determine whether it is a frequent or infrequent issue. I discuss further how I conduct this process below.

To create the statutory subset of the data, first I limited the dataset to those cases that are noted as citing a provision of the U.S. Code. Looking at this subset of the data, we see that not all statutory provisions appear with equal frequency before the appellate courts. Table A.10 in the Appendix shows the 10 statutory provisions that appear the most often as the primary statutory provision cited from 1965–2002. In 25% of the statutory subset, the most cited statutory provision is one of the top 10 presented in Table A.10.

However, the frequency whereby statutory provisions come before the appellate courts varies across time and over circuits. In order to take this into account, for each observation in the dataset I constructed a measure, assigned by circuit-year, that I will call *Issue Preva-*

lence. This measure is constructed by estimating the number of opinions per circuit-year for which a given statutory provision was the most- or second-most cited in the Westlaw headnotes,[42] as denoted in the U.S. Courts of Appeals Database. The number of opinions per circuit-year is an estimate since the U.S. Courts of Appeals Database is a random sample of the published opinions issued by the circuit courts.[43] Figure A.5 in the Appendix presents the measure of issue prevalence for the 10 most common statutory provisions noted in Table A.10. Note that some provisions that were either new (21 U.S.C. 841, from the Controlled Substances Act of 1971, and 42 U.S.C. 2000, from the Civil Rights Act of 1964), or newly strengthened (Section 1983 following *Monroe v. Pape*, 365 U.S. 167 (1961)), start out with a very low issue prevalence and see significant growth over time.

We can use this issue prevalence measure to help adjudicate between the precedent and consistency mechanisms. For cases that are novel or infrequent, there is likely little precedent being issued; however, once an issue becomes a non-trivial part of a court's docket, it becomes more reasonable to assume that changes in court composition can lead to a change in the ideological tenor of precedents. I estimate a model similar to Model 3 in Table 1.1 but adding an interaction between *Circuit Conservatism* and *Issue Prevalence*. This model includes statute fixed effects rather than case type fixed effects to take into account that judicial behavior may systematically differ across statutory provisions. Because issue prevalence is highly positively skewed, I present results with the variable logged.[44] In addition to plotting the linear interaction, I also present the binning estimator of Hainmueller,

[42]Because these headnotes are not compiled by the judges themselves, this avoids the concern that judges may be manipulating this measure through their citation practices.

[43]Only 30 cases are sampled each circuit-year, yet there is considerable variation in the total number of cases actually heard each circuit-year. So if 90 opinions were issued overall in a particular circuit-year, one observed opinion on a statute leads to the estimate that three opinions on that statute were actually issued that circuit-year. Since this approach is necessarily imprecise, I consider another measurement strategy using a different dataset, discussed further below.

[44]In the Appendix, I present specifications with the unlogged variable.

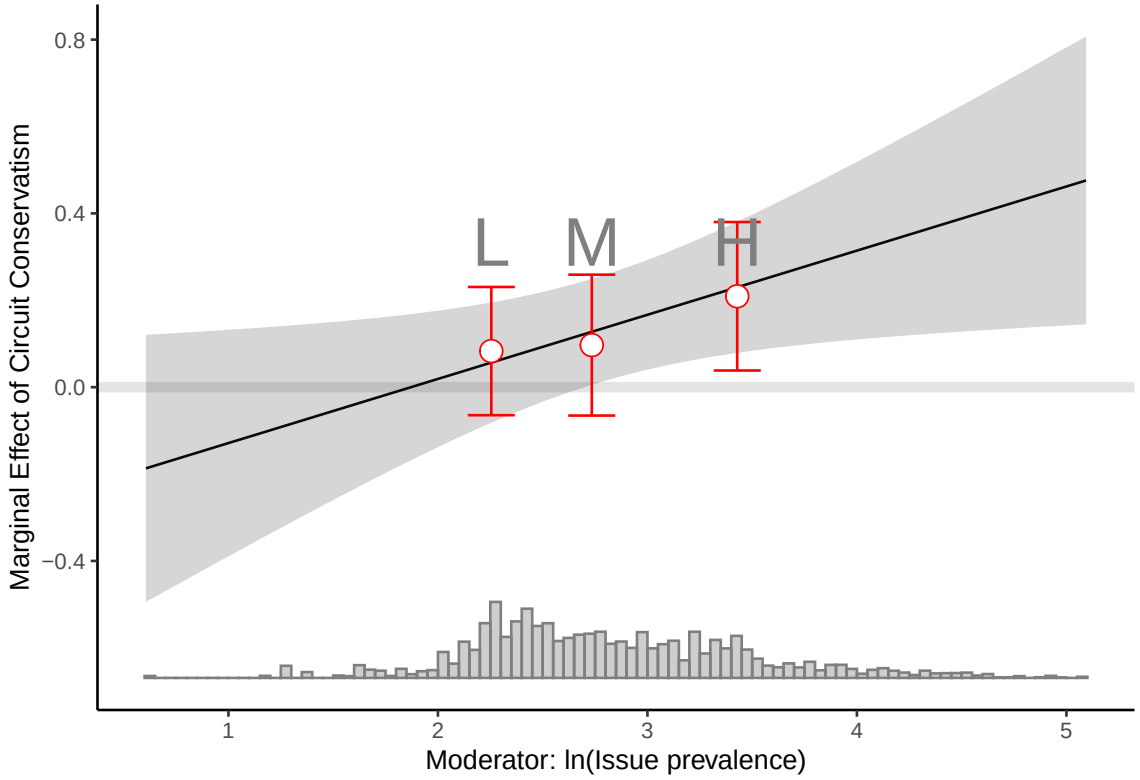

Figure 1.3: Predicting conservative voting with circuit conservatism, interacted with a log transformation of the statutory subset measure of issue prevalence, using the binning estimator of Hainmueller, Mummolo, and Xu (2019) with three bins (indicated on the graph with L, M, and H), and copanelist conservatism. Standard errors clustered by case.

Mummolo, and Xu (2019), which suggests that the relationship is indeed linear.

Figure 1.3 shows the marginal effect of circuit conservatism by issue prevalence for cases in the statutory subset.[45] Here we see that when the circuit courts are issuing few opinions focusing on a statutory provision, there is no effect of circuit conservatism on judicial voting in cases for which that provision is the most cited in the headnotes. However, as a statutory provision becomes more common on the appellate courts' dockets, the effect of circuit conservatism grows. This suggests that the spillover effect of circuit composition on individual judicial behavior is at least in part a function of the development of precedent.

[45] See Table A.11 in the Appendix for the regression coefficients.

Where little precedent exists on an issue because the issue is novel or infrequent, we do not see an effect of circuit conservatism. Thus, this model provides evidence in support of H1a but against H1b.

This issue prevalence measure has important limitations. First, since the U.S. Courts of Appeals Database notes only the top two provisions cited in the headnotes, some provisions may be more frequently considered than this measure can take into account. Second, these are estimates based on a sample of cases, and thus some statutory provisions may not appear at all; relatedly, it is difficult to conduct within-statute analyses since we may only see cases considering a statute in the sample once that statute at least somewhat frequently appears before the courts. Finally, since the votes in the Database are classified by substantive case type, not legal issue, it may not be the case that all the "conservative" votes in cases that share a most-cited provision reflect similar approaches to the statute. To address these concerns, I present an alternative analysis in Figure A.6 in the Appendix using the Sunstein et al. (2006) data as extended by Epstein, Landes, and Posner (2013) (hereinafter the "Sunstein data"), which allows for precise measures of issue prevalence for 14 issues. Using this more precise measure, the magnitude of the estimated effect is greater than in the statutory subset. The downside is that the relationship seen for these 14 issues may not hold for a broader range of issues; thus, we should consider these two measurement strategies for issue prevalence in tandem.

Further evidence that responsiveness to one's circuit is driven by precedent is presented in Table A.12 in the Appendix. There we see that there is no effect of circuit conservatism in *en banc* decisions. Rather, voting in these cases is driven by one's own ideology to a much greater extent than in panel decisions. If precedent is the primary mechanism by which circuit conservatism matters, it makes sense that we should see no effect in *en banc*

voting, since judges sitting *en banc* are not bound by the decisions of panels.[46]

Thus, the preferences of the circuit as opposed to those of individual judges gain more weight through repeated interactions on a particular issue and the development of precedent (supporting H1a). Judges do not appear to be looking to the views of the circuit to avoid creating inconsistencies in the law on novel issues (rejecting H1b); votes in those cases are not influenced by the ideological views of the circuit, although they are still affected by the ideological views of the panel.

1.3.2 Considering Agenda Effects Using Resolved Circuit Splits

Circuit splits, where different circuits rule on the same legal question and at least one is out of line with the others, are a particularly useful dataset with which to study the appellate courts. In particular, I will focus on resolved circuit splits, which are circuit splits where the Supreme Court grants cert and resolves the intercircuit conflict. There are several reasons why these data can provide additional information beyond the ordinary panel decisions considered in Table 1.1. Most importantly, we can hold a single precisely defined legal question constant across multiple circuits, helping to address concerns about possible agenda effects.[47]

This is a non-random sample of cases, unlike those considered in the U.S. Courts of

[46] In addition, in Table A.14, I show a version of Table 1.1 with the measure of circuit conservatism lagged by 12 months. The magnitude of the coefficients for circuit conservatism are similar to those in Table 1.1. This is consistent with the precedent mechanism, as it may take time for a change in circuit composition to reflect itself in the ideological tenor of the circuit's precedential opinions. However, this is less consistent with the reversal aversion mechanism, which involves strategic anticipation of the circuit's future behavior and thus should not operate with a lag.

[47] There are other ways in which these cases differ from typical cases. The rate of review by the Supreme Court *ex ante* for circuit splits is much higher than in ordinary cases, with about a one third probability that one of the cases in the split will be taken up (Beim and Rader 2019); in *resolved* circuit splits, the *ex post* rate of review for the legal question is 1. Furthermore, these cases are likely more important than ordinary cases, and thus they provide a useful comparison group to check if the effect of circuit conservatism persists even in cases raising more interesting legal issues.

Appeals Database (which were a random sample of published opinions), so while Table 1.1 can show the effect of changes in court composition on typical cases, we may be interested in how the different pressures in these extraordinary cases change judicial behavior relative to more typical cases, given the differences discussed above. Thus, in addition to considering the resolved circuit split data on their own, I will also join the resolved circuit splits data to the U.S. Courts of Appeals Database in order to compare the effects in typical cases to those in resolved circuit splits.

I have collected data on all the resolved circuit splits from the 1985–2016 Terms.[48] Data on resolved circuit splits from 1985–1995 are already available thanks to the groundbreaking data collection of Lindquist and Klein (2006),[49] and to this I add over two decades of cases. Then, I gathered the circuit votes in the resolved splits.[50] Table A.15 in the Appendix shows an example of the development of a circuit split. The case, *Astrue v. Capato*,[51] addressed whether posthumously conceived children are always eligible for survivor's benefits or if eligibility depends on state law. Four circuits considered the issue, with the first two (the 9th in 2004 and the 3rd in 2011) taking the former position and the second two (the 4th and 8th, both in 2011) taking the latter position.[52] The Supreme Court unanimously chose the latter, which was the more conservative position. Figure A.9 in the

[48] While this does not encompass all circuit splits during this period, since some are left unresolved, the Supreme Court is more likely to take up a case if the conflict is serious (Black and Owens 2009); thus, we can be more confident that these are cases that the lower court judges themselves would have perceived as potentially presenting important conflicts with other circuits. Beim and Rader (2019) discusses further the differences between resolved and unresolved splits using a dataset of all circuit conflicts between 2005 and 2013.

[49] This is included as part of the replication data for Clark and Kastellec (2013); I supplemented these data with a few additional resolved circuit splits noted in the Supreme Court Database (Spaeth et al. 2020).

[50] The process is described in the Appendix prior to Table A.15.

[51] 566 U.S. 541 (2012).

[52] Four circuits is the median number of circuits to consider an issue in my dataset. Among issues heard by four circuits, seven years is the median time it took for those four circuits to issue their opinions. In this way, *Capato* is a representative case.

Supplementary Materials shows how the Supreme Court characterized the circuit split in this case; it is from these descriptions (of which the description in Figure A.9 is typical) that I collected the circuit splits.

Results: Resolved Circuit Splits

Table 1.2 presents models similar to those in Table 1.1, but including the panel decisions from the resolved circuit splits dataset. One difference, however, is that here we can have fixed effects for the precise legal question on which the circuits are splitting rather than the more general case type fixed effects employed in Table 1.1. Thus, agenda effects are less likely to contaminate the estimates of responsiveness for the resolved circuit splits.

Table 1.2: Predicting conservative voting with circuit conservatism and copanelist conservatism, including resolved circuit splits. Linear regression coefficients presented with standard errors clustered by case.

	Conservative vote	
	Model 1	**Model 2**
Circuit conservatism	0.29^{***}	0.13^{***}
	(0.08)	(0.04)
Copanelist conservatism	0.13^{**}	0.14^{***}
	(0.06)	(0.02)
Judge conservatism	0.09^{***}	
	(0.02)	
Circuit conservatism $\times$ Circuit split		0.15^{**}
		(0.07)
Copanelist conservatism $\times$ Circuit split		-0.03
		(0.05)
Judge fixed effects		✓
Year fixed effects		✓
Legal question fixed effects	✓	✓
Splits included	5+ circuits	All
N	6784	51230
Adj. R-squared	0.20	0.15

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Model 1 in Table 1.2 presents estimates of spillover effects in the resolved circuit splits dataset. Since there is limited intra-item variation in circuit conservatism, this is a simpler model than in Table 1.1. In particular, this is limited to those splits with panel decisions from at least 5 circuits (so there is at least some reasonable variation in both circuit and co-panelist conservatism). Descriptive statistics for this sample are presented in the Appendix (Figure A.10). Item fixed effects are included, although judge and year fixed effects are not because of the more limited variation.

We see evidence of an effect both of circuit conservatism in these data (0.29, s.e. 0.08) and of copanelist conservatism (0.13, s.e. 0.04). In particular, we see no evidence that taking into account the precise legal issue reduces the effect of circuit conservatism; if anything, the effect is larger. Even though these cases differ in important ways from the cases considered in Table 1.1, the spillover effects caused by the institutional structure of the U.S. Courts of Appeals persist.

Model 2 combines the data from the resolved circuit splits and the data from the U.S. Courts of Appeals Database. That way, we can gain some leverage by obtaining better estimates of the judge and year fixed effects for those judges and years which appear in both datasets; in addition, we can test whether the behavior in the circuit splits dataset (which are an admittedly unusual set of cases) differs substantially from the behavior of ordinary cases. Given the greater sample size, here I include all splits (no matter how many circuits participated) and all the fixed effects included in Model 3 of Table 1.1. I interact the variables of interest with whether a case comes from the circuit split database or not.

We see in Table 1.2 that there is a 0.15 (s.e. 0.07) increase in the effect of circuit conservatism when a case arises from the resolved circuit split dataset. Including the circuit split cases makes little difference in the estimate of the effect of circuit conservatism in ordinary cases (0.13, s.e. 0.04). These results suggest that the responsiveness to circuit composition we saw in Table 1.1 was not a result of agenda effects, since we see an even

higher estimate for those cases where we can take into account the precise legal issue rather than the broader case type measure. Furthermore, these results also indicate that responsiveness to circuit conservatism is not limited to less important cases, since the issues in the resolved circuit splits dataset are likely more important on average than those in the random sample of published decisions included in the U.S. Courts of Appeals Database.

1.4 Discussion

By measuring the average conservatism of circuit panels at a given time and also taking into account copanelist and judge ideology, I distinguish between the effect of copanelists and the effect of the circuit as a whole on judicial behavior. In a random sample of published panel decisions from 1965–2002, we see both a responsiveness to changes in the ideology of the circuit beyond one's copanelists, as well as a responsiveness to the ideologies of judges on one's own panel. The former effect is strongest for statutes that arise more frequently on the courts' dockets; this suggests that such responsiveness to non-copanelists is likely driven by changes in circuit precedent. Finally, I find that in resolved circuit splits, where we can take better account of agenda effects and where the possible influence of the Supreme Court looms larger, responsiveness to changes in circuit composition increases.

Given that there is meaningful responsiveness both to copanelist ideology and circuit ideology, and given that presidents routinely make appointments to the Courts of Appeals, these findings have important implications for understanding the president's ability to affect the policymaking output of the judiciary. On the one hand, since most presidents are able to move most circuits in their preferred direction, the short-term effect of presidents on the judiciary is likely larger than commonly supposed, as a president's new appointees will both be more likely to vote in the president's preferred way and they will also influence the behavior of their circuit colleagues. On the other hand, these victories are likely to be

short-lived, as those judges will in turn be influenced by changes in the composition of their circuits that occur as future presidents make appointments to the federal bench.

In addition, these findings imply that "law" and "politics" are not entirely separate concepts on the U.S. Courts of Appeals. A judge's own ideology is a relatively poor predictor of voting behavior (see Model 1 in Table 1.1). Moreover, judges appear to follow the lead of their circuits in areas of the law where there is a great deal of precedent. However, following the preferences of one's circuit is not a politics-free decision, as those preferences themselves reflect ideological values. Thus, adherence to precedent, which is in some respects a clear example of following the "law" instead of politics, can be in other respects political if those precedents reflect the aggregated political preferences of circuit judges and the presidents who appoint them.

Future study can consider some nuances of the relationship between judicial behavior and circuit precedent. Are judges more likely to respect circuit precedent when they themselves played a role in its formation? Furthermore, are judges more likely to uphold precedent authored by judges with whom they have closer interpersonal relationships? In addition, this study raises some interesting questions about other aspects of the judicial hierarchy that could be addressed by future research. For example, how strong are the pressures faced by circuit courts as compared to the pressures faced by district court judges facing circuit review?[53] This comparison could help distinguish between different potential mechanisms at work, since district court judges both face greater workload pressures and are reviewed more frequently than are circuit court judges. Fear of reversal thus may operate more directly in this context, rather than concerns about collegiality, precedent, or legal consistency. A fruitful area of inquiry may also be to consider how circuit court behavior changes as an intercircuit conflict progresses, especially given that the likelihood of

[53] See Chapter 2.

Supreme Court review surely is higher once multiple courts have weighed in on a topic than it is for the first circuit to hear the issue.[54] Further data collection could also see whether these patterns are replicated in measures of circuit court behavior that come from opinions rather than votes.

[54] See Chapter 3.

2 | Appellate Court Influence over District Courts in the United States

Much ado has been made about the Trump administration's success in placing judges on the federal bench. The most high profile instances, of course, are the confirmations of Neil Gorsuch, Brett Kavanaugh, and Amy Coney Barrett to the Supreme Court, but pundits have noted that his administration also made appointments to the lower levels of the federal judicial hierarchy with alacrity.[1] The expected result, to the delight of conservatives and chagrin of liberals, is that the relative youth and ideological purity of these new federal judges will shift the federal bench to the right for the foreseeable future.

But these appointments have not been evenly dispersed across the federal judicial hierarchy: while the pace of President Trump's appointments to the U.S. Courts of Appeals vastly outstrips that of his predecessors, his pace of district court appointments is considerably more pedestrian. In his single term, Trump surpassed President Obama's eight-year total of appeals court appointments, at fifty-four to Obama's forty-nine, but his pace of district court appointments was less notable, at 174 versus 268 for Obama's two terms.[2] It is natural to assume that higher courts are more important, but the existence of higher and lower courts reveals a fundamental tension in the federal judiciary between judicial independence and the pressures induced by a hierarchical structure with oversight poten-

[1] https://tinyurl.com/y28hbw6c; https://tinyurl.com/upbzs68.

[2] https://tinyurl.com/y6xky9mf

tial. While district courts retain features that the Founders felt were important in a judicial system, such as lifetime tenure and comfortable salaries, they may also face pressure from above – specifically, from circuit courts – that affect how these district judges make decisions. If this is the case, President Trump's stacking of the federal appeals courts is doubly important: both for the cases that they do ultimately decide, and for the downward pressure that they exert on the district courts.

In this paper, we demonstrate that district judges respond to changes in the ideological composition of the appellate panels above them in the federal judicial hierarchy. This effect is increasing in the rate at which they are actually reviewed or reversed by the appeals court, and is largest for cases dealing with civil rights and civil liberties. These results highlight an under-appreciated feature of the lower levels of the federal judicial hierarchy: not only are appeals courts powerful because of their greater formal powers, but because they have the potential to alter patterns of decision-making in the broadest-reaching level of federal courts, the district courts.

Previous scholarship offers mixed intuitions for whether district judges ought to be responsive to the appellate panels they are likely to face. Despite their lack of hierarchical pressures, considerable evidence holds that Supreme Court justices are conscious of and respond to their strategic environment (Epstein and Knight 1998). Yet in general, the literature finds relatively little adaptation by circuit judges to Supreme Court preferences (Bowie and Songer 2009; Klein 2002; Klein and Hume 2003). Klein (2002), for example, states that circuit judges' "work does not appear to be closely supervised by the Supreme Court, nor does it seem that they try very hard to anticipate the Court's reaction when making their own decisions. [...] Their decision-making appears individualistic, with ideology playing an important role" (8). Randazzo (2008), on the other hand, finds evidence of district court responsiveness to anticipated appellate behavior when considering a random sample of cases that were appealed, leaving open whether this responsiveness persists when

considering a broader range of cases.

To examine district court responsiveness to appellate court ideological composition, we combine data on politically salient decisions at the district court level[3] (Carp and Manning 2016) with data on the ideological composition of U.S. Courts of Appeals panels based on Judicial Common Space scores (Epstein et al. 2007). We take advantage of variation over time in the ideological composition of the appellate panels that oversee a given district judge to estimate the effects of circuit court ideology on district court decision-making. This feature of our research design allows us to control for all other factors that a specific judge may consider that are constant over his or her career, and also to control for time-specific factors that are common to all courts. Our study also benefits from its focus on district court decisions as the outcome, which are made by a single judge. This allows for a more straightforward interpretation compared to studies that focus on circuit court decisions, where any hierarchical effects may interact with the paneled decision-making process.

We build on previous work that has shown strategic adaptation by district courts (e.g., Boyd 2015b; Choi, Gulati, and Posner 2012; Randazzo 2008) by investigating this behavior over a long time span (1965–2012) and taking into account many potential confounders. Furthermore, we test possible mechanisms that may be driving this behavior, including fear of reversal, progressive ambition, and workload pressures. While we find evidence that district court judges are motivated by fear of reversal, we do not find evidence that this is driven by the desire to be promoted to a higher court or attempts to reduce their court's workload. Taken together, our findings suggest that the Constitutionally-granted independence of district court judges does not preclude the operation of hierarchical pressures from

[3]We rely on Carp and Manning's (2016) decisions about what cases are included; they include cases in the Federal Supplement "[i]f the case contains a relevant and traditional liberal-conservative dimension" (Carp and Manning 2016, Codebook Page 1). We expect that these cases are where responsiveness is most likely to manifest, and these cases are often substantively important.

the circuit courts.

Our results offer important new understanding of both district courts and the federal judiciary more generally. First, district courts are where most litigation occurs in the federal courts,[4] yet understanding of how judges make decisions in these courts is limited. In this paper, we provide evidence that districts are sensitive to the circuit courts that their cases are passed on to, and that this sensitivity is particularly heightened when the threat of appellate review is high. Second, our study allows us unique leverage on broader questions of decision-making in the federal judiciary. As we highlight below, the very construction of the American federal judiciary creates tension between competing principles of judicial independence and hierarchy. Extensive focus on the Supreme Court and circuit courts, for theoretical and empirical reasons, makes drawing conclusions about the relative effects of these competing principles difficult. Our study, by extending the inquiry one step lower, to district courts, offers a fuller picture of the way that the hierarchical structure of the federal judiciary impinges on the "complete independence" of the judiciary that Hamilton held to be "peculiarly essential" (Hamilton 1788/1961, 466).

2.1 Independence and Hierarchy in the U.S. District Courts

Article III of the Constitution attempts to insulate the judiciary from public opinion and political influence by giving judges life tenure and salary protection. Alexander Hamilton defended this scheme of judicial appointment by stating that "[p]eriodical appointments, however regulated, or by whomsoever made, would, in some way or other, be fatal to their necessary independence" (Hamilton 1788/1961, 471). Yet the Constitution also allows for the creation of the judicial hierarchy, granting Congress the power to create "inferior

[4]In the twelve month period ending September 30, 2020, about 470,000 cases were filed in the U.S. District Courts and about 50,000 in the U.S. Courts of Appeals (https://tinyurl.com/2rr2426y).

courts." This arrangement creates a basic tension. The concerns about outside pressures improperly affecting judicial behavior exist at the lower levels of the judiciary just as they do for the Supreme Court, so the judges of the inferior courts receive the same tenure and salary protection as do the justices of the Supreme Court. Yet these protections, created to insulate judges from pressures outside the judiciary, also lessen hierarchical pressures.

In the Supreme Court, where there is no hierarchical pressure, scholars have investigated whether the high court's independence allows justices to be (strictly) ideological in their decision-making, termed the "attitudinal model" (Segal and Spaeth 2002), or whether strategic considerations factor into their calculus when choosing how to vote and write opinions (Epstein and Knight 1998; Murphy 1964). Are strategic considerations more important at lower levels of the judicial hierarchy, given that lower court judges face review by higher courts? The answer to this question has important practical consequences, given that a tiny percentage of federal litigation makes its way to the Supreme Court. Of particular importance to most litigants are the U.S. District Courts. It is in these ninety-four courts, staffed by 677 presidentially appointed judges,[5] that all cases in the federal system begin and most end. Each judge serves in a district (or occasionally is assigned to span multiple districts in a single state), and, except in rare circumstances, each judge decides cases alone.[6]

Given the importance of district judges, it is worth considering how they differ from other judges in the federal court system. Previous research has shown that district judges are less able to decide cases in accordance with their policy preferences, likely in part because they face a greater prospect of appellate review (Zorn and Bowie 2010), which is nonexistent for Supreme Court justices and rare for circuit judges. Yet like Supreme Court

[5] As of 2020, there are 667 authorized permanent judgeships on the U.S. District Courts and 10 temporary judgeships. See https://www.uscourts.gov/sites/default/files/districtauth.pdf.

[6] See 28 U.S.C. § 2284 for an exception that arises in cases involving redistricting.

justices, lower court judges have lifetime tenure and salary protection, and thus the possible sanctions for failing to conform to the ideological preferences of higher courts are limited. Why would judges care about potential review and reversal, given these protections meant to encourage the exercise of their independent judgment? Baum (2009) notes that "Beyond the judge's own court, reversal by an appellate court carries the mark of a defeat and perhaps even of incompetence [...] Thus judges have very good reason to engage in strategic voting as a means to gain an image of success with others and to protect and enhance their self-esteem" (95). Thus, while judges may have formal independence, existing theoretical perspectives suggest that they still care deeply about how they are perceived by their professional peers. Furthermore, Cohen (1991) notes that, in trying to promote their own self-interest, judge-specific factors "such as the reputation of the judge among his or her peers, the workload of the court, and the judge's future career opportunities" (184) may shape judicial behavior.

Before turning to the case of district courts, it is worth noting that many scholars have studied hierarchical responsiveness among circuit courts. This scholarship has found little evidence that circuit judges are motivated by a fear of reversal in their voting behavior (Bowie and Songer 2009; Klein 2002; Klein and Hume 2003).[7] Cross (2003), who finds no evidence of strategic behavior by appeals court judges (and instead finds that their behavior is shaped by legal doctrine and personal ideology), attributes the lack of strategic motivations to the low probability of review circuit courts face: "There is substantial theoretical reason to doubt the significance of the strategic theory of appellate court decisionmaking under either model, primarily due to the very low rate of Supreme Court review of circuit court opinions" (1483). Other research suggests that this low rate of reversal is endogenous,

[7]Of course, even non-strategic circuit judges may still follow the Supreme Court because of a legal commitment to precedent rather than through fear of reversal, so a lack of strategic response need not lead to non-compliance (Bowie and Songer 2009, 395). Furthermore, strategic behavior may manifest in opinion writing rather than voting (Boston 2020; Hinkle 2016).

demonstrating that appeals courts are strategic in deciding when to ignore or comply with Supreme Court precedent (Westerland et al. 2010). Appeals court justices may, therefore, be strategic, but in ways that make ideological adaptation difficult to pinpoint.

Yet the evidence we have of district court responsiveness to circuit courts paints a different picture. For example, previous research looking at a sample of district court decisions that were appealed has shown that district judges strategically anticipate the response of reviewing courts when making decisions (Randazzo 2008). In addition, district judges modify their behavior when given specific directions upon a remand from the appellate court (Boyd 2015b) and the decision to publish an opinion is negatively associated with ideological distance from the circuit (Choi, Gulati, and Posner 2012).[8] Why might district judges be more responsive than circuit judges? The most straightforward explanation is that district judges face a much higher chance of review and ultimate reversal than circuit judges do. Litigants can appeal from final judgments of the district courts to the U.S. Courts of Appeals as a matter of right, while the Supreme Court has almost total discretion over its docket. Moreover, even though most appealed district court decisions are affirmed (Hettinger, Lindquist, and Martinek 2006; Songer, Sheehan, and Haire 2000), the much higher rate of review of district court decisions ensures that a typical district court decision is much more likely to be reversed than a Court of Appeals decision.[9] Thus, fear of reversal is more likely to affect judicial behavior at the district court level.

The greater potential for review and reversal appears the most likely explanation for why district judges would respond to ideological changes in appeals courts; if we can demonstrate that such responsiveness is conditional on rates of review and reversal, we would have good evidence for such an account. We should not immediately presume that

[8]However, district judges' citation behavior to Supreme Court precedent does not depend on appellate judge ideology (Boyd and Spriggs 2009).

[9]Although Hettinger, Lindquist, and Martinek (2006) note that there are also cross-cutting reasons why a circuit court may be reluctant to overturn a district court, including peer effects (90).

greater responsiveness among district judges is entirely driven by non-instrumental concerns for self-esteem or peer-regard, however. There are other factors that differentiate district judges from circuit judges, which could also drive greater responsiveness to hierarchical pressures by incentivizing judges to avoid reversal. Specifically, concerns about progressive ambition and workload are likely to be more acute at the district court level. These instrumental mechanisms are alternative explanations for why judges may wish to avoid reversal beyond reputation and self-esteem.

"Progressive Ambition" refers to a given politician's desire to progress from one office to another, usually higher office that they would rather hold (Schlesinger 1966). District judges may very well have their eyes on higher office. There are about 25% as many circuit court seats as district court seats, but only 5% as many Supreme Court seats as circuit court seats, which means that the opportunities for promotion are greater for district judges. In addition, presidents often appoint circuit judges from the district courts, both because district judges have been successfully confirmed by the Senate before and because promoting a district judge creates a new vacancy at the district court level that the president can fill (Barrow, Zuk, and Gryski 1996). Previous scholarship explores which judges seek to move up: Jensen and Martinek (2009), for example, find that female and non-white judges are more ambitious, and Vining, Wilhelm, and Wanless (2019) show that ideologically motivated judges seek to move up state judicial hierarchies. Bratton and Spill (2004) demonstrate that promotion from the state to the federal bench is correlated with partisan alignment, age, and court prestige. More directly related to our inquiry about progressive ambition as it relates to judicial decisions, Pérez-Liñán, Ames, and Seligson (2006) examine the relationship between progressive ambition and deference in the Bolivian judiciary and find no evidence for such a relationship, and Budziak (2013) shows that promotion potential is positively associated with ideologically consistent voting, which a judge hoping to be promoted would seek to maintain. Yet if judges think reversal will harm their reputations,

and thus affect their promotion potential, it is possible that this may affect district court responsiveness to higher courts.

Judges may face another practical reason to wish to avoid reversal: a reversed decision may be paired with a remand, returning the case to the judge's docket. Remands from a higher court may increase a judge's workload, and workload pressures likely affect judicial decision-making (Choi, Gulati, and Posner 2012, Randazzo 2008, 673, n. 5). Thus, to the extent judges are motivated by workload pressures, we should expect greater responsiveness to the appellate court's preferences when caseloads are high. There is evidence that judges do alter their behavior in response to changing caseloads. Both Supreme Court justices and Court of Appeals judges dissent less frequently when their caseloads increase, a rational response given the effort involved in crafting a dissent (Epstein, Landes, and Posner 2011). Perceived workload pressures have led to organizational changes in the Courts of Appeals, such as bringing in more visiting judges from other circuits and district courts to sit on three-judge panels and deciding a smaller percentage of cases after oral argument (Cohen 2002). In addition, workload is negatively associated with opinion publication in the Courts of Appeals, although the effect is small (Bowie, Songer, and Szmer 2014).

Our main hypothesis is straightforward: when circuit courts become more liberal (conservative), district judges overseen by them will vote in a more liberal (conservative) fashion. We certainly do not expect that this is the only or even necessarily the most important consideration that district judges have, or that it will happen in every single circumstance, but we do expect that on average district judges will be somewhat responsive to changes in higher court preferences. We build on this existing scholarship by using a broad selection of district court decisions from 1965–2012, employing a measure of appellate liberalism that takes into account the paneled decision-making process of the U.S. Courts of Appeals, and using an empirical strategy that controls for a broad array of potential confounders. We then test whether the responsiveness that we find is limited to when district courts face

high rates of review and reversal. Finally, we consider whether progressive ambition or workload pressures affect district court responsiveness to circuit courts. While we find evidence that district judges are trying to avoid reversal, we do not find evidence that this is motivated by progressive ambition or a desire to reduce workload.

2.2 Data and Empirical Design

Our goal is to determine whether there is a relationship, as free as possible of potential confounders and alternative explanations, between the liberalism of circuit courts and the liberalism of district court decisions. If there is, this serves as evidence that district judges are strategic and hew their decisions to the ideological preferences of the court that would hear an appeal of their decision. Achieving this goal requires data on both circuit court ideology and the ideological bent of district court decisions, as well as a research design that allows us to rule out possible confounding factors that may generate a relationship between district case outcomes and circuit court ideology even in the absence of a direct link between them. We describe our data sources and research design in turn.

2.2.1 Data on Court Decisions and Ideology

To establish whether district judges are responsive to the composition of the appellate panels that they (potentially) face, we draw on two main sources of data: a database of politicized district court cases compiled by Carp and Manning (2016), and estimates of circuit judge ideology (Epstein et al. 2007).

District Court Decision-Making Our primary dependent variable is a case-level measure of whether a decision made by a district judge is liberal or conservative. The data is drawn from the Carp-Manning database (Carp and Manning 2016) of politicized district

court cases, and therefore excludes the many cases without an obvious ideological dimension. The data extends from 1927 to 2012 and includes more than 100,000 cases.[10] Cases are categorized into one of more than thirty case types, which fall into three more general categories: criminal justice, civil rights/civil liberties, and economic regulation or labor. Roughly speaking, the side of the employee (as opposed to employer), those seeking to expand (rather than encroach on) civil rights or civil liberties, or defendants in criminal cases are coded as "liberal" decisions. While this is somewhat coarse, it is consistent with other codings of judicial decisions (Songer 2008; Spaeth et al. 2020). Our outcome variable is therefore simply an indicator for whether a decision was liberal or not. The detailed level of the data, at the case level, allows us to very accurately map to each case the specific composition of a circuit court that the district judge would know waited as the next stop for a case, should it be appealed.

Circuit Court Ideology Our primary independent variable is a measure of circuit court ideology. First, this requires estimates of the ideology of each individual judge; second, it requires us to combine these judge-level ideology estimates to develop a measure of the entire circuit's ideology.[11]

To estimate the ideology of individual judges, we rely on JCS scores, using the data for judges serving from 1965 onward (Epstein et al. 2007).[12] These scores use the measure-

[10]We limit the data to 1965 onward to match our circuit court ideology measures, described below. We also removed a small number of likely data errors: judges who are coded as being appointed after the year of the decision or more than 55 years before.

[11]We explain the process further in this section, but it is very similar to that in Chapter 1. The only differences are: a) we use JCS scores from the 115th Congress, since we are not using the district court data from Boyd (2015a), b) we scale the measure so higher is more liberal, to match the Carp-Manning database, and c) we scale the measure from 0-1, as explained below.

[12]Joseph Chappell Hutcheson, Jr., who served as an active judge until late 1964, was appointed by Hoover, for whom there is not a NOMINATE score. Moreover, in earlier periods it is not clear that JCS scores are as useful proxies for judicial ideology, given that they are based on first-dimension NOMINATE scores but in the 1950s many important issues before the federal courts, such as desegregation, were likely second-dimension issues (Poole and Rosenthal 1997, Ch. 3); furthermore, the idea that lower court judges are appointed at least

ment strategy for circuit judges from Giles, Hettinger, and Peppers (2001), which is not based on the actions of judges themselves, but rather on the judicial appointment process. Each judge is assigned an ideology score that is the average of the first dimension NOMINATE Common Space scores (Poole and Rosenthal 1997) of any home-state senators who are copartisans with the president, reflecting the traditional role of these senators in the nomination process,[13] or of the appointing president if neither home-state senator is of the president's party. Traditional methods of ideal point estimation, which draw on broad data sets of roll call voting, in the case of legislatures (e.g., Poole and Rosenthal 1997), or judicial decisions, in the case of the U.S. Supreme Court (e.g., Martin and Quinn 2002) or state supreme courts (Windett, Harden, and Hall 2015), are difficult to employ for estimating ideology on the U.S. Courts of Appeals, where most decisions are heard in three-judge panels and strong collegial norms lead to most of those decisions being unanimous (Epstein, Landes, and Posner 2011; Songer 1982). Though indirect, JCS scores have proven successful in previous studies at capturing the preferences of circuit judges. Moreover, the measure has a particular advantage for our present application: because alternative ideal point estimation strategies generally use, in some way, the full set of a judge's decisions, in our present case such a measure could be endogenous to the decisions of a district judge. It is preferable, in our case, that appellate court ideology is estimated without using the cases that also constitute our outcome variable. In this sense, using a measure based on the nomination process is highly desirable.

We then use these judge-level ideology estimates to calculate a circuit-level ideology score that explicitly accounts for the paneled nature of circuit court decision-making. Since district judges are reviewed by appellate panels, we use a process that helps ensure that

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in part for ideological reasons is more plausible in more recent time periods.

[13] As Steigerwalt (2010) notes, senatorial courtesy has given a limited role to *outpartisan* home-state senators as well, but this does not operate in most nominations, and when it does, it is usually not invoked because of ideological opposition to the nominee.

our measure captures the liberalism of those reviewing bodies. In particular, our measure captures the following dynamic: suppose two extremely conservative judges replace two moderately conservative judges on a circuit. If the two moderates were already to the right of the median judge, this affects the median of the circuit as a whole not at all. But the circuit court has moved to the right, as the universe of possible panels by which a district judge's decision might be reviewed has clearly moved to the right. Our measure captures this dynamic in a way that other measures that ignore the panel structure of circuit court decision-making miss.

We construct the measure as follows. First, we take every possible three-judge panel of active judges for each circuit-month, giving us (close to) the universe of possible panels by which a district judge's decision might be reviewed.[14] Then, we take the median JCS score for each of those panels, drawing on standard median voter results.[15] Finally, we take the mean of all of these panel medians. For ease of interpretation, we rescale this measure so 0 is the most conservative observed value and 1 is the most liberal observed value.

Figure 2.1 shows an example of the construction of this ideology measure. On the y-axis we plot all the active judges in the Tenth Circuit in 2012. The x-axis presents the percentage of the possible three-judge panels among these judges where a given judge is the panel median. The dotted line then presents the mean of these panel medians, which is our principal independent variable. Note that while there is a clear conservative majority,

[14]The panel assignment process is not entirely random, given such factors as accommodations made for judicial schedules (Levy 2017), and this can lead to skewed panel compositions in some circuit-years (Chilton and Levy 2015). However, the criteria used by circuits to assign active judges to panels are non-ideological and the most common non-random factor is circuit judge scheduling (Levy 2017), which district judges are unlikely to be able to predict. Beyond active judges, senior judges and visiting judges from other courts can also sometimes sit on panels. In Table B.13 in the Appendix we show that our substantive conclusions are similar if senior judges are included in calculating our circuit ideology score. While the assignment of visiting district judges is likely nonrandom (Budziak 2015), scholarship shows such judges usually defer to other judges on the appellate panel (e.g., Benesh 2006); thus, the possibility of visiting judges probably plays little role in district judges' strategic calculations.

[15]This is similar to the approach of Broscheid (2011), which generated random panels by circuit-year from 1993-2000 to compare the distributions of the JCS medians across circuits.

there is a reasonably high chance of a district judge drawing a liberal panel.

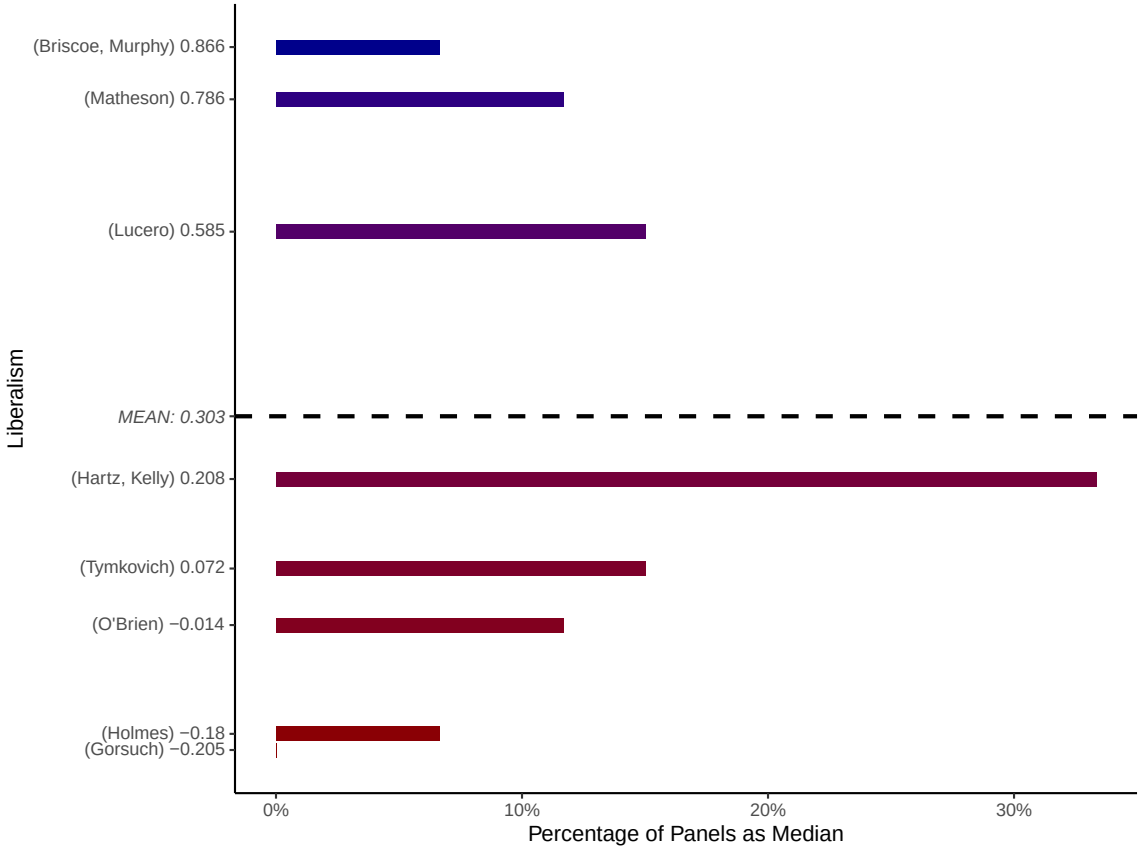


Figure 2.1: Construction of mean panel ideology: Tenth Circuit, 2012

2.2.2 Fixed Effects Model

To assess the impact of circuit court ideology on district court decision-making, we take advantage of the time-series cross-sectional structure of our data on district court decision-making to control for unmeasured confounding factors. With data on judges over time we are able to statistically control for the effect of any time-invariant judge attributes, as well as any time period-specific characteristics that are common to all judges in that period. In short, our estimates are based on within-judge changes in the ideological character of the appeals court above them. This allows us to rule out a variety of confounding factors and

alternative explanations that might otherwise make drawing firm conclusions difficult.

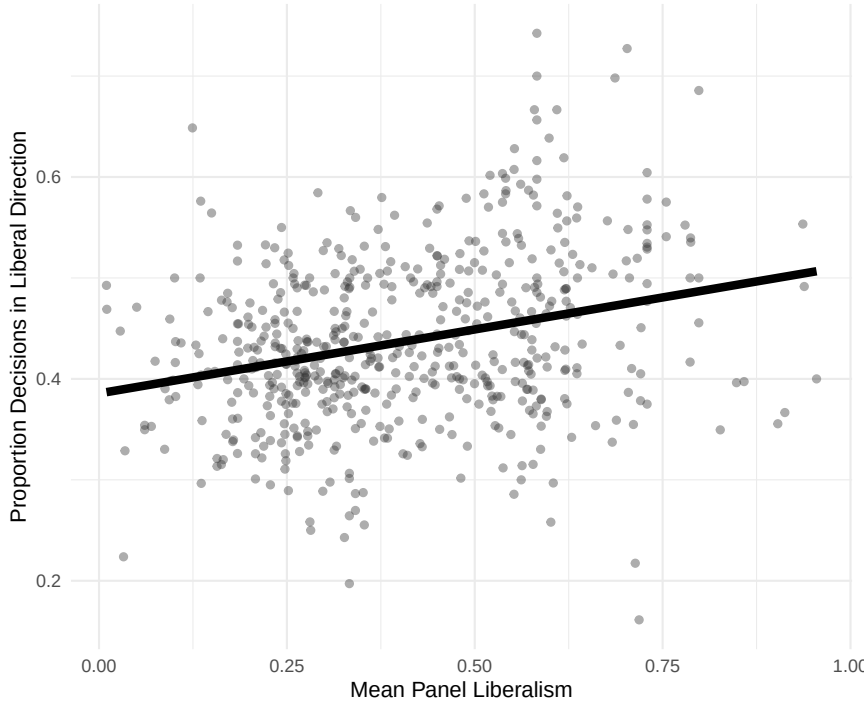


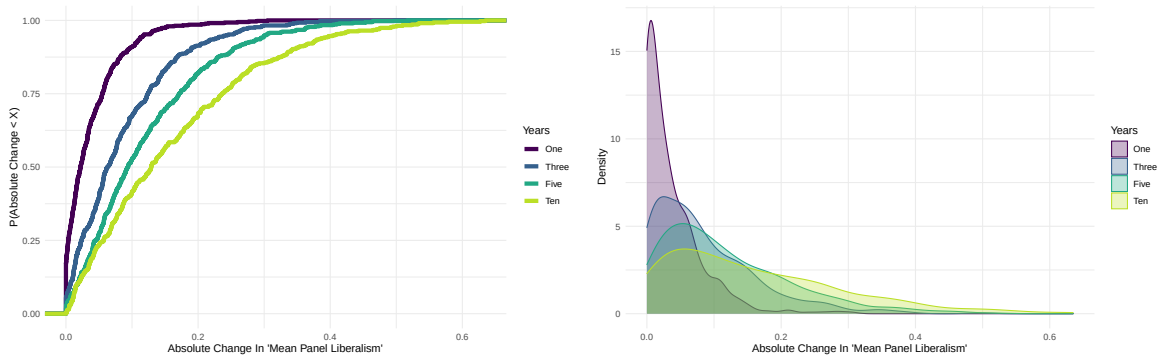
Figure 2.2: Relationship between circuit court liberalism and district court voting

We begin by simply presenting the cross-sectional relationship between liberal district court decisions and circuit court liberalism, with both variables aggregated to the circuit-year level. This is shown, with a linear fit, in Figure 2.2. As this indicates, there is non-negligible positive relationship between the variables before any additional statistical control is introduced.[16] There are, of course, any number of possible explanations for this association beside our hypothesized one. The ideological reputations of particular courts, home-state senator influence in the appointment process (Steigerwalt 2010), and over-time changes could all account for this relationship.

While we unfortunately cannot randomly assign district judges to circuits, we can control for every feature of judges that do not change over time (such as their ideological

[16]The correlation between the two measures is 0.279.

preferences, perhaps), and for every feature of a given time period that is common to all judges (such as national mood for reform). We then rely on changes in circuit court panel ideology, brought about by the replacement of circuit judges, to examine how affected district judges' decisions change. Changes in circuit court liberalism are often gradual, though they are not always so – in Figure 2.3 we plot the distribution of within-circuit changes in our independent variable over one, three, five, and ten years. As this figure shows, over a relatively short period a number of circuits experience significant changes in mean panel ideology, while most experience at least some change. Our design departs from a traditional differences-in-differences setup due to the continuous and relatively slow-changing nature of our independent variable of interest, but nevertheless improves on existing scholarship that relies exclusively on cross-sectional comparisons by controlling for an array of judge- and time period-specific factors.



(a) Empirical CDF **(b)** Density Plot

Figure 2.3: Distribution of within-circuit changes over a period of time

To estimate the consequences of appeals court ideology on district court decision-making, we use ordinary least squares regression with judge and year fixed effects. We use linear regression with a binary outcome as it allows us to retain the desirable "within-unit changes" interpretation when using fixed effects and also allows for straightforward

interpretations, which are lost when using a logit model.[17] District judge fixed effects account for any feature of the judge that does not change over time, such as (possibly) their ideology, party, gender, or race. Year fixed effects account for anything in a year common to all judges: who the president is, the "national mood," prevailing public opinion, the status of Supreme Court precedent, etc. To further control for possible changes in the court docket, we also include fixed effects that control for the types of cases judges face in some specifications. This may take the form of either "case type" fixed effects, which account for systematic differences in the likelihood of a liberal ruling across case types, or "judge-by-case category" fixed effects, which allow for the fact that a judge's unobserved attributes and predispositions to rule a certain way may vary by case category. Our estimating equation takes the form

$$\textit{Liberal Decision}_{jdcit} = \beta \textit{Mean Panel Liberalism}_{ct} + \alpha_{jdc} + \tau_t + \phi_i + \varepsilon_{jdcit}$$

where j indexes district judges, d indexes court districts, c indexes circuit courts, i indexes issue categories, and t indexes time in years. The data, as this suggests, is at the decision level, with decisions nested in judges nested in districts nested in circuits.[18] We present estimates from a variety of specifications below, but this is our preferred specification and provides a reference point for our other tests. To conduct inference, we use cluster-robust standard errors, clustered at the level of treatment (the circuit). This accounts for arbitrary within-cluster correlation, both cross-sectionally and over-time (Bertrand, Duflo, and Mullainathan 2004).[19]

[17] We do, however, report estimates for logit models in the Appendix in Table B.9.

[18] We also present results estimated on data aggregated to either the judge, district, or circuit level; these results are presented in Table B.10, and are generally as or more impressive than the estimates reported in the text.

[19] Cluster-robust standard errors are possibly unreliable with small numbers of clusters (Bertrand, Duflo, and Mullainathan 2004; Donald and Lang 2007); in our case, we have only twelve (the number of circuits). In

2.3 District Judges React to Circuit Court Ideology

In this section we present our main results. We begin with our baseline results, indicating that district judges are responsive to changes in the ideological composition of the circuits they face. We then present further evidence to bolster these results, demonstrating that the result is greater in circuits that are more prone to receive appeals. In the next section, we further explore possible mechanisms, and find no support for either a "progressive ambition" mechanism nor a "workload reduction" mechanism.

2.3.1 Main Results

In Table 2.1, we consider how changes in the mean liberalism of panels that district judges face is associated with liberal voting by district judges. Recall that the key independent variable, "Mean Liberalism of Panels," is rescaled 0-1 so that 0 reflects the empirical minimum and 1 the empirical maximum in our dataset. Thus, a one-unit change is a change from the most conservative appellate court to the most liberal.

Model 1 is a linear model with no fixed effects. This model captures the correlation between circuit liberalism and liberal district court votes. Models 2 through 5 subsequently add to the simple model a number of fixed effects. Model 2 adds judge fixed effects to account for a given district judge's predisposition to vote in a liberal manner;[20] Model 3 adds year fixed effects to Model 2, to account for year-to-year changes in the overall liberalism across all district judges. Model 4 is our preferred specification, adding case

Figure B.1 in the Appendix we show that a variety of approaches to conducting inference, including the use of a wild block bootstrap, a recommended approach for dealing with small numbers of clusters (Cameron, Gelbach, and Miller 2008; Esarey and Menger 2019), produce conclusions very similar to the more-familiar clustered standard errors.

[20]To ensure that we are capturing unique individuals, given the complexity of the dataset, our judge fixed effect is constructed as the intersection of the judge code, party, and appointment year. We also corrected these and other variables for a small number of observations.

type fixed effects to allow for the possibility that in thirty-one different types of cases there may be different underlying predispositions to vote in a liberal direction. Finally, in Model 5 we replace the judge fixed effect with a "Judge-by-Category" fixed effect, where the thirty-one case types are binned into either "Economic," "Criminal," or "Civil Rights/Civil Liberties" categories.[21] This model allows for the possibility that judges may have different ideological leanings in different types of cases; if a judge is a liberal on economics but a conservative on civil rights, this may wash out with a judge fixed effect and they may appear moderate; the judge-category fixed effect, however, will control for this within-judge variation across issue areas.

Table 2.1: Predicting liberal district court voting with the mean liberalism of appellate panels; coefficients from linear fixed effect models (standard errors clustered by circuit)

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.11^{**}	0.08^{**}	0.07^{**}	0.06^{**}	0.06^{*}
	(0.05)	(0.04)	(0.03)	(0.02)	(0.03)
Constant	0.38^{***}				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed xffects				✓	✓
N	99188	99188	99188	99188	99188
Adj. R-squared	0.002	0.06	0.07	0.13	0.14

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Substantively, these effect sizes are meaningful. In a hypothetical scenario where a district judge went from the most conservative (Mean Liberalism = 0) to the most liberal (Mean Liberalism = 1) circuit court in our data, the models suggest that this would result in a six or seven percentage point increase in the probability of a liberal decision. Thinking in

[21] This is necessitated by the relatively small number of cases in some judge-case type bins.

terms of more realistic changes, consider Figure 2.3; more than 25% of circuits will have a change of more than 0.2 in mean panel liberalism over a ten year period, which would in our estimates correspond to a more than one percentage point change in liberal decision-making. In other words, this suggests that one out of a hundred litigants would have their cases decided differently, and their lives altered for better or worse, after such a change in circuit court liberalism. As we note below, our estimates are considerably larger in certain types of cases or districts, suggesting that these aggregate results mask important variation.

We further examine our results by exploring variability in responsiveness across case categories. For each of the specifications from Table 2.1 above, we add in interactions between Mean Liberalism of Panels and indicators for Economics cases and Civil Rights cases, leaving Criminal cases as an omitted category. While our sample consists entirely of cases with some amount of political content, prior work leads us to believe that judges have less discretion in criminal cases and therefore are less likely to be influenced by higher court preferences (Randazzo 2008). The results of this analysis are presented in Figure 2.4.[22] As this illustrates, the effect of mean panel liberalism on district court voting is indistinguishable from zero for both criminal and economic cases, but for civil rights and civil liberties cases the effect is large, positive, and statistically significant. The point estimate for civil rights and liberties cases suggests that a one-unit change in mean panel liberalism is associated with a nearly fifteen percentage point increase in the probability of a district judge voting in the liberal direction.

2.3.2 Heterogeneity by Rate of Review or Reversal

The results in the previous section provide preliminary evidence that district judges respond to the ideological preferences of the panels that review them. Our theoretical

[22]Full results are presented in the Appendix in Table B.5.

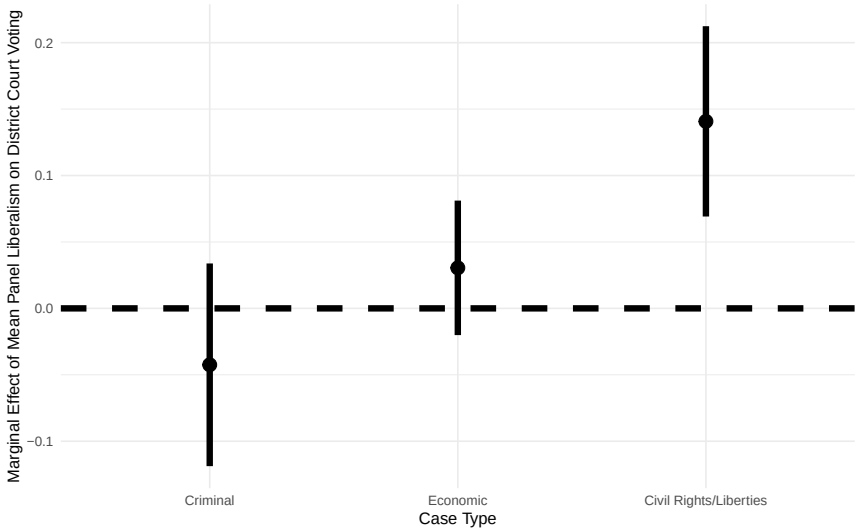


Figure 2.4: Marginal effect of mean panel liberalism by the issue category of the case. Results based on same specification as model 4 in Table 2.1. 95% confidence intervals based on circuit-clustered standard errors.

expectation for this result rests on the idea that appeals courts can review and overturn district court decisions, which district judges view as undesirable. Previous scholarship establishes that concern about being reviewed is a major driver of deference to higher courts (Pérez-Liñán, Ames, and Seligson 2006). Haire, Lindquist, and Songer (2003) demonstrate explicitly that review is more likely when district court decisions diverge from circuit court preferences. While circuit court review or reversal need not occur in any given case for the district courts to respond to circuit preferences through strategic anticipation, we expect that in districts where cases are in fact reviewed or reversed more often, district judges will be more sensitive to the preferences of circuit judges. As we note above, district court litigants have an automatic right of review; therefore, systematic differences in rate of review must arise through some mechanism other than appeals courts' formal discretion. Instead, we posit that litigants' anticipation of success on appeal and the high monetary costs of appeal, paired with appeals courts' varying deference to district court decisions, affect the rate of review. Reversal, despite being rarer and therefore in all likelihood more noisily measured,

is more directly at the discretion of the appeals court itself.

To measure review and reversal, we introduce two new measures. "Rate of Review" is the ratio of the number of cases heard by an appellate court in a given year from a given district to the number of cases terminated in that district-year, while "Rate of Reversal" is the ratio of the number of cases reversed[23] by an appellate court in a given year from a given district to the number of cases terminated in that district-year.[24,25] For both measures we use data from the year prior, so as to be at least plausibly causally prior to the events of a particular year, as well as to use a measure that district judges might have in their head.

The results of these analyses are presented in Figure 2.5.[26] In the left panel, focusing on appellate review, we show that district judges are essentially not responsive to the ideological preferences of the appellate court in the case where appellate courts are not reviewing cases; responsiveness increases, however, as the rate of appellate review increases. While the results for appellate *reversal* are not quite so dramatic, it is again the case that higher levels of appellate reversal corresponds to greater district court responsiveness to appeals court ideology. These results provides important suggestive evidence for our hypothesized mechanism – fear of potentially embarrassing review and reversal of decisions – and further demonstrate that our main results capture real variation in district judge behavior.

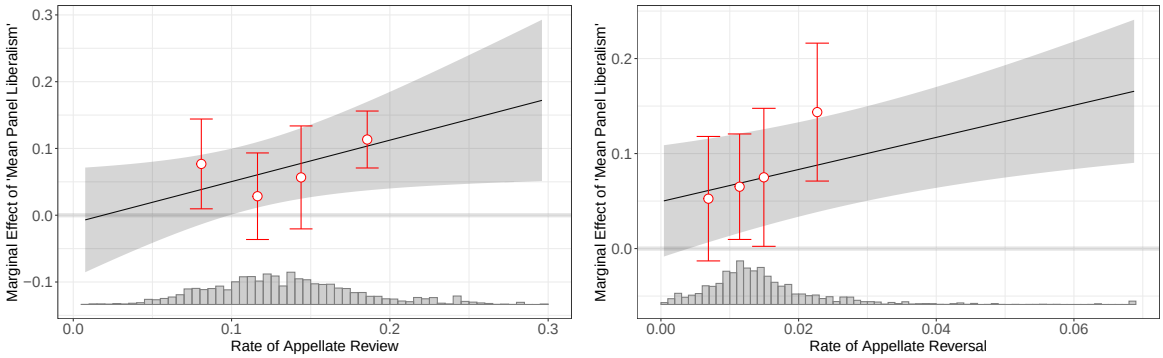
To further demonstrate that these results reflect a trade-off between a judge's personal ideological preferences and those of the appeals court, we also estimate a model where we

[23]We classify any decision as "reversed" if it is reversed in whole or in part.

[24]To construct these measures, we employ the Federal Judicial Center's Integrated Database, available at https://www.fjc.gov/research/idb. For criminal cases, the unit of analysis is the defendant, rather than the case. The Integrated Database has data from 1971 onward, so our analysis using this measure covers 1971-2012. We remove one notable outlier from the reversal data (the Eastern District of Texas in 1993); if we retain this outlier, our results are stronger than those reported.

[25]The "Rate of Reversal" measure explicitly uses the total count of cases in a district as the denominator, rather than the number of reviewed cases, since review itself is likely a function of ideological disagreement, making conditioning on it inappropriate.

[26]Full results are presented in Tables B.1 and B.3 in the Appendix.



(a) Review **(b)** Reversal

Figure 2.5: Marginal effect of mean panel liberalism by the rate of appeals court activity (either review or reversal) from the previous year. Results based on same specification as Model 4 in table 2.1, covering 1971-2012. Histogram on x-axis show the distribution of rate of appellate activity. Straight line represents linear fit of the marginal effect; dots represent estimates based on binning the rate of review variable (Hainmueller, Mummolo, and Xu 2019). 95% confidence intervals based on circuit-clustered standard errors.

interact each judge's party,[27] as well as circuit court liberalism, with the rate of review. We find in these models, presented in Table B.2 in the Appendix, that the relationship between circuit court liberalism and liberal district court votes is increasing in the rate of review, as we find above, but we also find that the effect of the judge being a Democrat is *decreasing* with the rate of review. This suggests that district judges trade off their own preferences for higher courts' when rates of review are high. However, we do not find this relationship with rate of reversal (Table B.4).

2.4 Progressive Ambition, Workload, or Neither?

In this section we put to the test two additional hypotheses proffered by earlier scholarship for why judges are likely to be responsive within the judicial hierarchy. First, we examine the role of "progressive ambition," or judge's desire for a promotion. Second,

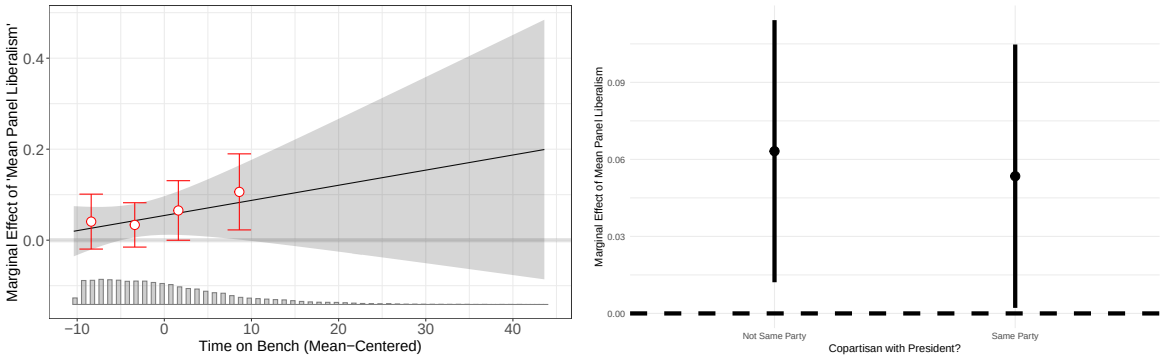
[27] We use the party codes from Carp and Manning (2016), and omit any judge with multiple party codes from the analysis.

we consider "workload" factors, to ascertain whether judges are simply acting so as to minimize their workload. We ultimately find support for neither mechanism, albeit the coarseness of our measures leave considerable room for future scholarship in these areas.

To test whether progressive ambition is a mechanism at work in our sample, we use two proxies that are commonly held to relate to a judge's promotion potential. Both Budziak (2013) and Bratton and Spill (2004) emphasize the importance of partisan alignment between those responsible for promotion and the judge and the length of service that a judge serves on the court, which is expected to be negatively related to promotion. Specifically in the context of district courts, Savchak et al. (2006) shows that being a copartisan of the current president is a strong predictor of elevation from the district courts to the Courts of Appeals, while Campbell and Wilcox (2020) shows that older district judges are less supportive of presidential power, likely because they are less likely to be promoted. It stands to reason that if the pressure to avoid reversals is driven by progressive ambition, it should be greatest when district judges are in partisan alignment with the president and are relatively early in their district court careers.

Figure 2.6 presents results for our two tests of whether progressive ambition is a mechanism through which our documented responsiveness operates. Both are estimated based on adding an interaction term to our preferred specification (Model 4) above. The left panel presents evidence for "time served on court," and the right panel for copartisanship with the president. We find no significant difference across time served nor between copartisans and non-copartisans of the president.[28] While the marginal effect of mean panel liberalism is significant only in the highest bin of time served, this result is not significantly different from the lower bins, and the difference is not signed correctly per our hypothesis. We thus find no evidence that progressive ambition motivates responsiveness to circuit courts by

[28]Results for these models can be found in the Appendix in Tables B.6 and B.7.



(a) Marginal Effect of Time Served

(b) Marginal Effect of President's Party

Figure 2.6: Figure on left plots the marginal effect of mean panel liberalism by each judge's time served in office. Results based on same specification as Model 4 in table 2.1. Histogram on x-axis shows the distribution of (mean-centered) time served. Straight line represents linear fit of the marginal effect; dots represent estimates based on binning the rate of review variable (Hainmueller, Mummolo, and Xu 2019). Figure on right plots the marginal effect of mean panel liberalism depending on whether judge is in the same party as the current president . 95% confidence intervals based on circuit-clustered standard errors.

district judges. This does not, of course, rule out strategic behavior aimed at progressive ambition – it may simply be that avoiding being overturned is not the key pathway through which progressive ambition operates. Black and Owens (2016), for example, show that circuit judges who are contenders for Supreme Court vacancies do adapt their behavior to please the president, but actually dissent more in doing so. Avoiding being overturned may therefore not be a first-order consideration for judges pondering their career advancement.

To test the workload mechanism, we interact our measure of panel liberalism with a measure of judicial caseloads. We use the judicial caseload measures developed by Habel and Scott (2014), which measure caseloads by district-year from 1964 to 2012.[29] Our claim is that if a mechanism through which responsiveness operates is fear of increasing one's workload, then district judges in districts with higher workloads ought to be more

[29]Specifically, we use the measure of total pending cases per district-year divided by the number of judges serving in that district-year, which captures the average workload for a judge serving on a given district in a given year.

responsive than those with more time on their hands (lower workloads).

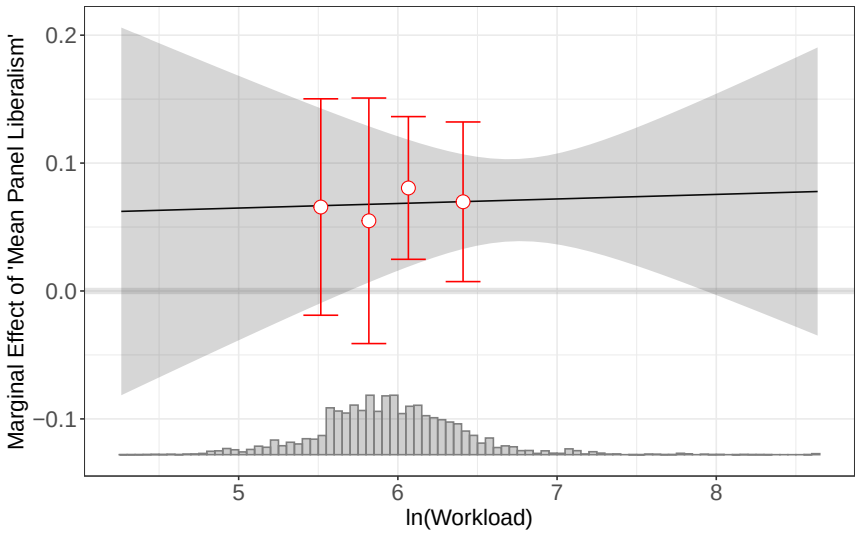


Figure 2.7: Figure plots the marginal effect of mean panel liberalism by the natural log of the caseload faced by the district court. Results based on same specification as Model 4 in table 2.1. Histogram on x-axis show the distribution of the workload variable. Straight line represents linear fit of the marginal effect; dots represent estimates based on binning the workload variable (Hainmueller, Mummolo, and Xu 2019). 95% confidence intervals based on circuit-clustered standard errors.

Figure 2.7 presents the results of a regression where we allow the effect of mean panel liberalism to vary with this measure of district court workload.[30] We find no evidence of meaningful variation in responsiveness across levels of workload; if anything, the relationship goes in the opposite direction. We therefore find no evidence that the deeply practical consideration of minimizing one's own workload drives district judges' responsiveness to appellate panels. District judges appear to modify their behavior in response to changing appellate ideology for reasons other than progressive ambition or workload pressures. As we note above, previous scholarship identifies concern about reputation among peers and self-esteem as another, less-measurable mechanism through which district court responsiveness may operate (Baum 2009, 95, Randazzo 2008, 673, n. 5). However, the tests

[30]Full results for each of the specifications in Table 2.1 are presented in Table B.8 in the Appendix.

presented here ought not to forestall further investigation into the mechanisms through which the judicial hierarchy influences lower court decision-making; alternative measurement strategies and approaches may well reveal instrumental factors influencing responsiveness.

2.5 Robustness

We undertake a number of additional tests to demonstrate the robustness of our results to alternative methods of inference, functional forms, and measurement strategies. The results are presented in the Appendix. We explore alternative methods of conducting inference (Figure B.1), using a logit model (Table B.9), aggregating to different levels of analysis (Figure B.10), estimating models with circuit-specific time trends and lagged dependent variables (Table B.11), and alternative measures of circuit court liberalism (Tables B.12 and B.13). Across most of these models, we continue to find positive and statistically significant effects of circuit court liberalism on district court decision-making.

We also explore the importance of our particular sample for our conclusions. To do so, we iteratively omit circuits (Figure B.2), decades (Figure B.3), and issue areas (Figure B.4), and re-estimate our main specification. Across these various models, we find little evidence that any particular circuit, decade, or issue area is unduly influencing our results.

2.6 Conclusion

In this paper, we explore the effectiveness of circuit courts at shaping the rulings of district courts in the U.S. federal judiciary. In doing so, we examine a relationship that has interested generations of scholars, lawyers, and litigants. Howard (1981/2014) notes that "In theory, of course, federal judges form a pyramid that supports the will of the [Supreme

Court] Justices. In reality, federal judicial power is widely diffused among lower court judges who are insulated by deep traditions of independence, not only from other branches of the government but also from each other" (3).[31] Our project explores the success of these countervailing currents of hierarchy and independence in an underexplored corner of the federal judiciary, the district courts.

We find that district judges do adapt their decision-making to the appellate panels that they are likely to face. Even after accounting for judge-specific attributes, year-specific attributes, and case-specific attributes, we find that changes in the liberalism of circuit court panels are associated with corresponding changes in district court decisions. We show further that this responsiveness is highest in circuits with the highest proclivity to review district court cases, suggesting that fear of review and reversal is, in fact, the mechanism. Digging further, we find no evidence that this fear is motivated by hope for promotion or fear of increasing one's workload.

We expect and hope that this paper does not represent the final effort to understand the relationship between district courts and appeals courts in the federal judicial hierarchy. Further evidence on the mechanisms through which hierarchical responsiveness operates is needed. Nevertheless, our documentation of responsiveness at a key level of the American judicial hierarchy, arrived at with a research design that controls for a broad array of potential confounders, provides compelling evidence for the importance of appeals court judges in the federal judiciary. While existing scholarship suggests that the combination of lifetime appointments and the limited hope of promotion or review that appeals court judges face allows them to operate quite independent of Supreme Court oversight, we show here that these same judges exert considerable influence over the largest level of the federal bench, the district courts. With a single judicial replacement at the circuit court level, a

[31] Quoted in Haire, Lindquist, and Songer (2003, 1).

president can influence a broad swath of the federal judiciary, molding decisions on key issues toward his or her ideological preferences.

3 | Does the Supreme Court Constrain Circuit Court Behavior?

American federal courts have a hierarchical structure, with two levels of appellate courts (the circuit courts and the Supreme Court) above the trial courts. While much of political science research focuses on the Supreme Court, about 50,000 cases are filed and over 30,000 cases are decided on the merits each year in the circuit courts,[1] whereas the Supreme Court hears fewer than 100.[2] This low rate of review raises an important question. How effectively can the Supreme Court constrain circuit court decision-making? Furthermore, if the low rate of review means that the Supreme Court's supervisory powers are limited, do lower courts nonetheless rule in line with the Supreme Court for other, indirect reasons, such as the shared appointment process between the lower federal courts and the Supreme Court?

The imbalance between the caseloads of the Supreme Court and the circuit courts thus means that, even if circuit judges prefer not to be reversed, reversal is a very unlikely outcome in the typical case (Bowie and Songer 2009). For this reason, previous research has generally found that congruence between circuit courts and the Supreme Court is low or nonexistent (Benesh 2002; George 1997; Klein 2002; Reddick and Benesh 2000; Revesz 1997). Yet it is difficult to measure agreement between the Supreme Court and the circuit

[1] See https://www.uscourts.gov/statistics/table/b-1/judicial-business/2020/09/30, which shows that 32,796 cases were terminated in the year leading up to September 30, 2020.

[2] The Supreme Court decided only 61 cases on the merits in the 2019 Term (Feldman 2020).

courts on particular legal issues, given that very few legal issues are reviewed. That is, the fact of the low rate of review means we do not know how the Supreme Court would have ruled on most cases that are decided by the Courts of Appeals.

Using a novel empirical strategy, I show that when the chance of Supreme Court review is unusually high, there is moderate agreement between the Supreme Court and the circuit courts. When it is low, as in the typical case, circuit courts are as likely to disagree as agree. I do this through looking at resolved circuit splits, where multiple circuits have considered a legal issue and the Supreme Court has ultimately decided in favor of one position. By looking at the development of Supreme Court agreement over time, I can leverage the changing possibilities of review: before multiple circuits have weighed in, the probability of review is very low. That congruence goes up after the first case in a split thus suggests that the Supreme Court's decisions about what it reviews does affect circuit court behavior, but the small magnitude of the effect indicates that circuit courts have a great deal of independence, even when the Supreme Court is most likely to intervene.

This has several implications for the study of American courts. First, the Supreme Court's decisions about which cases it puts on its agenda are likely at least as important to its control of the lower courts as the outcomes it reaches in the cases it chooses to take. Circuit judges are unlikely to respond to general ideological shifts in the Supreme Court, given the low rate of review, but they may shift their behavior if the high Court begins to grant cert on different kinds of cases.[3] Second, the circuit courts have a great deal of independence from the Supreme Court and thus deserve more scholarly attention. Third, the shared appointment process for federal judges is not enough to lead Supreme Court and circuit court judges to vote together more than one would expect by chance, indicating that the President's power to shape the federal judiciary as a whole through Supreme Court

[3]Other work also suggests this is true. For example, Strayhorn (2020) shows in a formal model how the Supreme Court prioritizing the resolution of circuit conflicts could help create those conflicts in the first place.

appointments is limited.

3.1 The Puzzle: Why Don't the Circuit Courts Agree with the Supreme Court?

Before we can analyze whether Supreme Court behavior affects circuit court behavior, we need a good estimate of the correlation between the two. However, deciding how to measure this correlation is difficult. One problem is that the Supreme Court takes a non-random sample of appellate court cases (e.g., Caldeira and Wright 1988; Provine 1980). Thus, one cannot use the high rate of reversal in the cases that the Supreme Court actually takes[4] as evidence that the lower courts are out of sync with the Supreme Court; rather, this could entirely be driven by the Supreme Court's well-known penchant for following a "reversal strategy" whereby it disproportionately chooses to hear lower court decisions that it wants to reverse (Boucher and Segal 1995; Segal and Spaeth 2002).

One can instead look at a broader universe of circuit court decisions and compare those to the Supreme Court's behavior. Using the U.S. Courts of Appeals Database (Songer 2008; Kuersten and Haire 2011) and the Supreme Court Database (Spaeth et al. 2020), the correlation between liberal voting by year at the circuit court level[5] and liberal voting by year at the Supreme Court level is 0.31 – nonzero, but not large (see Figure 3.1).

However, as shown in Figure 3.2, the correlation entirely disappears when one looks within decades. A regression of yearly liberal voting at the circuit level on yearly liberal Supreme Court voting and decade fixed effects shows a non-significant *negative* effect of Supreme Court liberalism on appellate liberalism (coefficient -0.1). As seen in Figure 3.2,

[4] In the 2019 Term, for example, it was a striking 68% (Feldman 2020).

[5] Since the Songer database is a random sample of published appellate cases, this is a weighted mean based on what proportion of the total cases in a given circuit were sampled each year.

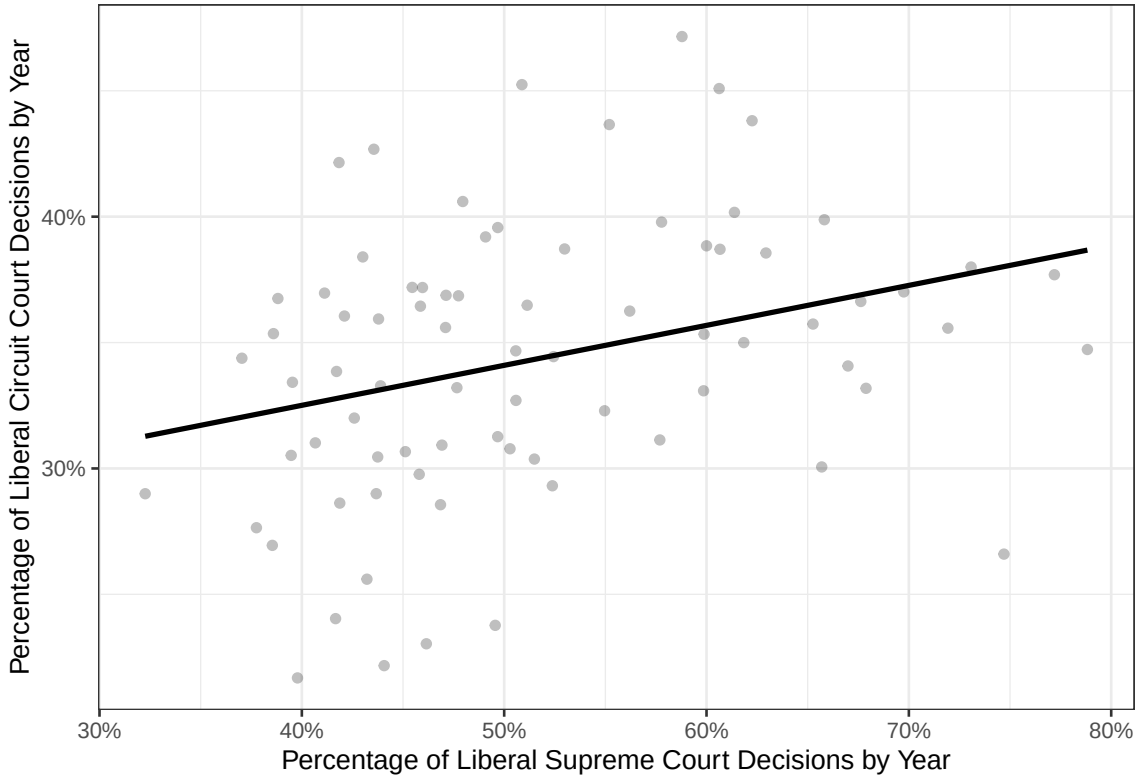


Figure 3.1: Percentage of liberal circuit court decisions by year by percentage of liberal Supreme Court decisions by year

the only positive within-decade correlations exist for the 1920s-1940s, and the largest of those (for the 1930s) is entirely driven by the outlier of 1938. This may be suggestive evidence that a suitably large shift in Supreme Court jurisprudence such as that following the "switch in time that saved nine," may lead to immediate changes in circuit court voting, but it appears that ordinary Supreme Court behavior does not have such an effect. In the Appendix (Table C.2), I show that this lack of correlation is not caused by some particularly large circuits being unresponsive to the Supreme Court; the within-circuit correlation is similar to the correlation one sees when looking both across and within circuits. Much like the overall correlation, the within-circuit correlation disappears when one controls for decade fixed effects.

One may wonder whether taking a random sample of published circuit court cases is

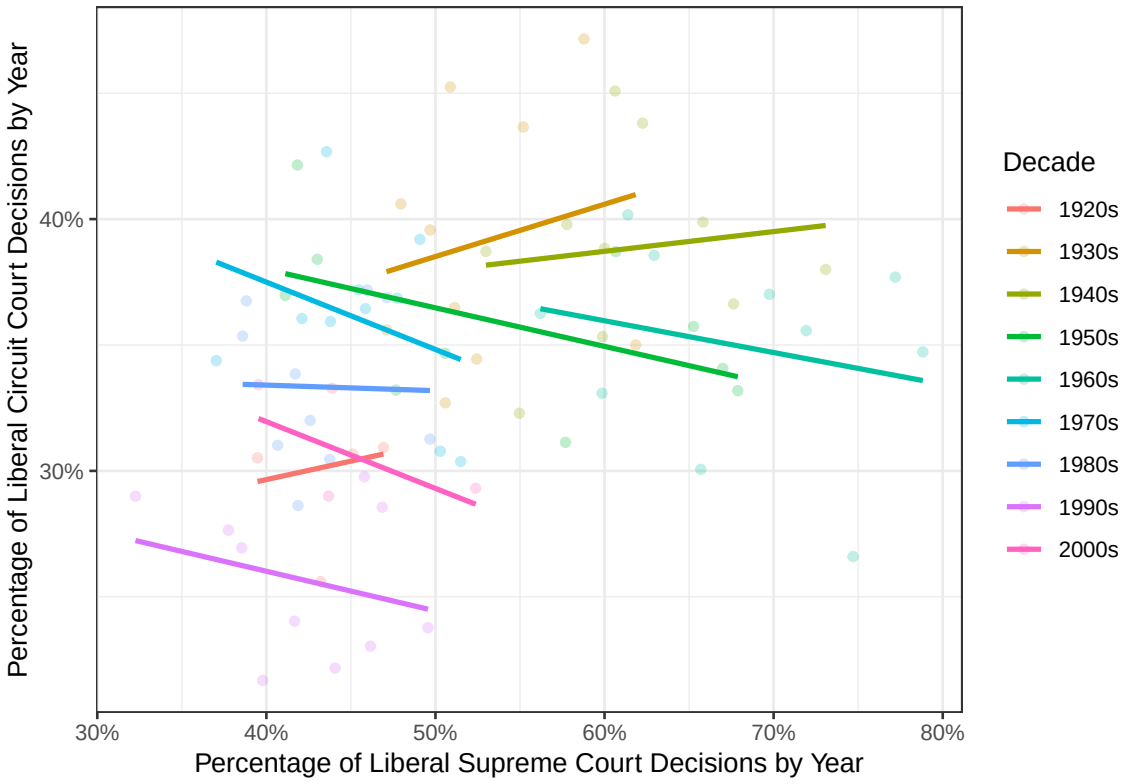


Figure 3.2: Percentage of liberal circuit court decisions by year by percentage of liberal Supreme Court decisions by year, with colors and regression lines by decade

the proper approach to measuring agreement between the Supreme Court and the Courts of Appeals. Again, because the Supreme Court reviews a non-random sample of lower court decisions, the types of cases heard by the Supreme Court and the circuit courts will differ on average in many ways, some easily observable (such as the issue area) and some not easily observable (the legal strength of each side's case). This is not merely a problem of data collection, either; this would not be solved if we had a database of the ideological direction of all circuit court cases. However, this finding of no correlation within decades should lead one to be broadly skeptical of the claim that the Supreme Court imposes tight constraints on circuit court behavior. Other approaches lead to the same conclusion. For example, Cross (2005) looks at the overall correlation between circuit court voting and Supreme Court ideology and finds that the effect of Supreme Court ideology is *negative* after taking into account circuit court ideology.

What about when one looks at more fine-grained measures of circuit court responsiveness to the Supreme Court? Several studies have found that the circuit courts do move in response to the Supreme Court (Cross 2003) but that the effect is small compared to other factors, such as a judge's own ideology (Benesh 2002; George 1997; Reddick and Benesh 2000; Revesz 1997). Klein (2002) found no responsiveness to the Supreme Court at all. Bowie, Songer, and Szmer (2014) subset the U.S. Courts of Appeals Database to cases where Supreme Court review was relatively high and found a larger effect of judicial partisanship in those cases, which is inconsistent with the hypothesis that judges are strategicially voting against their attitudes in those cases.

The most persuasive evidence concerning the Supreme Court's effect on circuit court voting has come from studies focused on individual issue areas, where at least one aspect of the case can be held constant between the two levels of the judiciary. Evidence against a relationship comes from Cross (2005), which finds a negative effect of Supreme Court ideology after taking into account circuit court ideology when subsetting to criminal cases

and to civil rights cases. However, crime and civil rights are relatively large categories; there could still be a good deal of difference between the types of criminal and civil rights cases the appellate courts hear and the Supreme Court hears.

When looking at more fine-grained issues, it does appear that lower courts' behavior is correlated with the Supreme Court's. In search and seizure cases, the same case facts that predict the Supreme Court upholding a search tend to predict an appellate court upholding a search (Songer, Segal, and Cameron 1994). That shows at least some congruence between appellate court behavior and Supreme Court behavior. The same study showed that as the Supreme Court became more conservative on search and seizure cases, the appellate courts also became more conservative, suggesting responsiveness as well as congruence. Further evidence of congruence appears in cases involving *Chevron* deference, where panels tend to defer to the agency (following the Supreme Court's decision in *Chevron U.S.A., Inc. v. Natural Resources Defense Council, Inc.*[6]) as long as they agree with the agency or if the panel is divided by party; they tend not to defer, however, in all-Democratic or all-Republican panels which are ideologically opposed to the agency (Cross and Tiller 1998).

Thus, it appears that at least in some issue areas there is a positive association between circuit court voting and Supreme Court voting. However, subsequent work has suggested that little of this comes from reversal aversion, the phenomenon whereby lower court judges strategically vote in order to avoid reversal by a higher court (Klein and Hume 2003). In search and seizure cases where the Supreme Court was more likely to grant cert, there was no stronger relationship between appellate court voting and expected Supreme Court voting than in the cases where it was less likely to grant cert (Klein and Hume 2003). While the greater adherence to *Chevron* deference in divided panels could be because cases with divided panels had a greater risk of reversal, Cross and Tiller (1998) notes that this is

[6]467 U.S. 837 (1984).

unlikely given the low rate of Supreme Court review[7] and the greater level of ideological voting among majority-Democratic panels, despite the Supreme Court being relatively conservative at the time.

Unfortunately, looking at individual issue areas has at least one important cost to weigh against the benefits noted above: what holds true in search and seizure cases or *Chevron* cases may not hold true in circuit court cases as a whole. Thus, it would be helpful to be able to analyze a dataset that spans multiple issues while still being able meaningfully to compare circuit votes to Supreme Court votes.

3.2 Why Circuit Courts Are (Usually) Relatively Unconstrained

While the lack of correlation between Supreme Court and circuit court voting shown in Figure 3.2 may seem surprising, there are good theoretical reasons to be skeptical that a correlation should exist. The most important reason is the low rate of review of circuit courts by the Supreme Court (Bowie and Songer 2009; Cross 2003). The ratio of merits opinions at the circuit level to the Supreme Court level for the Supreme Court 2019 Term and the circuit court year leading up to September 30, 2020 is 538 to 1. This means that the Supreme Court is doing more than just weeding out the politically or legally uninteresting cases. Given this very low rate of review, even if reversal aversion plays a part in circuit court decision-making, it thus is unlikely to play a role in most cases.

In the absence of direct constraint from the Supreme Court, there could be indirect responsiveness. However, the mechanisms for that are weak as well. First, circuit court judges have life tenure. This insulates them both from outside political pressure and from

[7]For a further discussion of how Supreme Court review is generally low, even in cases that are more likely to be reviewed, see Bowie and Songer (2009).

pressure from other parts of the judicial hierarchy. Just as life tenure gives greater room for Supreme Court justices to vote their preferences (Segal and Spaeth 2002), it gives similar insulation for lower court judges. If circuit court preferences do not hew very closely to Supreme Court preferences (see below), then preferential voting will not lead to congruence.

Second, circuit judges may have similar preferences to Supreme Court justices, but this is hardly guaranteed. It is natural to hypothesize that the shared appointment mechanism of the two sets of actors (both are nominated by the President and confirmed by the Senate) should lead to some level of shared preferences. However, this pressure is weak. The small size of the Supreme Court means that presidents' abilities to affect the Court's composition will vary a great deal simply by chance. For example, Jimmy Carter appointed no Supreme Court justices, while Donald Trump appointed three, despite both presidents serving for one term. Furthermore, presidents may not care about all the policy issues that appear before the courts. The policy agenda may change over individual judges' tenures to issues for which the candidates were not vetted, which would depress agreement among judges with the same appointing president. An example of this is the change of the Supreme Court's agenda from New Deal issues (on which the FDR and Truman appointees were solid pro-New Deal votes) to civil rights issues (on which those appointees differed among themselves).

Third, the differences between the circuit courts and Supreme Court highlighted in Segal and Spaeth (2002) do not obviously lead to greater circuit court agreement with the Supreme Court. As noted above, the effect of reversal aversion is limited by the low rate of reversal. While circuit court judges may have progressive ambition (i.e., to become Supreme Court justices), this likely will affect only a few circuit court judges since most will never make a presidential appointment shortlist (Black and Owens 2016; Nemacheck 2007). Further, it is not obvious that poor agreement with the Supreme Court necessarily

will harm a judge's chances of promotion; for example, Republican judges who take more restrictive views of abortion rights than the Supreme Court does presumably do not harm their chances of promotion, but rather improve them. While circuit court judges have a greater formal requirement to adhere to precedent, this can cut in multiple directions. On the one hand, adherence to Supreme Court precedent clearly would lead to greater agreement with the Supreme Court. However, since most cases will not involve issues upon which the Supreme Court has spoken directly, there will not be precedent at the Supreme Court level to consider. Rather, there will often be a more detailed line of precedent developed within the circuit to which circuit court judges are also bound. Presumably, the main way by which the Supreme Court can keep circuit precedent in line with its own preferences is through reversal, but its ability to do this is limited by the small size of the Supreme Court's docket.

Finally, circuit court judges and Supreme Court judges may simply find themselves in agreement to the extent that a) there is a correct outcome to the case, b) judges want to rule in accordance with that outcome, and c) on average tend to come to the right answer more than the wrong answer. However, this is also a relatively weak pressure, since while circuit court judges often will hear cases that are legally or factually "easy," the Supreme Court usually does not. Yet this can be justified in hard cases by taking the view of Dworkin, that "even when no settled rule disposes of the case, one party may nevertheless have a right to win" (Dworkin 1977, 81). If even in more difficult cases, there may be a "right answer" (see also Dworkin 1985) and if judges are pulled toward it, this could lead to some level of congruence.

Thus, circuit judges face a utility function like the following:

$$U_j = f(x_j, r_c s_i, p_c, l_i)$$

where U_i is the judge's utility and the subscript j indicates judges, c indicates cases, and i indicates legal issues. x_j represents a judge's ideology. r_c is the probability of review for the given case and s_i is the Supreme Court's preferred outcome for a given legal issue. p_c is the status of precedent at the time of the case, while l_i is the correct legal resolution of the issue (to the extent this exists, which may not be the same as current precedent).[8] The Supreme Court's ability to impose its preferred outcome s_i on the circuit courts is limited by generally minuscule size of r_c.

3.3 Data: Circuit Splits

I use a novel dataset to test whether the Supreme Court is able to constrain circuit court behavior through the threat of reversal. It is based around circuit splits, where multiple circuits have heard (and disagreed) on a single legal issue. When the Supreme Court resolves such a split, it allows one to compare agreement with the Supreme Court across multiple circuits on precise legal questions, rather than having to aggregate up to broad issue areas.

Circuit splits are a particularly useful data source for examining the effect of Supreme Court review on appellate court behavior. Once there is a circuit split, the probability of Supreme Court review skyrockets. Cert petitions from cases where there is a circuit split are more likely to be granted (George and Solimine 2001). In fact, one third of circuit splits are taken up by the Supreme Court (Beim and Rader 2019), which is much higher than the base rate of Supreme Court review. If reversal aversion exists anywhere, it should exist here.

In my data collection, I started with those cases in the Supreme Court Database (SCDB) (Spaeth et al. 2020) which were coded as having a "Reason for Granting Cert" of either

[8]Note that the correct resolution of the legal issue, in this theoretical model, does not vary across time and across circuits, but the status of precedent does.

"federal court conflict" or "federal court conflict and to resolve important or significant question" in the 1996-2016 Terms. I then used the Supreme Court's descriptions of the splits to categorize the lower court decisions,[9] and inspected the lower court decisions to see how each judge voted on the issue for which there was a split.[10] I coded all the votes as liberal or conservative as the Supreme Court votes were coded in the SCDB. I combine this original data collection of all the circuit votes in the splits resolved by the Supreme Court in the 1996-2016 Terms with the circuit splits collected by Lindquist and Klein (2006) and used by Clark and Kastellec (2013), which cover the 1985-1995 Terms. Thus, I have all splits that were resolved in the 1985-2016 Terms. Since a split may begin many years before it is resolved, some observations come before 1985; the earliest circuit case is from 1953 even though the earliest Supreme Court case collected is from the 1985 Term.

3.3.1 Descriptive Statistics

When circuit splits are resolved by the Supreme Court, who wins? Does the Supreme Court tend to side with the position that a greater number of circuits supported? We see in Figure 3.3 that the Court indeed favors the position that did better at the circuit level, consistent with Lindquist and Klein (2006). As the percentage of circuits supporting the liberal position in a circuit split increases, the probability of a liberal Supreme Court resolution to

[9]In order to avoid researcher bias in the characterization of the splits, I deferred to the Supreme Court's description of which circuits supported which side as long as it was present. If the Supreme Court cited more than one case from the same circuit, I include them all; thus, it is possible for a circuit to appear multiple times in a legal issue, although for simplicity I generally refer to "the first circuit in the split," etc., rather than "the first case in the dataset on an issue on which the circuits split." This, again, is to avoid researcher bias in deciding which case was more "important" or "on point." When the Supreme Court did not adequately describe which circuit supported which side of the split, I would look to the opinions in the decision below and, if that was insufficient, the petition for certiorari to attempt to fill in the gaps. The cert petition is the least desirable source for categorizing a split, since the petitioner will have a strong incentive to characterize the split in a favorable way. If the Supreme Court simply cited a representative example of each side of the split, and I could not find a fuller account of the split, I nonetheless included those two cases.

[10]This is not equivalent to counting votes, as sometimes judges will concur but disagree on the issue for which there is a circuit split, or will dissent but agree with the majority on the relevant issue.

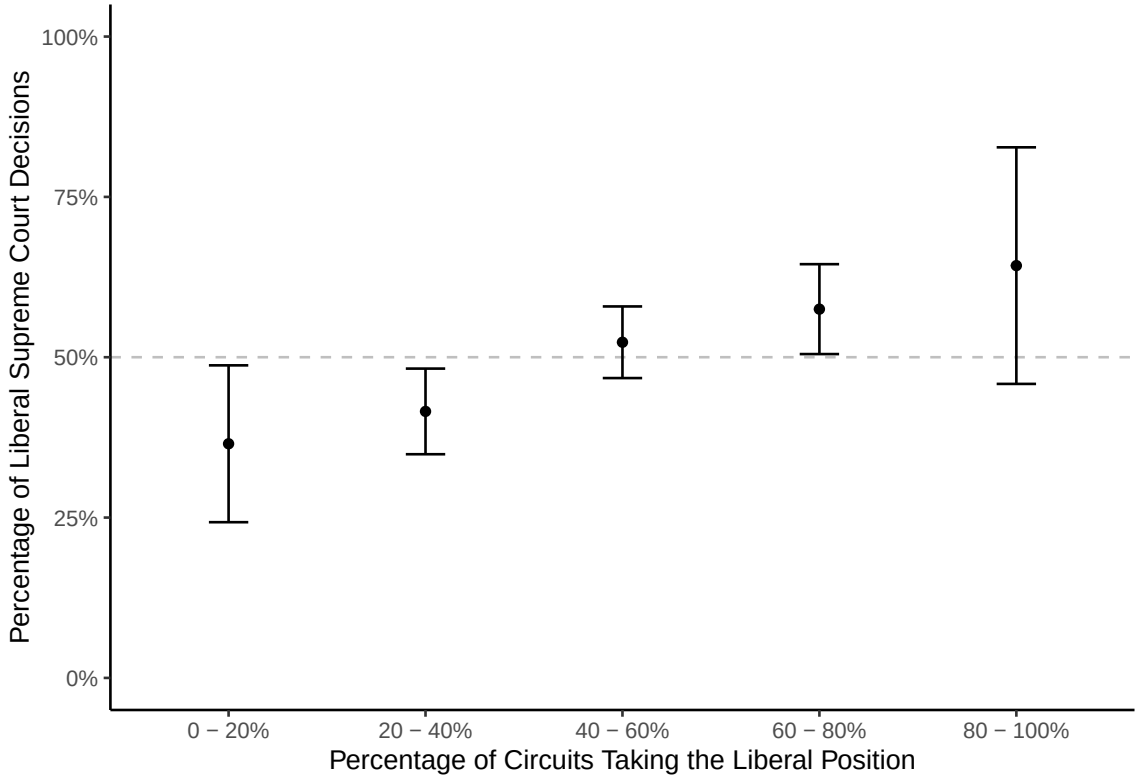


Figure 3.3: Percentage of cases where a circuit decision agrees with the Supreme Court's decision by the percentage of circuit cases to take the liberal position on that issue

the split also increases. While Figure 3.3 presents binned means, we can see the relationship is almost linear. Each gain of a circuit on the liberal side appears to count about as equally as any other; there is no "tipping point" where the Court suddenly becomes much more or less likely to resolve the split in a liberal way.

Even though the Supreme Court is generally considered to be at least moderately conservative over this period, the Supreme Court does not appear to vote more conservatively than the circuit courts on these issues. When fifty percent of the circuit courts take a liberal position, the Supreme Court also takes the liberal position about fifty percent of the time.

Table 3.1 shows a crosstab of the Supreme Court's resolution of circuit splits and whether a majority of circuit courts supported the conservative side or the liberal side (or if there was a tie). When the circuits are tied, the Court is about as likely to pick the con-

servative position as the liberal one. However, when the conservative side is more popular among the circuits, the Supreme Court tends to rule conservatively (58%), and when the liberal side is more popular, the Supreme Court tends to rule more liberally (57%).

Table 3.1: Cross-tabulation of Supreme Court resolution of circuit splits and whether a majority of circuits supported the conservative or liberal side

	Majority Conservative	**Tie**	**Majority Liberal**	Total
Conservative Victory	173	119	124	416
	58%	48%	43%	
Liberal Victory	125	128	162	415
	42%	52%	57%	
Total	298	247	247	831
	100%	100%	100%	

Note: $p < 0.05$, Pearson's chi-squared test

However, it is not always the case that the Supreme Court follows the majority of circuit courts. In fact, in those issues where the circuits get off on the wrong foot – that is, the first case in a split rules against the position the Supreme Court ultimately takes – the Court follows the majority of circuits only 39% of the time.

3.4 Empirical Strategy

There are many possible interpretations of the relationship between circuit voting and Supreme Court voting shown in Figure 3.3. Perhaps the circuits are anticipating what the Supreme Court will do and voting accordingly (reversal aversion). Perhaps the Supreme Court follows the lead of the majority of circuits. Or perhaps the Supreme Court and the circuits vote similarly for another reason (e.g., shared preferences, one legal argument being stronger than the other).

Likely all of these play a part. However, we can test the reversal aversion hypothesis by taking advantage of a feature of the circuit splits data: *there can't be a split without a second circuit.* Thus, the possibility of Supreme Court review is high for the second case onward, but is more ordinary for the first case.[11] If circuit courts are anticipating the Supreme Court's position and attempting to avoid reversal (the first interpretation given above), there is a natural empirical implication: *agreement with the Supreme Court should be lower on average for the first circuit to decide an issue than for subsequent circuits.* In fact, agreement with the Supreme Court should be close to 50% for the first case, since these cases are much more similar to the broad universe of cases in the Songer Database for which we saw no short-term responsiveness to the Supreme Court (in Figure 3.2).

The simplest empirical strategy thus is to compare the mean agreement with the Supreme Court's disposition for the first circuit to consider an issue to all subsequent circuits to consider the issue. A more sophisticated approach will consider several alternative explanations of why judges may agree with the Supreme Court. For example, a majority-Democratic panel is more likely to rule in a liberal direction and a majority-Republican panel is more likely to rule in a conservative direction (Sunstein et al. 2006); since the lower court judges share an appointment process with the Supreme Court, the long-term partisan shifts in the lower courts will in general track the Supreme Court and can create the illusion of direct responsiveness. Further, the status of precedent will change over time and within each circuit; this is an important factor to consider to the extent that the circuit courts are bound by precedent for reasons other than fear of reversal. And to the extent that there are issue-specific factors that make one side of the split stronger than the other, this

[11] Of course, we cannot be certain that the first case cited is actually the first case to consider an issue. Measurement error in the Supreme Court's citation practices will introduce error in this measure of circuit split development and thus will likely bias downward any estimate that compares the first case cited to subsequent cases. To obtain the dates each case was filed, I used data from CourtListener (https://www.courtlistener.com/api/bulk-data/clusters/), supplemented by manual entry for cases with missing dates.

also must be taken into account.

A fixed effects approach is the best way to address these concerns. I will present specifications that include fixed effects by circuit-decade, which addresses changes in the overall liberalism of different circuits over time, as may happen through the accumulation of precedent. I also run a specification that adds fixed effects by issue; given that some of these cases were heard by very few circuits, I run that model on a subset of the cases for which at least five circuits weighed in. For all specifications, I construct a variable called "expected agreement," which equals 1 if the Supreme Court's outcome was liberal (conservative) and the panel was majority Democratic (Republican). In all the models presented in the Results section, the standard errors are clustered by circuit-decade.

A threat to this empirical strategy is if the litigants are strategically choosing which circuit to file in such that the first litigant is more likely to file in a circuit out of line with the Supreme Court than subsequent litigants. In that case, we would see a difference between the average outcomes of the first case and subsequent cases even if judges did not care about reversal by the Supreme Court. In the Results section I will show some empirical evidence that suggests that litigants are not driving the patterns we observe; in the Appendix, I present a simple model of litigant behavior that shows why this may be the case (Section C.3).

3.5 Results

3.5.1 Are earlier circuits or later circuits more likely to agree with the Supreme Court?

Figure 3.4 shows the difference in means in agreement with the Supreme Court between the first case and subsequent cases in a circuit split. The first case to emerge in a split agrees

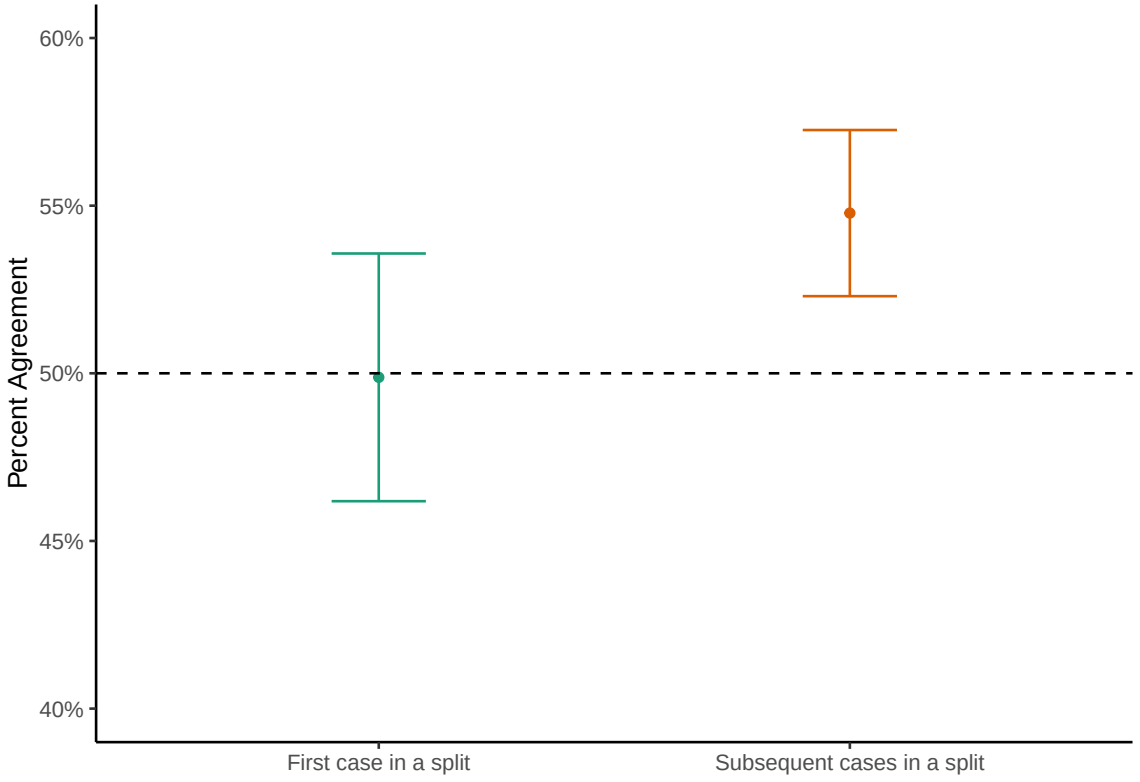


Figure 3.4: Percentage of cases where a circuit decision agrees with the Supreme Court's decision by whether the circuit decision is the first to be decided in a circuit split; standard errors clustered by circuit-decade

with the Supreme Court only 50% of the time, suggesting that the judges are not attempting to conform their behavior to the ultimate Supreme Court resolution of the issue. However, subsequent cases agree with the Supreme Court 55% of the time.

Two main takeaways from Figure 3.4 are that the baseline level of responsiveness to the Supreme Court is low, as previously suggested by the within-decade correlations of Figure 3.2, and that the Supreme Court may be able to increase this responsiveness through its agenda-setting procedure, but even that ability is limited. Judges on the Courts of Appeals may have some level of reversal aversion; however, it is merely one factor among many in their decision-making and it does not produce anything approaching complete agreement with the Supreme Court.

An important limitation of Figure 3.4 is that the number of cases in the "not first" category is endogenous to whether they agree with the Supreme Court. The Supreme Court determines when a circuit conflict ends and it may do so based on lower court agreement with the Supreme Court's position. In particular, if the Supreme Court is still following a reversal strategy in circuit conflict cases, the Court may be more likely to end a circuit split by taking up a case with which it disagrees than by taking up a case with which it agrees.

Indeed, this appears to be happening. Figure 3.5 shows that when one subdivides the cases in a conflict to the first, middle cases, and last,[12] both the first *and the last* cases show low agreement with the Supreme Court. This suggests that the Court is choosing to resolve conflicts by taking cases with which they are more likely to disagree than the typical case in a circuit split. This selection effect suggests that the difference between the first and subsequent cases in Figure 3.4 is understating the real difference between appellate court behavior when they believe that the Supreme Court is watching and when it is not.

A further worry when interpreting the general pattern in either Figure 3.4 or Figure 3.5 is that the types of conflicts the Supreme Court revolves quickly may systematically differ from those it resolves slowly. Therefore the overall pattern may be driven either by the quickly-resolved or slowly-resolved conflicts. We see in Figure 3.6 that the general pattern where the first case and the last case to be decided in a circuit conflict have lower agreement than the middle cases is visible across different conflict lengths (although we have small sample sizes when we subdivide the cases by the total number of circuits to hear an issue). Across various conflict lengths, the first and last cases in the conflict are usually the two to evince the lowest average agreement with the Supreme Court.

[12] This is usually the one for which the Supreme Court grants cert.

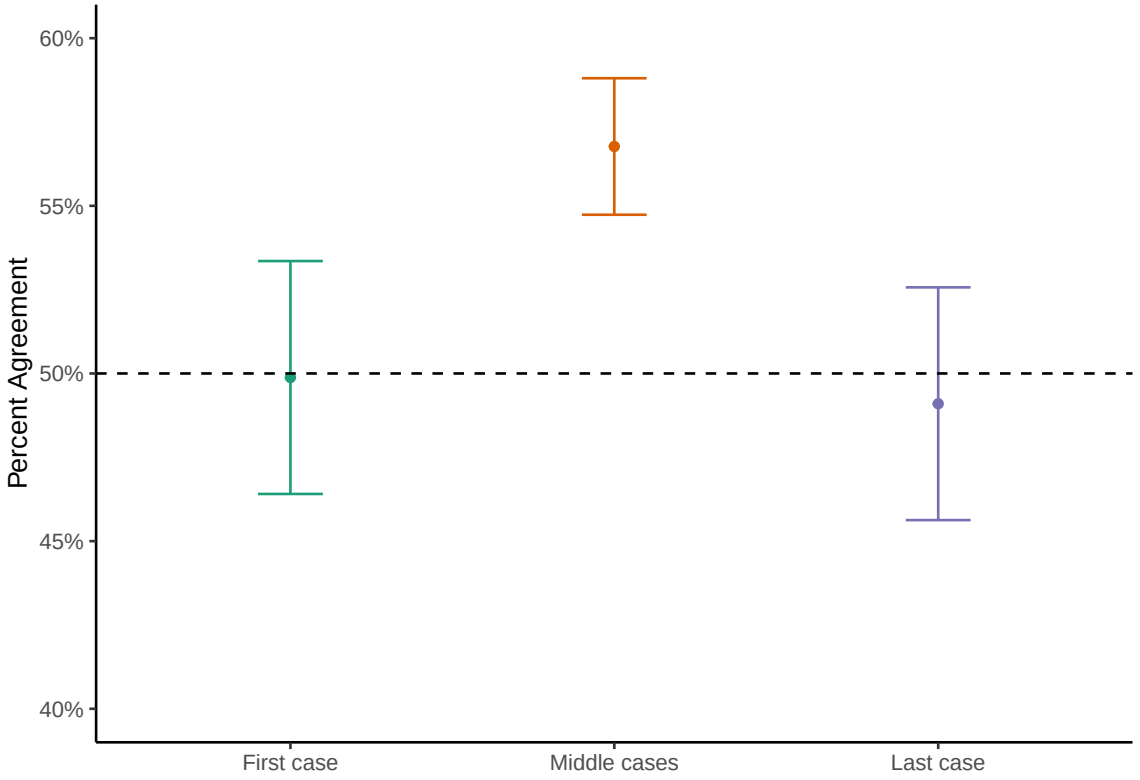


Figure 3.5: Percentage of cases where a circuit decision agrees with the Supreme Court's decision by whether the circuit decision is the first or last to be decided in a circuit split; standard errors clustered by circuit-decade

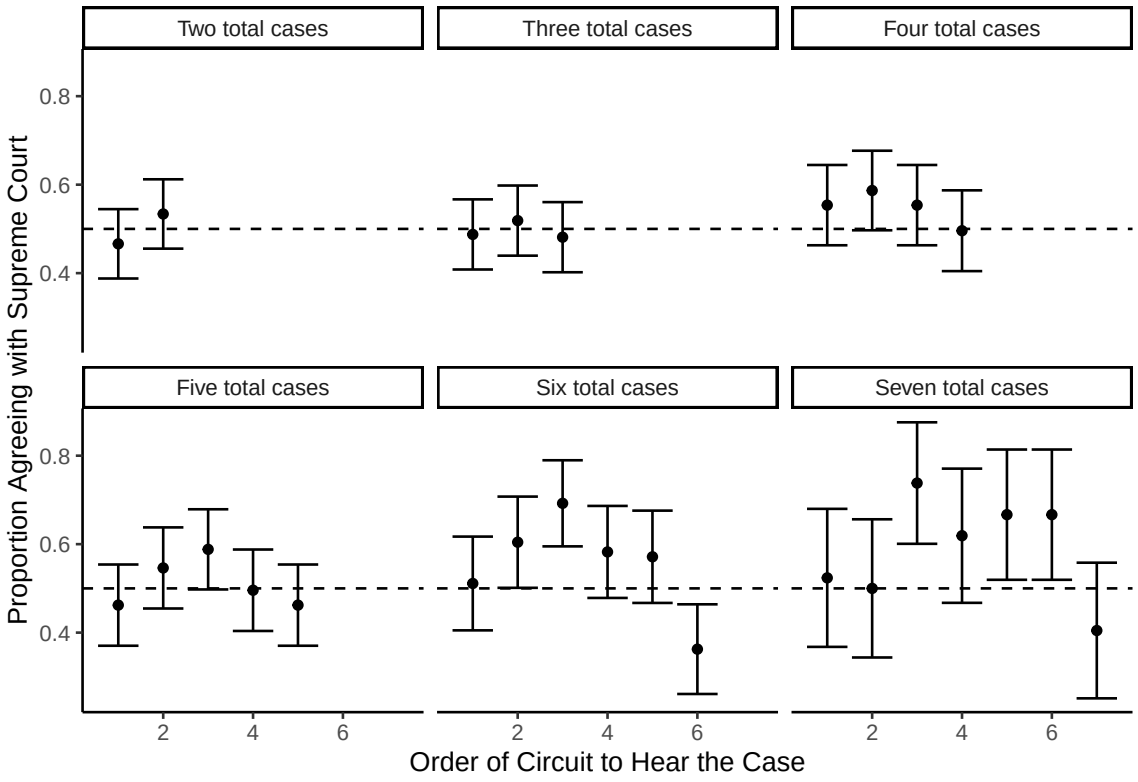


Figure 3.6: Percentage of cases where a circuit decision agrees with the Supreme Court's decision by the order a case was decided within a circuit conflict, subdivided by total number of cases in the conflict

3.5.2 Panel models

Table 3.2 presents linear probability models with some important controls:

- The majority partisanship of the judges hearing the case (addresses shared preferences) (all models)[13]
- Fixed effects by circuit-decade (Models 2-3)
- Fixed effects by legal issue (addresses possible disparities in the strength of the legal arguments on each side of the issue) (Model 3). The model including issue fixed effects drops those splits with fewer than 5 circuits.

Table 3.2: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative)

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.05^{**}	0.05^{**}	0.07^{***}
	(0.02)	(0.02)	(0.02)
Expected agreement	0.10^{***}	0.09^{***}	0.11^{***}
	(0.02)	(0.02)	(0.02)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4032	4032	2742
Adj. R-squared	0.01	0.02	0.08

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

[13] I used data from the Attributes of U.S. Federal Judges Database (Gryski and Zuk 2008; Gryski, Zuk, and Goldman 2008) and from the party of first appointing president when lacking other information on party; dates of appointment available from *History of the Federal Judiciary.* http://www.fjc.gov. Web site of the Federal Judicial Center, Washington, DC.

Table 3.2 shows that the basic pattern in Figure 3.4 is not driven by the first case in a split being more likely to be drawn from an ideologically opposed Court. While the effect of ideology is large, including it in the model has no effect on the value of the main coefficient of interest. Nor is this pattern likely driven by circuit-specific factors such as the development of circuit precedent or time-specific factors such as changes in the Supreme Court median (Models 2-3). Most strikingly, Model 3 shows that when accounting for issue-specific factors, the effect of reversal aversion is at its highest value. This is not merely a result of changing the sample to those issues with 5 or more circuits, either; in the bivariate regression subsetted to those issues, the effect of not being the first circuit to hear an issue is 0.06.

One may be concerned that the partisanship of the median judge on the panel is a poor proxy for ideology. Table 3.3 shows that using the median judge's Judicial Common Space (JCS) score (Boyd 2015a; Epstein et al. 2007) does not affect the results. The measure used in Table 3.3 is simple: the *Expected agreement (JCS)* variable is the median judge's JCS score if the Supreme Court's ultimate decision was conservative and $-1\times$ the median judge's JCS score if the Supreme Court's ultimate decision was liberal. After rounding, the coefficients on the main variable of interest, *Not first case*, are close to identical.

This further demonstrates an important point: there is no evidence that the first panel to hear a case is particularly ideologically extreme relative to the other panels, nor is there evidence that any difference that does exist is meaningful. The difference in means between the median JCS score of the first panel and subsequent panels is 0.05 (s.e. 0.01), which, while statistically significant, is small relative to the standard deviation of the median JCS score in our data (0.28). This also provides further reason to doubt that this pattern is litigant-driven, since it the pattern does not appear to be driven by litigants seeing an outlier ruling by an ideologically extreme panel and filing in a more congenial circuit. As shown in Figure 3.7, there is no general pattern if one breaks down the data into more fine-grained

Table 3.3: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's ideology aligns with the ideological direction of the Supreme Court's ultimate decision, using Judicial Common Space scores

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.04**	0.05**	0.07***
	(0.02)	(0.02)	(0.02)
Expected agreement (JCS)	0.23***	0.22***	0.23***
	(0.03)	(0.03)	(0.04)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4032	4032	2742
Adj. R-squared	0.02	0.03	0.09

***p $<$ .01; **p $<$ .05; *p $<$ .1

orderings (with the first cases in a split being marked '1,' the second as '2,' and so on).[14]

Table 3.4 shows what happens if we subset the cases to those where the second circuit to decide an issue made its decision within three calendar years following the first circuit. This limits the amount of time for strategic behavior to occur. It also limits the possibility that the first circuit to decide the case misperceived the Supreme Court's eventual ruling because it was significantly more temporally distant from the Court that decided the issue than later circuits were. Unfortunately, limiting the data to this subsample eliminates 28% of the cases, making inferences noisier.[15] Nonetheless, we can see in Table 3.4 that the point estimates for models limited to this subsample are very close to the point estimates in Table 3.2.

[14]This figure is limited to those circuit splits with 8 or fewer cases, since we have a small number of observations (fewer than 30) for each group above 8.

[15]An even more stringent specification, limiting to two calendar years, is presented in the Appendix (Table C.4).

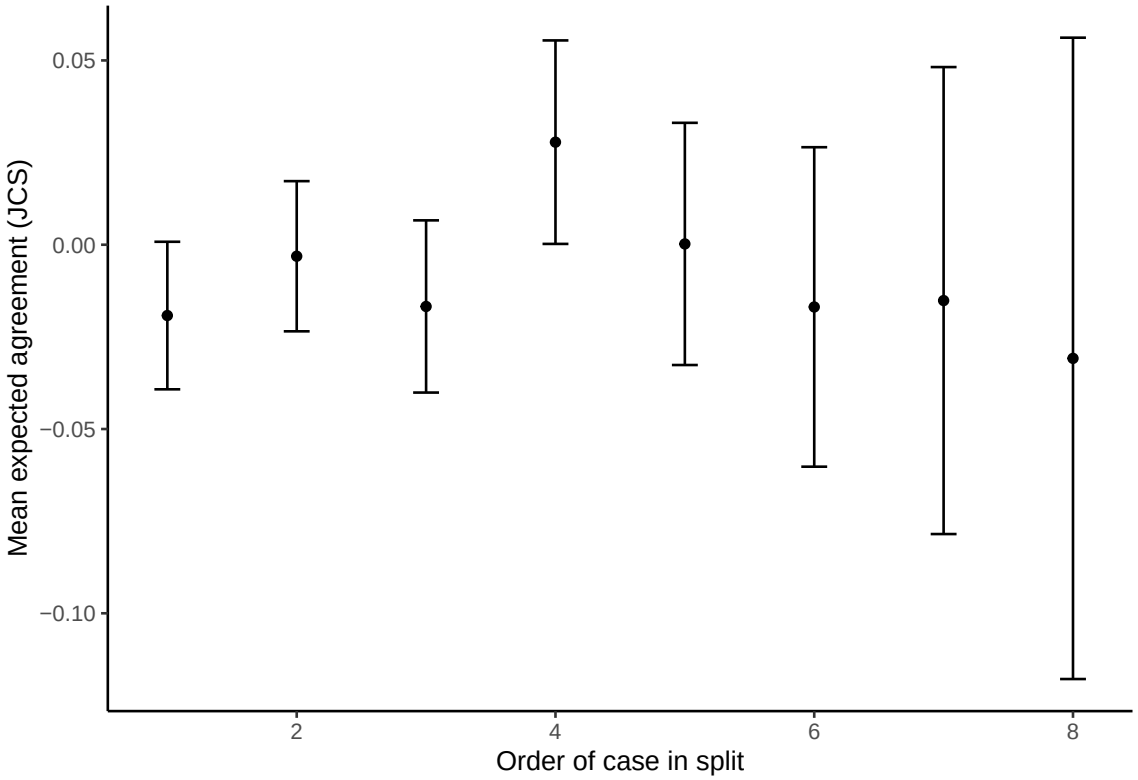


Figure 3.7: Average expected agreement using JCS scores by order of case within a split

Table 3.4: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Limited to cases where the second case was decided three or fewer calendar years after the first.

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.07**	0.07**	0.07**
	(0.03)	(0.03)	(0.03)
Expected agreement	0.09***	0.08***	0.10***
	(0.02)	(0.02)	(0.02)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	2919	2919	2067
Adj. R-squared	0.01	0.02	0.09

***p $<$ .01; **p $<$ .05; *p $<$ .1

3.5.3 Predicting the non-voting circuits

Another threat to inference is if the circuits that do not hear cases on a particular legal issue differ systematically from those who do. To investigate this concern, Table 3.5 separates the votes into four categories: the first circuit to hear an issue, the middle circuits, the last circuit, and the circuits who did not opine on the issue prior to the Supreme Court's involvement. The first column presents the mean agreement with the Supreme Court for those circuits who did vote, as in Figure 3.5. The second column presents the mean *predicted* agreement based on a purely ideological model of voting.[16] Thus, if the nonvoting circuits differ meaningfully in ideology from the other circuits, that should appear as a difference in the mean predicted agreement in the second column.

Table 3.5: Agreement and predicted agreement with the Supreme Court by the order a case emerged in a circuit conflict, including predictions for nonparticipating circuits.

Order case emerged	**Agreement with S.C.**	**Predicted agreement**
First case	0.499	0.499
Middle cases	0.567	0.504
Last case	0.491	0.501
Remaining nonvoting circuits	Did not vote	0.502

What we see, however, is that the predicted agreement based on ideology is nearly identical across the four groups. This suggests that strategic behavior, whether by litigants via forum shopping or by the Supreme Court in deciding when to resolve a split, is not

[16]This was constructed as follows. First, I filtered the dataset to the first cases in splits and ran a logistic regression of agreement with the Supreme Court on expected agreement as measured with JCS scores. The filtering was employed on the assumption that the judges in the first case in a split are least likely to be affected by the Supreme Court's ultimate decision. Then, using the coefficients from that model, I predicted agreement with the Supreme Court for all the cases in the dataset as well as for the circuits who did not vote on an issue. For the circuits that did not vote, I constructed the expected agreement measure by taking all the possible three-judge panels among active judges for a given circuit in January of the final year of the split, calculating the median JCS score for each panel, and taking the mean of all those panel ideology scores, in order to reflect the average ideology across the possible panels for each circuit.

leading to ideological bias in the circuits who hear a particular issue versus those who do not. What's more, we see that the mean predicted agreement based on ideology alone is very similar to the actual agreement for all cases except the middle cases. Indeed, mean predicted agreement is always almost 50%, suggesting that absent the clarifying process of the circuit split, the Courts of Appeals show remarkably little ideological congruence with the Supreme Court despite sharing an appointment mechanism.

3.6 Discussion

When no other circuit has decided an issue, agreement between a circuit court decision and the Supreme Court's decision is no better than chance. However, when the possibility of Supreme Court review increases, agreement increases as well. In the most demanding specification, the difference is 7 percentage points.

This suggests that circuit judges anticipate the Supreme Court's decisions, but only when Supreme Court review appears reasonably likely. The low level of agreement before the second case in a split suggests that mechanisms other than Supreme Court review, such as the shared appointment process, do not produce congruence between circuit courts and the Supreme Court. This has important implications for the study of the American judiciary. Most notably, it means that we cannot look at Supreme Court outcomes as a proxy for what the judiciary as a whole is doing. While the Supreme Court may be able to resolve certain high-profile issues, other courts make most of American law, and if the Supreme Court does not exercise great control over their behavior, that means that those other courts deserve more attention and scrutiny.

Further research can shed additional light on the mechanisms that drive responsiveness in cases with a high review rate, such as circuit splits. For example, does reversal aversion operate only through the possibility of Supreme Court review, or does it also come from

the possibility of *en banc* review from one's own circuit? In addition, leveraging additional data sources, such as the texts of opinions, may help explain whether there is greater responsiveness by circuit courts in behaviors other than votes on the merits.

A Appendix to Chapter 1

A.1 Presidential Success by Circuit

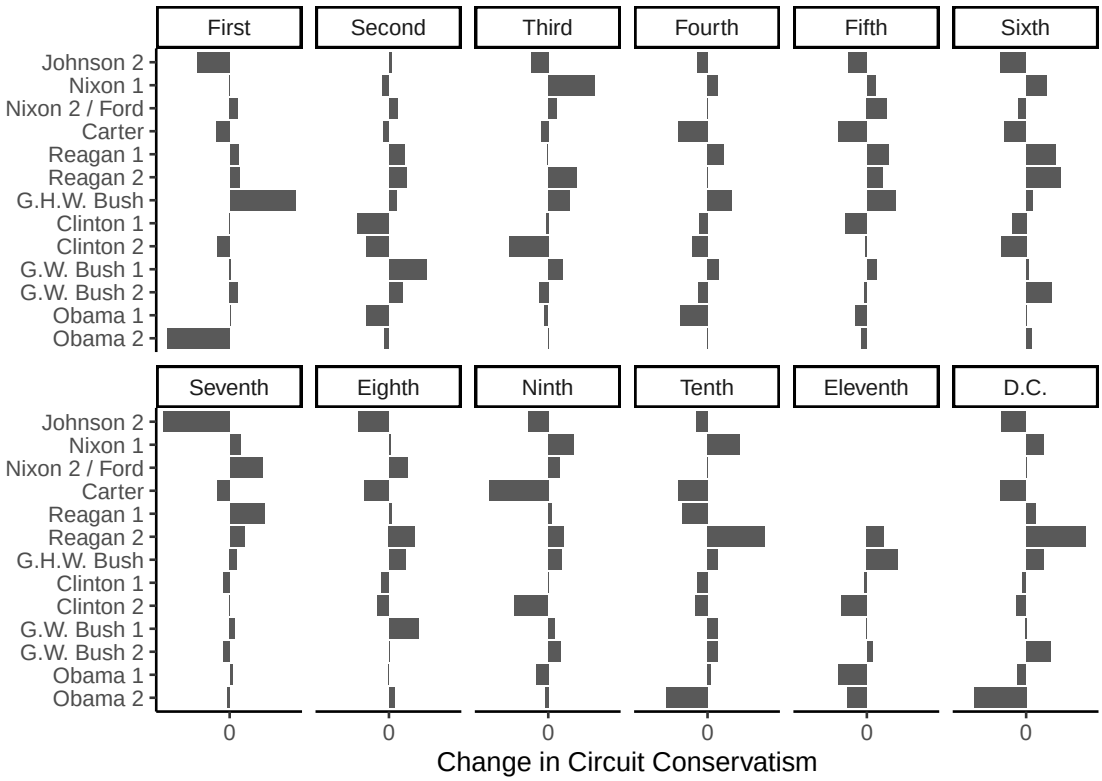


Figure A.1: Change in circuit conservatism by circuit and presidential term

A.2 Circuit Conservatism 1965–2017, All Circuits

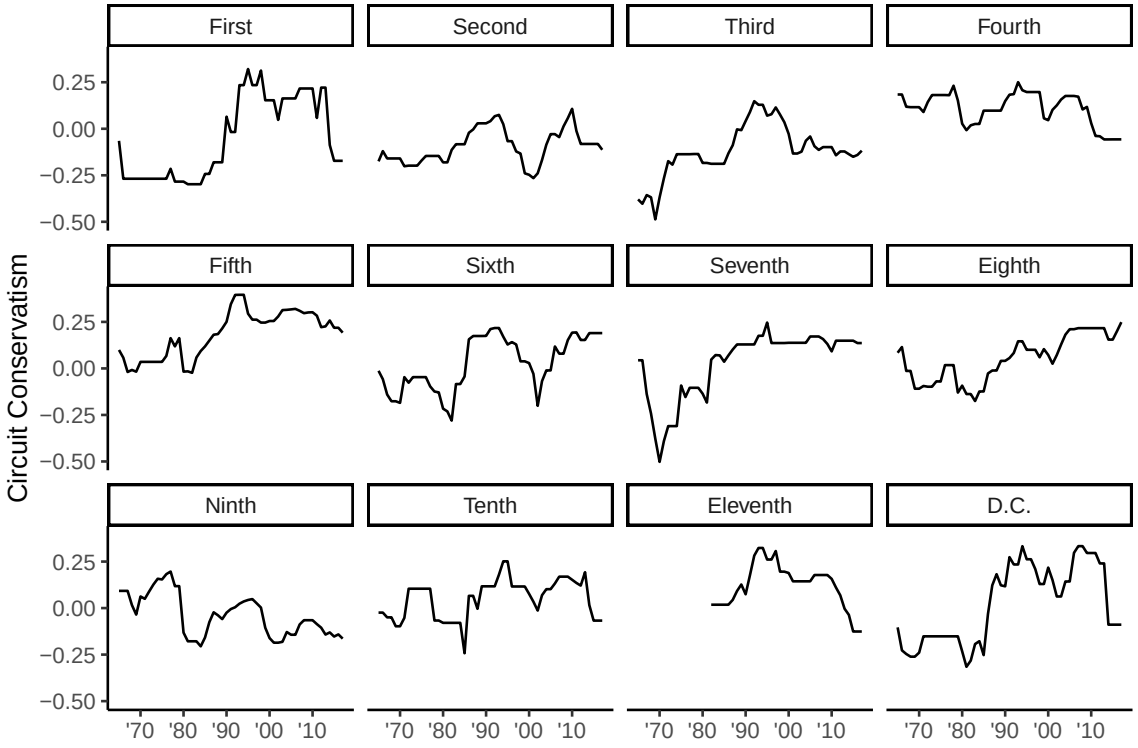


Figure A.2: Conservatism measures for all circuits, 1965–2017

A.3 Conservative Voting Over Time and Across Circuits

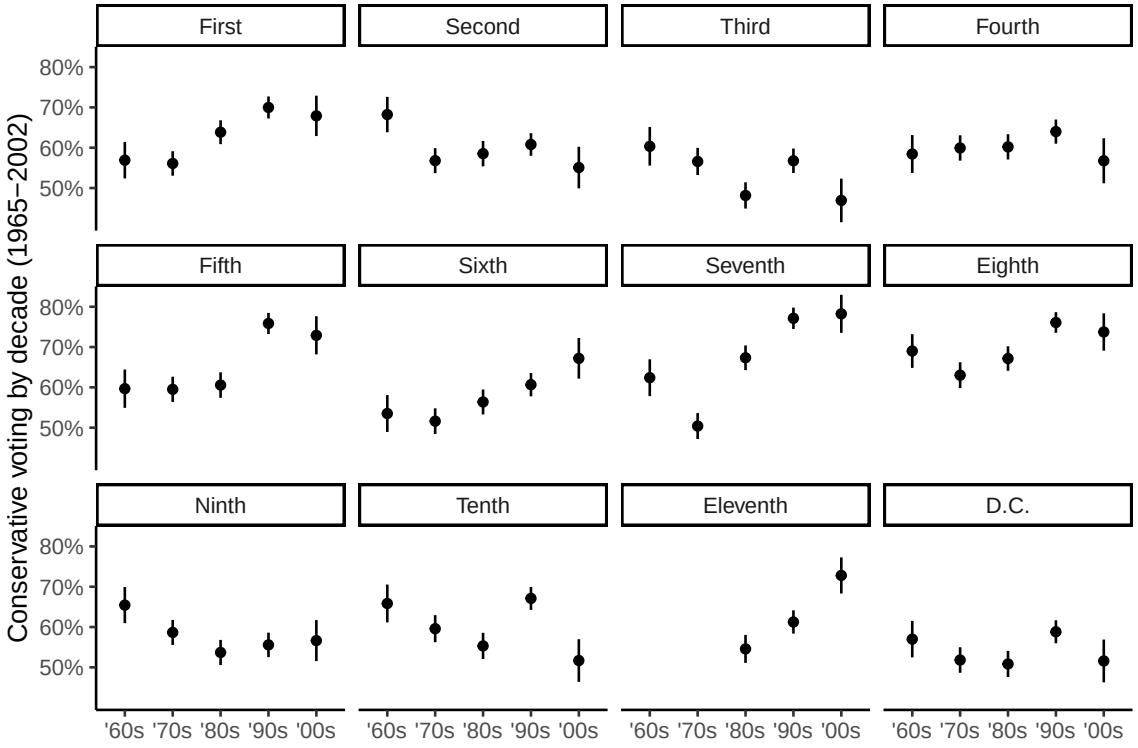


Figure A.3: Conservative voting by circuit-decade

A.4 Conservative Voting and Average Circuit Conservatism

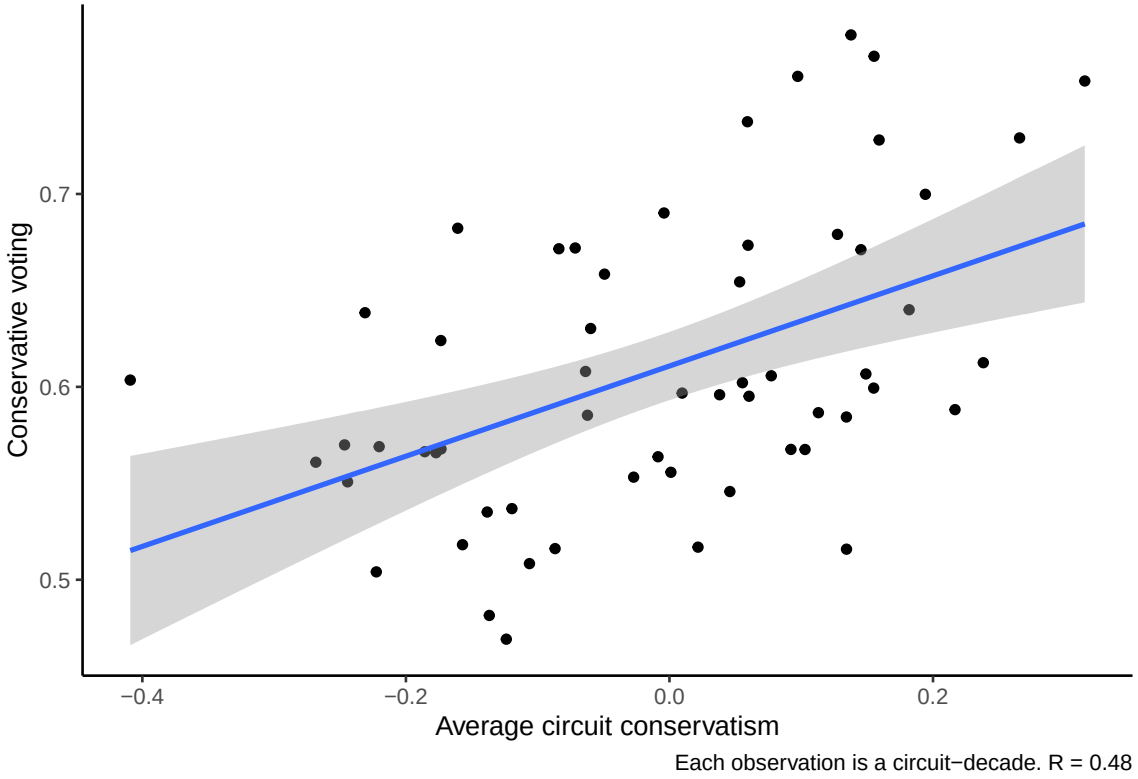


Figure A.4: Correlation between conservative voting and average circuit conservatism by circuit-decade

Table A.1: Predicting conservative voting with circuit conservatism and judge conservatism, weighted averages by circuit-decade. Linear regression coefficients presented.

	Conservative voting
Mean circuit conservatism	0.46**
	(0.18)
Mean judge conservatism	-0.31
	(0.24)
Constant	0.61***
	(0.01)
N	58
Adj. R-squared	0.23

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

A.5 Excluding Votes Classified as "Mixed"

Table A.2: Predicting conservative voting with circuit conservatism and copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database. Votes classified as "mixed" are excluded.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.20***	0.18***	0.16***	0.14**
	(0.04)	(0.04)	(0.05)	(0.06)
Copanelist conservatism	0.16***	0.17***	0.16***	0.19***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Constant	0.62***			
	(0.01)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	37111	37111	37111	37111
Adj. R-squared	0.01	0.04	0.16	0.39

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.6 Ordered Logit Model

Table A.3: Predicting conservative voting with circuit conservatism, copanelist conservatism, and judge conservatism. Ordered logit coefficients presented with standard errors clustered by case.

	Conservative vote
Circuit conservatism	0.52
	(0.12)
Copanelist conservatism	0.57
	(0.11)
Judge conservatism	0.22
	(0.04)
0|0.5	-0.61
	(0.02)
0.5|1	-0.25
	(0.02)
N	40553

A.7 Using the Circuit Median

Table A.4: Predicting conservative voting with the circuit JCS median and copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit JCS median	0.11***	0.10***	0.08**	0.07**
	(0.02)	(0.03)	(0.03)	(0.03)
Copanelist conservatism	0.16***	0.16***	0.16***	0.19***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Constant	0.61***			
	(0.00)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.8 Responsiveness to Circuit Conservatism and Supreme Court Conservatism

Table A.5: Predicting conservative voting with circuit conservatism, copanelist conservatism, and Supreme Court conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.18***	0.18***	0.14***	0.13**
	(0.03)	(0.04)	(0.05)	(0.05)
Copanelist conservatism	0.15***	0.15***	0.15***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Supreme Court conservatism	-0.03	-0.08**	-0.06	-0.15*
	(0.03)	(0.03)	(0.09)	(0.09)
Constant	0.62***			
	(0.01)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Models 1–4 include measures of Supreme Court conservatism in order to take into account the dual hierarchy under which appellate judges operate.[1] However, in these models we do not see evidence that circuit court judges rule more conservatively as the Supreme

[1] I use the median JCS score of Supreme Court justices for each month in the dataset to construct *Supreme Court conservatism* I engage in the same rescaling (0 is the Ninth Circuit's conservatism score in January 2017 and 1 is the Fifth Circuit's conservatism score in January 2017) to facilitate comparisons with the circuit measures.

Court becomes more conservative; if anything, the relationship is negative, although Models 3 and 4 are only picking up within-year changes in Supreme Court conservatism, which is measured by month.[2] It is worth considering why the mechanisms for responsiveness may be less applicable for the Supreme Court. As already noted, collegiality is far less relevant since circuit court judges will not often interact with Supreme Court justices directly. The other mechanisms, while present, are likely weakened in this context. For instance, while changes in the conservatism of the Supreme Court will certainly be reflected in precedent, the limited docket of the Supreme Court means that many issue areas never develop robust Supreme Court precedent and thus those issue areas will be governed by circuit precedent. Concern about maintaining consistency in the law is also blunted because the Supreme Court is not bound by what an appellate court panel does, while judges do have to consider the effect that their decisions will have on future appellate panels from their own circuit (because of the "law of the circuit" doctrine). Finally, the chance of Supreme Court review of most cases is essentially zero.

[2]A simple bivariate regression of conservative voting on Supreme Court conservatism shows a positive relationship, so Table A.5 does not show that the circuit courts are out of step with the Supreme Court, but rather that the Supreme Court has no additional predictive power after taking into account the other variables in the models.

A.9 Responsiveness to Circuit Conservatism and Judge Status

Table A.6: Predicting conservative voting with circuit conservatism and copanelist conservatism, interacted with judge status. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote	
	Model 1	**Model 2**
Circuit conservatism	0.15***	0.15***
	(0.05)	(0.05)
Copanelist conservatism	0.18***	0.18***
	(0.03)	(0.03)
From another court	-0.02	-0.02
	(0.01)	(0.01)
Circuit conservatism × From another court	-0.13**	-0.15**
	(0.06)	(0.06)
Copanelist conservatism × From another court		0.04
		(0.05)
Judge fixed effects	✓	✓
Year-case type fixed effects	✓	✓
N	40553	40553
Adj. R-squared	0.37	0.37

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.10 Responsiveness to Circuit Conservatism and Circuit Size

Table A.7: Predicting conservative voting with circuit conservatism interacted with circuit size, as well as copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.173***	0.148***	0.121**	0.115**
	(0.032)	(0.039)	(0.053)	(0.055)
Circuit size (mean-centered)	-0.003***	-0.001	-0.004*	-0.003*
	(0.001)	(0.002)	(0.002)	(0.002)
Copanelist conservatism	0.154***	0.152***	0.152***	0.183***
	(0.029)	(0.028)	(0.027)	(0.029)
Judge conservatism	0.060***			
	(0.010)			
Circuit conservatism × Circuit size	0.011	0.006	-0.003	0.005
	(0.007)	(0.009)	(0.009)	(0.009)
Constant	0.615***			
	(0.005)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Since the probability of serving with any particular judge on a panel does not increase linearly with circuit size, we can also estimate the probability that a judge will sit on a panel, drawn from active judges on the circuit, with another given judge (Hinkle, Nelson, and Hazelton 2020, 283).[3] Using that variable (*Copanelist probability*) in Table A.8. we

[3]I used the formula $1 - \frac{n-3}{n-1}$, where n is the number of active judges in a circuit.

Table A.8: Predicting conservative voting with circuit conservatism interacted with the probability that one will serve with a judge on a randomly selected panel, as well as copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.20***	0.16***	0.15***	0.13**
	(0.03)	(0.04)	(0.05)	(0.05)
Copanelist probability (mean-centered)	0.11**	0.03	0.12	0.08
	(0.05)	(0.07)	(0.08)	(0.07)
Copanelist conservatism	0.15***	0.15***	0.15***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Circuit conservatism × Copanelist probability	-0.16	-0.11	0.12	0.10
	(0.18)	(0.23)	(0.24)	(0.24)
Constant	0.61***			
	(0.00)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

still do not see a positive and significant interaction effect between copanelist conservatism and the probability of sitting on a panel with any given fellow judge.

A.11 Responsiveness to Circuit Conservatism and *En Banc* Rates

En banc rates were calculated using the complete list of *en banc* rehearings of three-judge panel decisions from 1986 to 2002 in Beim, Hirsch, and Kastellec (2016). These are calculated by circuit, since given the very low rate of *en banc* review, a circuit-year measure is not especially meaningful. To make the measure more interpretable, it is rescaled so 1 is the empirical maximum for *en banc* review; thus, a 0 to 1 shift in the variable *En banc rate* represents going from a circuit that takes up no cases *en banc* to the circuit that did so the most often.

Table A.9: Predicting conservative voting with circuit conservatism interacted with circuit *en banc* rate, as well as copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.23***	0.23***	0.21***	0.22***
	(0.06)	(0.07)	(0.08)	(0.08)
Copanelist conservatism	0.15***	0.15***	0.15***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Circuit conservatism × *En banc* rate	-0.13	-0.19	-0.18	-0.22
	(0.12)	(0.17)	(0.16)	(0.17)
Constant	0.61***			
	(0.01)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	40553	40553	40553	40553
Adj. R-squared	0.01	0.03	0.15	0.37

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.12 Most Common Statutes

Perhaps not surprisingly, Section 1983, which allows for civil suits vindicating federal rights against defendants acting under the color of state law, is the most common. We also see several important civil rights provisions (42 U.S.C. § 2000, with various sections indicated by letters not included in the U.S. Courts of Appeals Database), habeas corpus provisions (28 U.S.C. § 2254-55), labor provisions (29 U.S.C. § 158, 29 U.S.C. § 185), a key provision of the Controlled Substances Act (21 U.S.C. § 841), other important federal criminal law statutes (18 U.S.C. § 371 and 18 U.S.C. § 2 concern conspiracy and aiding and abetting, respectively), and an important provision concerning appellate jurisdiction (28 U.S.C. § 1291).

Table A.10: Top 10 statutes as a proportion of the statutory subset and mean conservative voting across cases considering these statutes

	Statute	Proportion of data	Mean conservative voting
1	Civil rights (42 U.S.C. 1983)	0.07	0.58
2	Civil rights (42 U.S.C. 2000)	0.04	0.56
3	Habeas corpus (22 U.S.C. 2254)	0.03	0.71
4	Labor (29 U.S.C. 158)	0.03	0.37
5	Controlled substances (21 U.S.C. 841)	0.02	0.79
6	Habeas corpus (28 U.S.C. 2255)	0.02	0.80
7	Conspiracy (18 U.S.C. 371)	0.01	0.82
8	Appellate jurisdiction (28 U.S.C. 1291)	0.01	0.59
9	Labor (29 U.S.C. 185)	0.01	0.47
10	Aiding and abetting (18 U.S.C. 2)	0.01	0.86

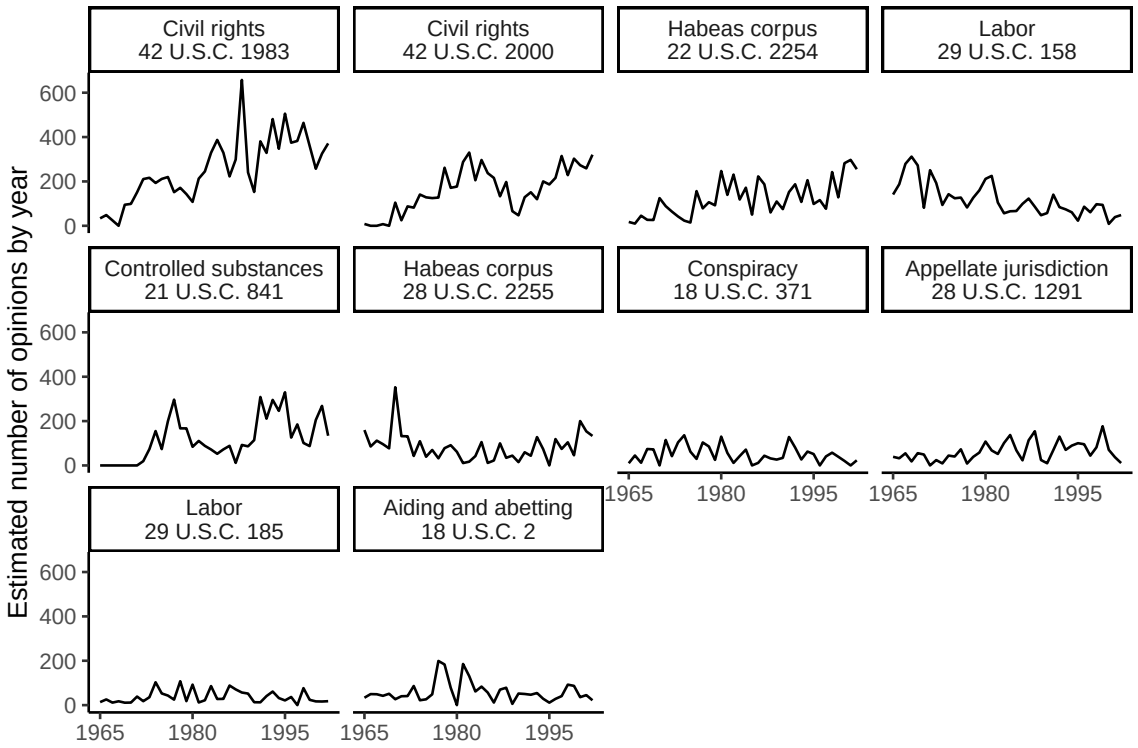


Figure A.5: Changes in issue prevalence over time for the 10 most common statutory provisions

A.13 Issue Prevalence Regressions: Statutory Subset and Sunstein Data

Table A.11: Predicting conservative voting with circuit conservatism, interacted with measures of issue prevalence, and copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. For the statutory subset, the issue fixed effects represent statutory provisions; for the Sunstein data, they represent the 14 issues included in that dataset.

	Conservative vote	
	Statutory subset	**Sunstein dataset**
	Model 1	**Model 2**
Circuit conservatism	−0.28	−0.20
	(0.19)	(0.14)
ln(Issue prevalence)	−0.02	0.001
	(0.01)	(0.01)
Copanelist conservatism	0.14***	0.12***
	(0.03)	(0.02)
Lower court conservative decision	0.22***	0.28***
	(0.02)	(0.02)
Circuit conservatism × ln(Issue prevalence)	0.15**	0.21***
	(0.07)	(0.06)
Judge fixed effects	✓	✓
Year fixed effects	✓	✓
Issue fixed effects	✓	✓
N	27492	13892
Adj. R-squared	0.29	0.21

***p < .01; **p < .05; *p < .1

One difference between these models and Model 3 in Table 1.1 is that the Sunstein data includes a variable for whether the lower court decision was conservative; I recoded that variable as 1 for conservative, 0 for liberal, and 0.5 if not assigned either category. Since this is likely a strong predictor of conservative voting at the appellate level, I include it in the model. However, the U.S. Courts of Appeals Database does not include such a variable.

I created a proxy for the lower court decision by constructing a variable that is assigned 1 if the appellate court decision is conservative and it affirmed the lower court or the appellate court decision is liberal and reversed the lower court,[4] 0 for the opposite, and 0.5 otherwise (for example, when the appellate court decision could not be classified or the treatment of the lower court decision was mixed, as in affirming in part and reversing in part). Because this measure is constructed in part based on the outcome variable, I do not present it in the main results under Table 1.1; however, I include it here for better comparison with the model using the Sunstein data.[5]

A.13.1 The Sunstein Data: Further Discussion

The Sunstein data, which covers 1995–2008, uses a different data collection strategy from that of the U.S. Courts of Appeals Database. Rather than collect a random sample of published appellate decisions, it attempts to collect *all* appellate court decisions on 14 issues for the time period under consideration.[6] This has some advantages for the present study. Most notably, it means that the measure is more precise, since this is not a sample of opinions but rather the whole universe of opinions on a given issue. In addition, it means that each case is not limited in terms of how many issues it can be coded as covering, since the data collection and outcome coding proceeded issue-by-issue; thus, a case can be coded as covering age discrimination, disability discrimination, and sex discrimination, for

[4]Under "reversed" I include the following codes indicating negative treatment: "reversed," "reversed and remanded," "vacated and remanded," and "vacated."

[5]Another difference between the two datasets is that the Sunstein data excludes district judges sitting by designation on appellate panels.

[6]The issues covered are abortion, the Americans with Disabilities Act, affirmative action, age discrimination, campaign finance, capital punishment, the Contract Clause, the EPA, federalism, piercing the corporate veil, sex discrimination, sexual harassment, takings, and Title VII of the Civil Rights Act of 1964.

example, with outcome codings for each issue.[7]

Figure A.6 shows how the effect of circuit conservatism is conditioned by issue prevalence in the Sunstein data. Much like with the statutorily-based measure of issue prevalence shown in Figure 1.3, we see that as an issue grows in prevalence, the effect of circuit conservatism increases. These analyses suggest that the responsiveness to one's own circuit that we saw in Table 1.1 is primarily a function of behavior in more well-developed issues.

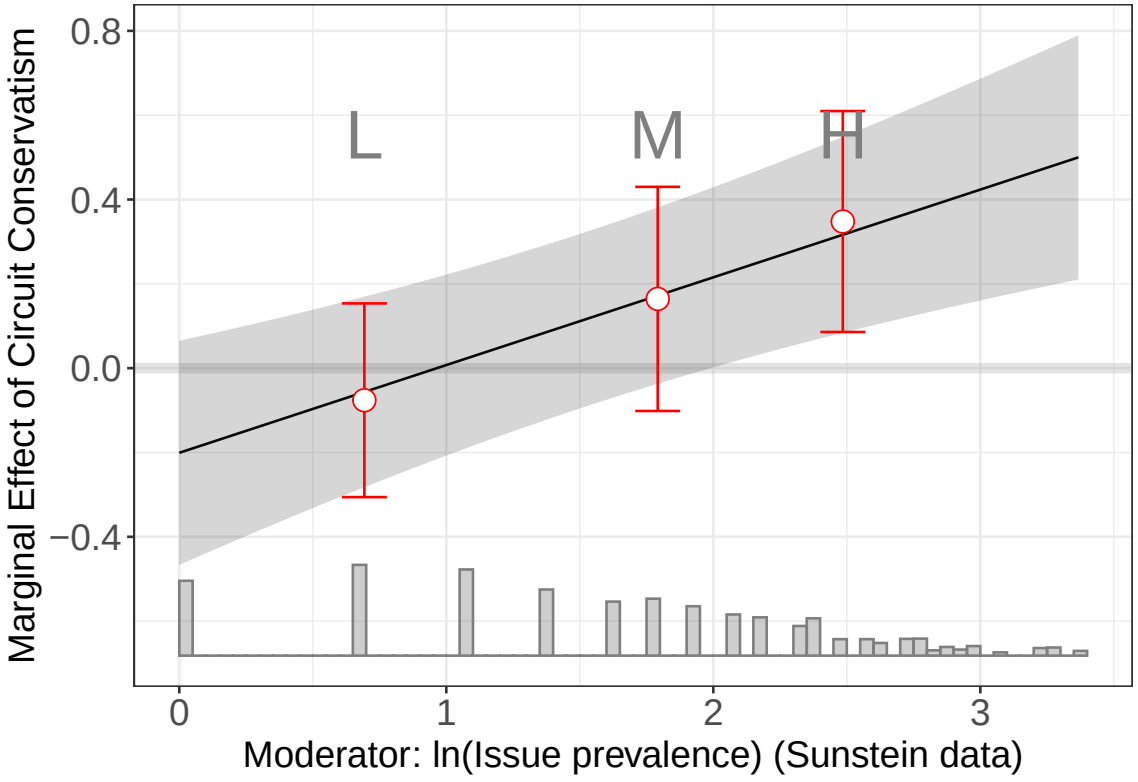


Figure A.6: Predicting conservative voting with circuit conservatism, interacted with a log transformation of the measure of issue prevalence constructed with the Sunstein data, using the binning estimator of Hainmueller, Mummolo, and Xu (2019), and copanelist conservatism. Standard errors clustered by case.

[7]One downside of this data source is that it lacks a variable for the month the case was decided. To obtain this, I used data from CourtListener (https://www.courtlistener.com/api/bulk-data/clusters/) to match the citation to the date filed; when this failed to produce a match, or led to a "year" variable that differed by more than 1 from the "dec_year" variable in the dataset, I manually entered the date variable.

A.14 Issue Prevalence: Unlogged Variable

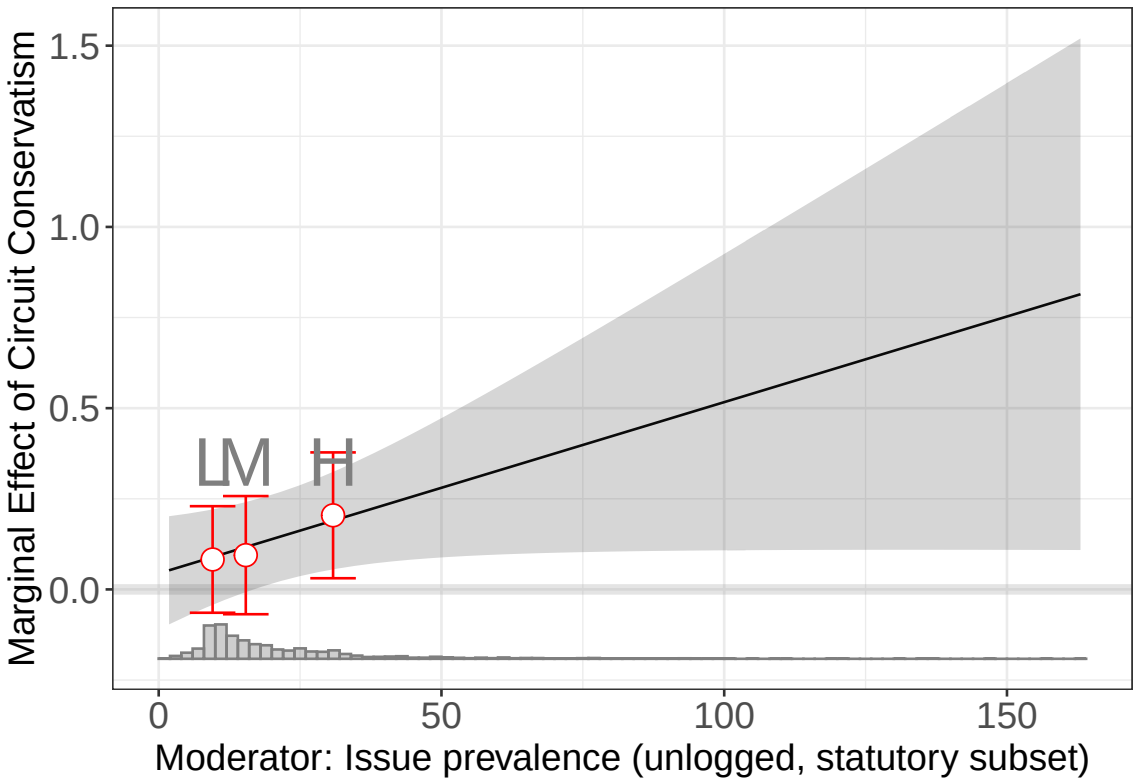


Figure A.7: Predicting conservative voting with circuit conservatism, interacted with the statutory subset measure of issue prevalence (unlogged), using the binning estimator of Hainmueller, Mummolo, and Xu (2019), and copanelist conservatism. Standard errors clustered by case.

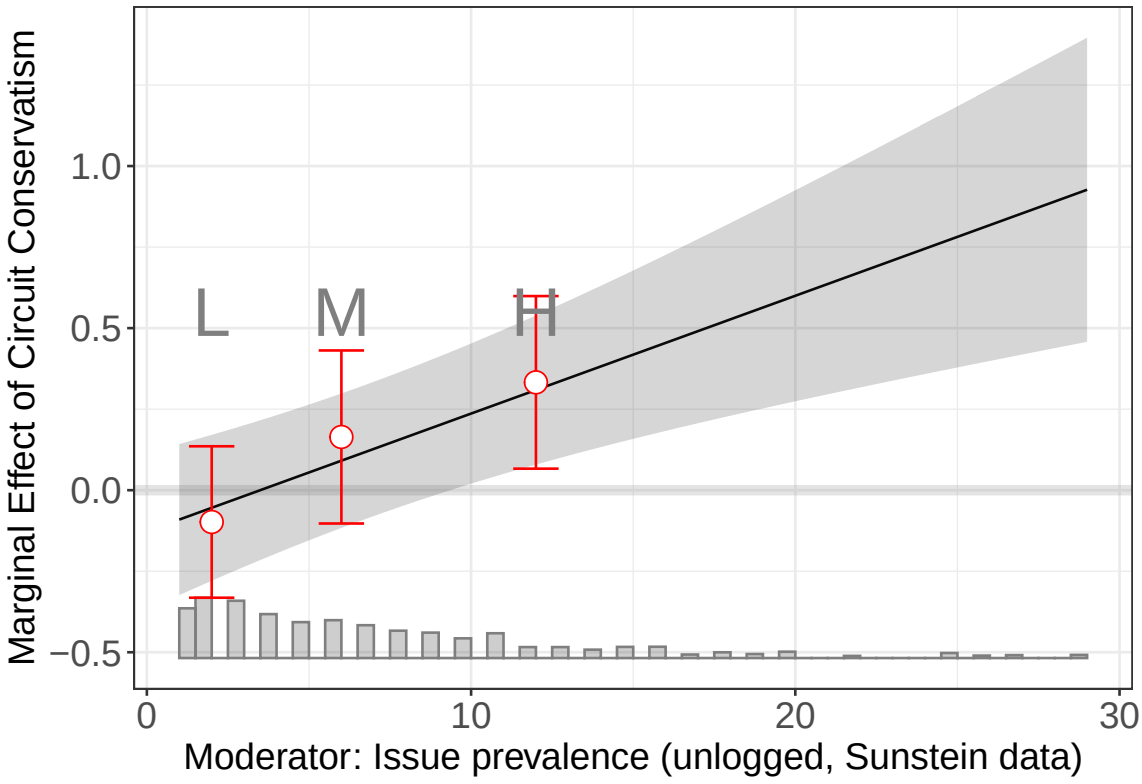


Figure A.8: Predicting conservative voting with circuit conservatism, interacted with the measure of issue prevalence constructed with the Sunstein data (unlogged), and copanelist conservatism. Standard errors clustered by case.

A.15 Circuit Conservatism in *En Banc* Cases

Table A.12: Predicting conservative voting in *en banc* cases (1986–2004) with circuit conservatism. Linear regression coefficients presented with standard errors clustered by case.

	Conservative vote	
	Model 1	**Model 2**
Circuit conservatism	−0.03	0.02
	(0.07)	(0.12)
Judge conservatism	0.53***	
	(0.02)	
Constant	0.52***	
	(0.01)	
Judge fixed effects		✓
N	9454	9454
Adj. R-squared	0.12	0.21

***p < .01; **p < .05; *p < .1

This dataset combines data from three sources:

1. The *en banc* cases collected as part of the resolved circuit split data from 1986 to 2004, described in the main text.

2. The list of *en banc* cases collected by Beim, Hirsch, and Kastellec (2016), which notes all three-judge panel decisions from 1986 to 2002 that were reheard *en banc*; those *en banc* cases span from 1986 to 2004. Using the case citations from Beim, Hirsch, and Kastellec (2016), I collected the individual judges' votes. Where there were multiple issues, I noted the votes for each issue.

3. The *en banc* cases from 1986 to 2002 in the U.S. Courts of Appeals Database (Songer 2008; Kuersten and Haire 2011) that were not in either of the above.

To test directly whether voting in *en banc* cases differs from panel cases, Table A.13 combines *en banc* cases with panel decisions from the U.S. Courts of Appeals Database during this time period; this allows for better estimates of the judge fixed effects. We see that the effect of circuit conservatism disappears in the *en banc* cases, but the effect of judge conservatism considerably increases. Furthermore, while we see no effect of Supreme Court conservatism in panel voting, it is considerably larger in *en banc* cases.

Table A.13: Predicting conservative voting in *en banc* cases (1986-2004) and panel cases (1986-2002) with circuit conservatism and Supreme Court conservatism. Linear regression coefficients presented with standard errors clustered by case.

	Conservative vote
Circuit conservatism	0.22^{***}
	(0.06)
Supreme Court conservatism	-0.04
	(0.10)
En banc	-0.19^{***}
	(0.06)
Circuit conservatism $\times$ *En banc*	-0.29^{***}
	(0.10)
Judge conservatism $\times$ *En banc*	0.46^{***}
	(0.02)
Supreme Court conservatism $\times$ *En banc*	0.28^{*}
	(0.17)
Judge fixed effects	✓
N	29299
Adj. R-squared	0.08

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

A.16 Responsiveness with Lagged Independent Variable

Table A.14: Predicting conservative voting with circuit conservatism (12 months prior) and copanelist conservatism. Linear regression coefficients presented with standard errors clustered by case. Votes are weighted to reflect the sampling procedure of the U.S. Courts of Appeals Database.

	Conservative vote			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Circuit conservatism	0.17***	0.15***	0.10**	0.14***
	(0.03)	(0.04)	(0.05)	(0.05)
Copanelist conservatism	0.15***	0.15***	0.15***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Judge conservatism	0.06***			
	(0.01)			
Constant	0.61***			
	(0.005)			
Judge fixed effects		✓	✓	✓
Year fixed effects			✓	
Case type fixed effects			✓	
Year-case type fixed effects				✓
N	39521	39521	39521	39521
Adj. R-squared	0.01	0.03	0.15	0.37

***p $<$ .01; **p $<$ .05; *p $<$ .1

A.17 Data Collection: Resolved Circuit Splits

The Supreme Court Database does not record the circuit court votes in the circuit splits which the Supreme Court resolves; those I have collected (except for the 1985-1995 Terms, which were collected by Lindquist and Klein (2006)). The process is as follows.[8] I noted all the cases which the Supreme Court Database (Spaeth et al. 2020) codes as granted either for a "federal court conflict" or "federal court conflict and to resolve significant or important question." Next, I read the Supreme Court opinion to identify the circuit cases involved in the split and then reviewed those cases to code which side of the split each judge in the case supported. It is more complicated than simply recording which judges dissented or joined the majority, since concurrences sometimes express disagreement with the issue in question. Sometimes, dissenters even agree with the majority on the issue for which there is a split but are dissenting for other reasons. For each vote, I note the name of the judge (coded to match the Judicial Common Space data (Epstein et al. 2007); district court judges' names are coded to match the District Courts Attributes Data (Gryski, Zuk, and Goldman 2008) where available). I also note whether the vote was conservative or liberal, using the codings from the Supreme Court Database. In the rare cases where the issue on which the circuits were split appeared to have the opposite ideological valence from the Supreme Court's overall decision, I reversed the coding.

[8] As these data are used more thoroughly in Chapter 3, I also provide a description there. In this chapter, I limit the data to those splits that began 1965 or later, to match the main dataset.

Courts of Appeals have divided on the statutory interpretation question this case presents. Compare *ibid.* and *Gillett-Netting* v. *Barnhart*, 371 F. 3d 593, 596–597 (CA9 2004) (biological but posthumously conceived child of insured wage earner and his widow qualifies for benefits), with *Beeler* v. *Astrue*, 651 F. 3d 954, 960–964 (CA8 2011), and *Schafer* v. *Astrue*, 641 F. 3d 49, 54–63 (CA4 2011) (posthumously conceived child's qualification for benefits depends on intestacy law of State in which wage earner was domiciled). To resolve the conflict, we granted the Commissioner's petition for a writ of certiorari. 565 U. S. ___ (2011).

Figure A.9: Example description of a circuit split in a Supreme Court opinion (*Astrue v. Capato*)

Table A.15: Development of a circuit split in *Astrue v. Capato*, 566 U.S. 541 (2012), a Social Security case where the circuits split 2-2 and the Supreme Court took the conservative position

Date	Liberal	Conservative	SCOTUS?
2004-06-09	9th		✗
2011-01-04	3rd		✗
2011-04-12		4th	✓
2011-08-29		8th	✓

A.18 Descriptive Statistics: Resolved Circuit Splits

Collecting the resolved circuit splits from the 1985–2016 Terms generated over 6,000 votes for judges sitting on panels in splits with at least 5 circuits. However, in order to study panel responsiveness to circuit composition, we need more than just a large sample size overall; we also need decent sample sizes both across time and across circuits. Figure A.10 shows how the votes in the resolved circuit splits considered in Model 1 of Table 1.2 are distributed across years and the geographic circuits.[9]

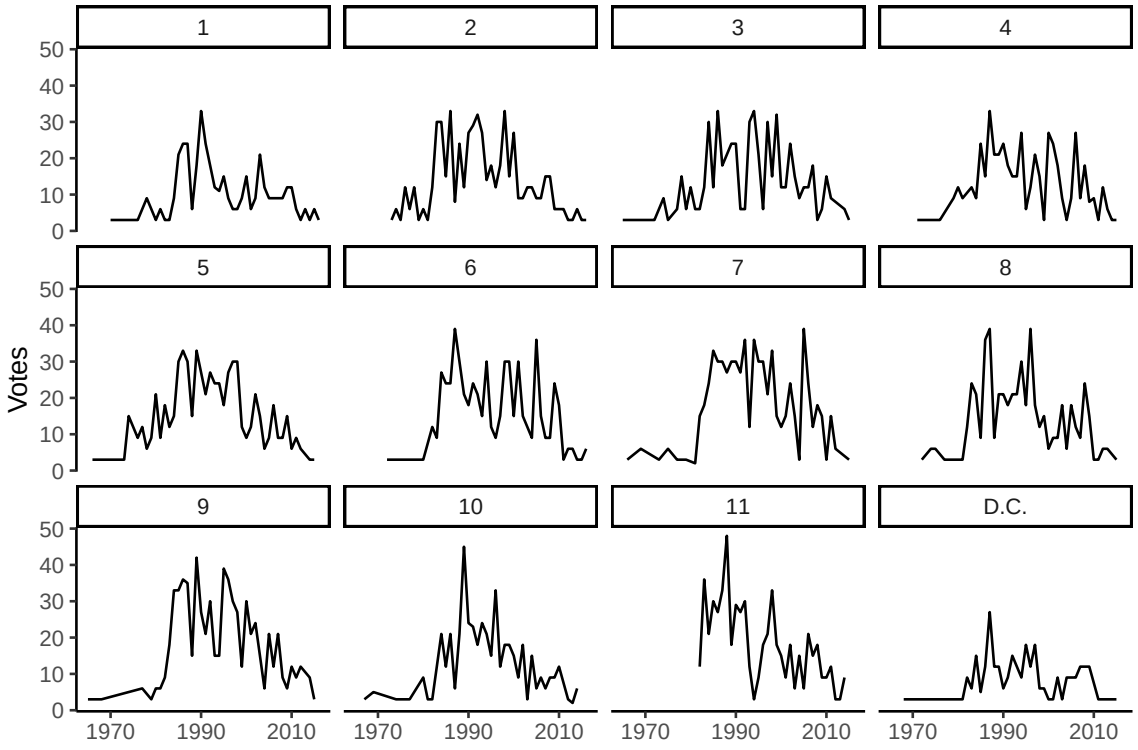


Figure A.10: Number of votes per circuit-year for circuit splits resolved in the 1985–2016 Terms

As seen in Figure A.10, we have a good number of votes for most circuits other than

[9]The Federal Circuit is excluded.

for the D.C. Circuit, which participates in few resolved circuit splits in this period; this is probably a reflection both of its small caseload[10] and its partial specialization in administrative law topics. Not surprisingly, the circuit for which we have the most observations is the Ninth, which both hears the most cases[11] and has a reputation as a liberal outlier.

In Figure A.10, we see that there is a spike in the number of cases heard around 1985, which is the first Term for which we have Supreme Court cases. There is a decline thereafter, primarily because while most circuit cases are close in time to the Supreme Court case that resolves the split, sometimes a circuit split takes time to percolate before arriving at the Supreme Court (for a theoretical explanation of this phenomenon, see Beim, Hirsch, and Kastellec (2016)). This means we have a non-trivial number of cases from before 1985 as well. Nevertheless, there are clearly many years and circuits (and therefore judges) for which we have few observations; by joining the resolved circuit splits data with the U.S. Courts of Appeals Database, we can make use of the unique features of the resolved circuit splits while also getting better estimates of the judge and year effects.

[10] As noted in https://www.uscourts.gov/statistics/table/b/federal-judicial-caseload-statistics/2020/03/31, it has the smallest caseload of the geographic circuits.

[11] For the year ending March 31, 2020, see https://www.uscourts.gov/statistics/table/b/federal-judicial-caseload-statistics/2020/03/31.

B | Appendix to Chapter 2

B.1 Full Results

B.1.1 Heterogeneity by Rates of Review and Reversal

Table B.1: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with a measure of the rate of appellate review for the previous calendar year. The data are limited to years after 1970. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.12	0.02	−0.02	−0.01	−0.01
	(0.08)	(0.07)	(0.05)	(0.04)	(0.05)
Rate of appellate review	−0.16	−0.48***	−0.33**	−0.22*	−0.22*
	(0.15)	(0.16)	(0.14)	(0.13)	(0.12)
Mean liberalism × Rate of review	−0.01	0.47	0.86**	0.62**	0.56*
	(0.82)	(0.55)	(0.41)	(0.30)	(0.31)
Constant	0.40***				
	(0.03)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	91587	91587	91587	91587	91587
Adj. R-squared	0.002	0.06	0.06	0.12	0.14

***p < .01; **p < .05; *p < .1

Table B.2: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with a measure of the rate of appellate review for the previous calendar year and with an indicator for the judge's party. The data are limited to years after 1970. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.03	0.01	−0.02	−0.03	−0.02
	(0.09)	(0.07)	(0.05)	(0.04)	(0.05)
Rate of appellate review	−0.38*	−0.35**	−0.21	−0.13	−0.14
	(0.20)	(0.18)	(0.16)	(0.15)	(0.14)
Judge is a Democrat	0.10***				
	(0.02)				
Mean liberalism × Rate of review	0.45	0.50	0.85**	0.71**	0.62*
	(0.85)	(0.55)	(0.43)	(0.32)	(0.34)
Judge is a democrat × Rate of review	0.03	−0.34**	−0.33**	−0.34**	−0.28*
	(0.14)	(0.15)	(0.16)	(0.16)	(0.16)
Constant	0.39***				
	(0.04)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	81923	81923	81923	81923	81923
Adj. R-squared	0.01	0.06	0.06	0.12	0.14

***p $<$ .01; **p $<$ .05; *p $<$ .1

Table B.3: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with a measure of the rate of appellate reversal for the previous calendar year. The data are limited to years after 1970. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.04	0.05	0.06*	0.05*	0.05
	(0.07)	(0.05)	(0.03)	(0.03)	(0.04)
Rate of appellate reversal	−0.75	−0.81*	−0.72***	−0.39	−0.30
	(0.53)	(0.49)	(0.27)	(0.41)	(0.42)
Mean liberalism × Rate of reversal	7.43**	4.22**	2.56***	1.69**	1.29
	(3.44)	(2.11)	(0.84)	(0.66)	(0.79)
Constant	0.38***				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	91537	91537	91537	91537	91537
Adj. R-squared	0.003	0.06	0.06	0.12	0.14

***p $<$ .01; **p $<$ .05; *p $<$ .1

Table B.4: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with a measure of the rate of appellate reversal for the previous calendar year and with an indicator for the judge's party. The data are limited to years after 1970. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.01	0.05	0.07**	0.06*	0.06
	(0.07)	(0.05)	(0.03)	(0.03)	(0.03)
Rate of appellate reversal	−1.97**	−0.61	−0.70**	−0.50	−0.34
	(0.82)	(0.45)	(0.29)	(0.34)	(0.34)
Judge is a Democrat	0.09***				
	(0.02)				
Mean liberalism × Rate of reversal	7.86**	3.45*	1.85*	1.27*	0.80
	(3.80)	(2.00)	(0.96)	(0.66)	(0.89)
Judge is a Democrat × Rate of reversal	1.31	−0.20	0.04	0.17	0.16
	(0.94)	(0.62)	(0.61)	(0.58)	(0.56)
Constant	0.36***				
	(0.03)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	81903	81903	81903	81903	81903
Adj. R-squared	0.01	0.06	0.06	0.12	0.14

***p $<$ .01; **p $<$ .05; *p $<$ .1

B.1.2 Heterogeneity by Type of Case

Table B.5: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted by category of case. The omitted category is criminal cases. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	−0.05	−0.07	−0.08**	−0.04	−0.01
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Civil rights/civil liberties	−0.01	0.01	0.01		
	(0.02)	(0.02)	(0.02)		
Economics	0.17***	0.18***	0.18***		
	(0.03)	(0.03)	(0.03)		
Mean liberalism × CR/CL	0.27***	0.23***	0.23***	0.18***	0.13***
	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
Mean liberalism × Economics	0.19***	0.16***	0.16***	0.07	0.02
	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Constant	0.32***				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	99188	99188	99188	99188	99188
Adj. R-squared	0.04	0.10	0.10	0.13	0.14

***p $<$ .01; **p $<$ .05; *p $<$ .1

B.1.3 Progressive Ambition

Table B.6: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with time on bench. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.11^{**}	0.06	0.07^{**}	0.05^{**}	0.05^{*}
	(0.05)	(0.04)	(0.03)	(0.02)	(0.03)
Time on bench (mean-centered)	0.002	-0.003^{**}			
	(0.001)	(0.001)			
Mean liberalism × Time on bench	-0.01^{*}	0.005	0.004	0.003	0.003
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Constant	0.38^{***}				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	99188	99188	99188	99188	99188
Adj. R-squared	0.002	0.06	0.07	0.13	0.14

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table B.7: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with co-partisanship with president. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.15^{**}	0.09^{**}	0.07^{**}	0.06^{**}	0.06^{**}
	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)
Same party as president	0.004	0.0001	-0.004	-0.004	-0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mean liberalism $\times$ Same party	-0.08^{**}	-0.02	-0.01	-0.01	-0.01
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	0.38^{***}				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	99188	99188	99188	99188	99188
Adj. R-squared	0.003	0.06	0.07	0.13	0.14

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

B.1.4 Workload

Table B.8: Predicting liberal district court voting with the mean liberalism of appellate panels, interacted with a measure of district court judges' workloads. Coefficients are from linear fixed effect models with standard errors clustered by circuit.

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.24	0.31	0.16	0.05	0.05
	(0.31)	(0.26)	(0.29)	(0.20)	(0.22)
ln(Workload)	0.01	0.02*	0.01	0.002	0.004
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Mean liberalism × Workload	−0.02	−0.04	−0.01	0.004	0.003
	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
Constant	0.31***				
	(0.09)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	95681	95681	95681	95681	95681
Adj. R-squared	0.002	0.06	0.07	0.13	0.14

***p $<$ .01; **p $<$ .05; *p $<$ .1

B.2 Robustness Checks

B.2.1 Alternative Methods of Inference

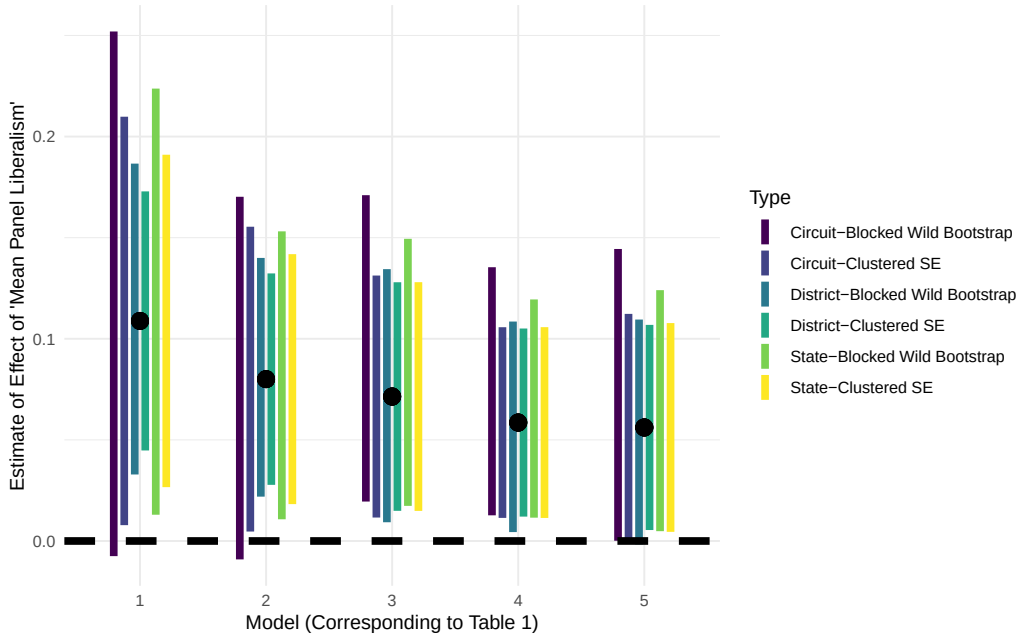


Figure B.1: Alternative methods of inference for main results. Clustered standard errors are estimated using felm in r; wild block bootstrap estimated using boottest in Stata.

B.2.2 Logit Results

Table B.9: Predicting liberal district court voting with the mean liberalism of appellate panels, coefficients from logit models (standard errors clustered by circuit)

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.26^{**}	0.18^{**}	0.16^{**}	0.13^{**}	0.12^{*}
	(0.12)	(0.08)	(0.07)	(0.05)	(0.06)
Constant	-0.96^{***}				
	(0.04)				
Judge indicators		✓	✓	✓	
Judge-category indicators					✓
Year indicators			✓	✓	✓
Case type indicators				✓	✓
N	99188	98865	98865	98865	97355
Pseudo Adj. R^2	0.001	0.004	0.004	0.025	0.002

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

B.2.3 Alternative Levels of Aggregation

Table B.10: Predicting liberal district court voting with the mean liberalism of appellate panels; coefficients from linear fixed effect models at different levels of aggregation (standard errors clustered by circuit)

	Judge-Year Level	**District-Year Level**	**Circuit-Year Level**
	Model 1	**Model 2**	**Model 3**
Mean liberalism of circuit panels	0.08*	0.17***	0.15***
	(0.04)	(0.05)	(0.03)
Judge fixed effects	✓		
District fixed effects		✓	
Circuit fixed effects			✓
Year fixed effects	✓	✓	✓
N	22788	4449	561
Adj. R-squared	0.12	0.10	0.38

***p < .01; **p < .05; *p < .1

B.2.4 Alternative Specifications

Table B.11: Predicting liberal district court voting with the mean liberalism of appellate panels; coefficients from linear models with alternative methods of controlling for unobservables (standard errors clustered by circuit)

	Judge-Decision	**Circuit-Year**		
	Model 1	**Model 2**	**Model 3**	**Model 4**
Mean liberalism of circuit panels	0.04^{**}	0.10^{*}	0.06^{*}	0.11^{***}
	(0.02)	(0.05)	(0.03)	(0.02)
Lagged DV			0.53^{***}	0.36^{***}
			(0.05)	(0.06)
Judge fixed effects	✓			
Circuit fixed effects		✓		✓
Circuit linear trends	✓	✓		
Year fixed effects	✓	✓	✓	✓
N	99188	561	548	548
Adj. R-squared	0.13	0.43	0.43	0.48

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

B.2.5 Alternative Measures of Circuit Court Ideology

Table B.12: Predicting liberal district court voting with alternative measures of circuit court liberalism; coefficients from linear fixed effect models (standard errors clustered by circuit)

	Liberal Vote	
	Model 1	**Model 2**
Median liberalism of circuit judges	0.01	
	(0.02)	
Mean liberalism of circuit judges		0.07***
		(0.03)
Judge fixed effects	✓	✓
Year fixed effects	✓	✓
Case type fixed effects	✓	✓
N	99188	99188
Adj. R-squared	0.13	0.13

***p < .01; **p < .05; *p < .1

Table B.13: Predicting liberal district court voting with the mean liberalism of appellate panels (senior judges included); coefficients from linear fixed effect models (standard errors clustered by circuit)

	Liberal Vote				
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Mean liberalism of panels	0.10*	0.14**	0.14**	0.10*	0.11**
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
Constant	0.38***				
	(0.02)				
Judge fixed effects		✓	✓	✓	
Judge-category fixed effects					✓
Year fixed effects			✓	✓	✓
Case type fixed effects				✓	✓
N	99188	99188	99188	99188	99188
Adj. R-squared	0.001	0.06	0.07	0.13	0.14

***p < .01; **p < .05; *p < .1

B.2.6 Omitting Circuits, Decades, and Issue Areas

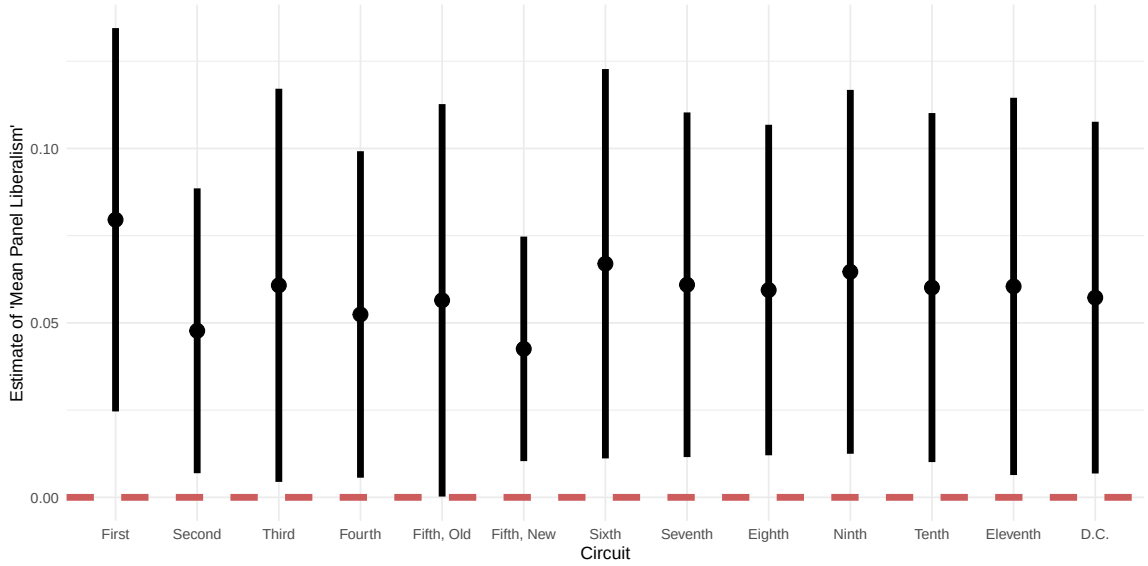


Figure B.2: Iteratively dropping circuits

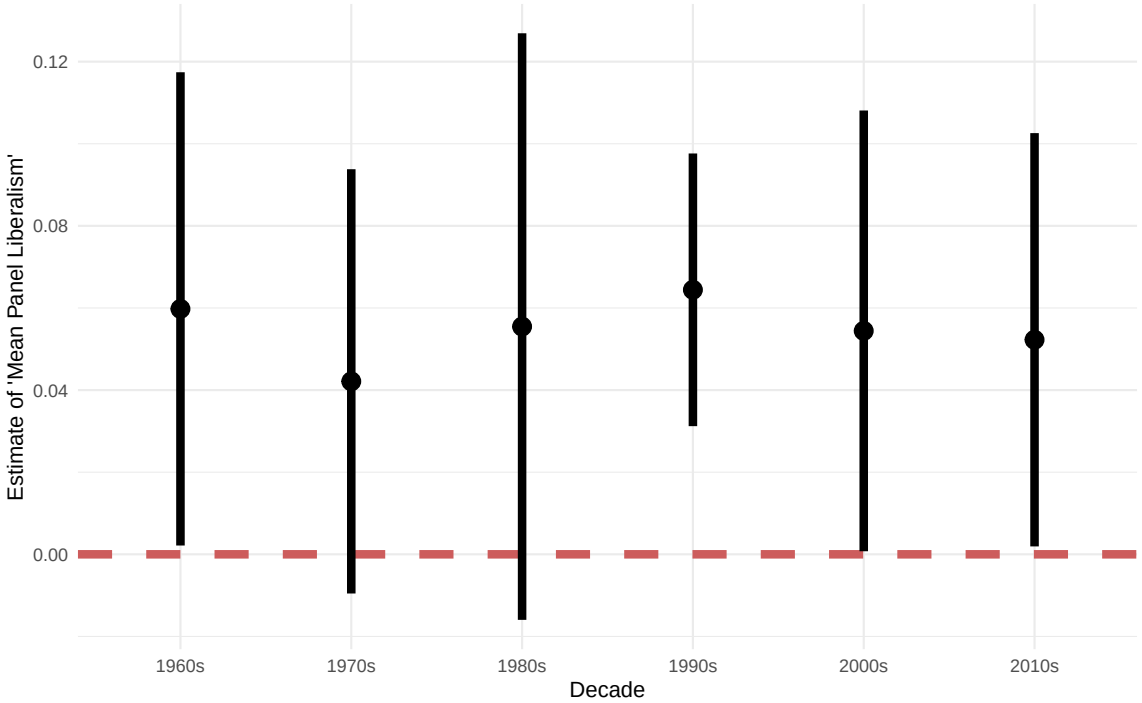


Figure B.3: Iteratively dropping decades

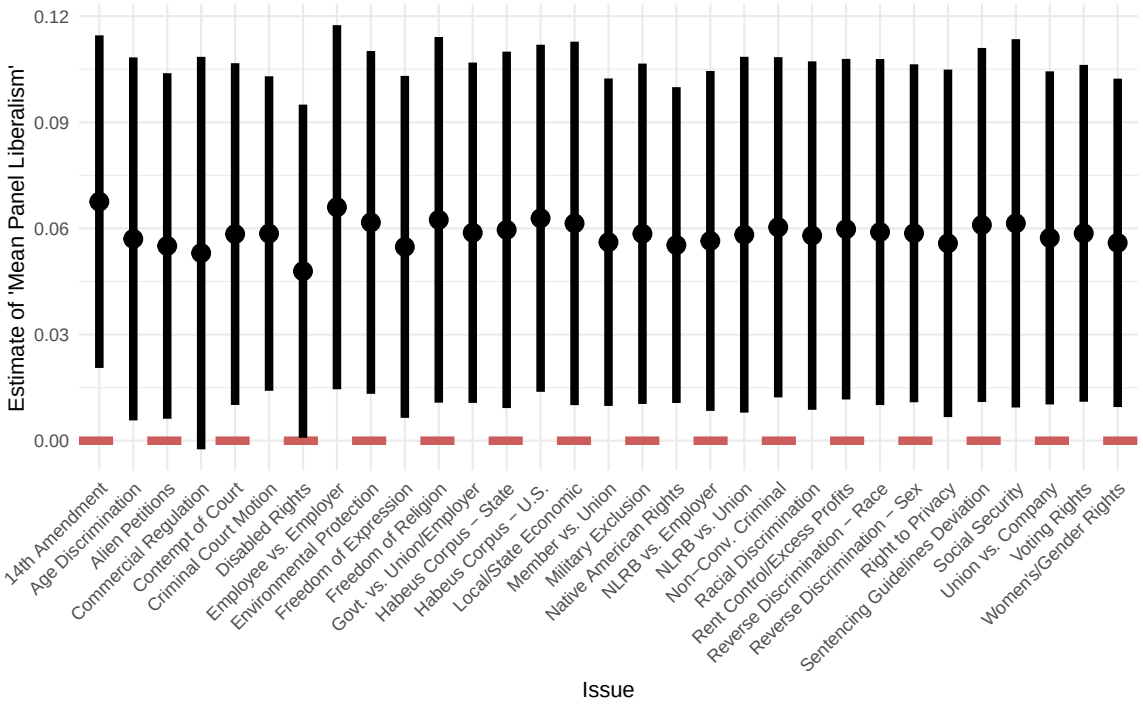


Figure B.4: Iteratively dropping issue areas

C | Appendix to Chapter 3

C.1 The Relationship between Supreme Court and Circuit Court Voting is Not Lagged

One may wonder whether a stronger correlation between Supreme Court and circuit court voting would appear if one used a lagged measure of Supreme Court voting. A danger of this approach if one is interested primarily in judicial rather than litigant behavior is that a contemporary response would be more likely to come from the judges, whereas a lagged response can indicate that different types of cases are being brought. In any event, Table C.1 shows that when using decade fixed effects, there is no effect of Supreme Court voting on circuit court voting when using lags from 0 to 5 years.

Table C.1: Effect of liberal Supreme Court voting on liberal circuit court voting within decade, for different lags of Supreme Court voting

	Liberal circuit voting					
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Liberal Supreme Court voting	−0.10	−0.03	0.003	−0.07	−0.10	−0.01
	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.07)
Decade fixed effects	✓	✓	✓	✓	✓	✓
Lag (years)	0	1	2	3	4	5
N	78	77	76	75	74	73
Adj. R-squared	0.55	0.55	0.54	0.54	0.55	0.54

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

C.2 The Correlation between Supreme Court and Circuit Court Voting is Not Stronger within Circuits

If there is at best a weak correlation between Supreme Court voting and circuit court voting by year, is the correlation stronger when looking within circuit? Figure C.1 gives us reason to doubt. In only seven of the 12 circuits is there even a positive correlation at all.

Furthermore, Table C.2 shows that while there is some correlation between Supreme Court liberal voting and circuit court liberal voting where circuit court liberal voting is measured by circuit-year, this effect disappears once one includes decade fixed effects, just as in Figure 3.2.

Table C.2: Effect of liberal Supreme Court voting on liberal circuit voting, with circuit fixed effects and standard errors clustered by circuit

	Liberal circuit voting	
	Model 1	**Model 2**
Liberal Supreme Court voting	0.13^{**}	-0.09^{*}
	(0.06)	(0.06)
Circuit fixed effects	✓	✓
Decade fixed effects		✓
N	874	874
Adj. R-squared	0.04	0.14

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

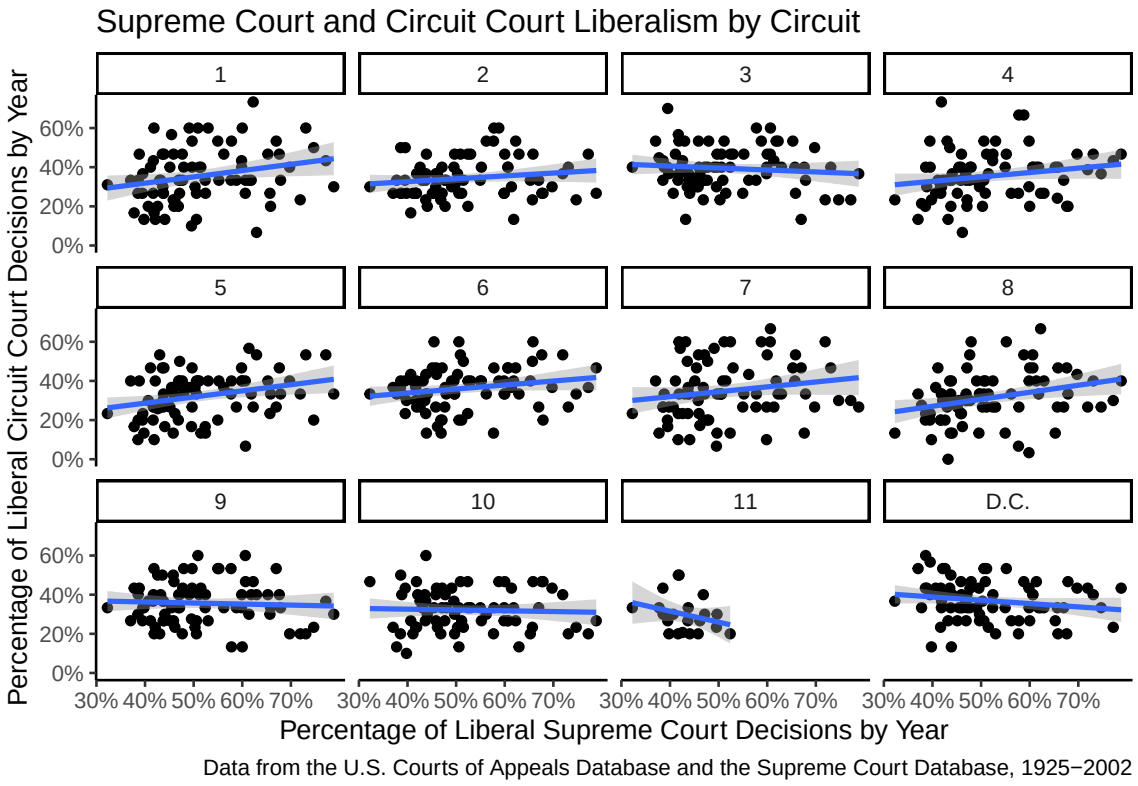


Figure C.1: Correlation between liberal voting by year at the Supreme Court and liberal voting by year in the Courts of Appeals

C.3 A Model of Strategic Litigant Behavior

Could strategic litigants be causing the pattern we observe? A simple model of litigant behavior suggests this is unlikely to be the case. When the Supreme Court is likely to favor a given position, only those who favor that position have an incentive to induce a circuit split and increase the probability of Supreme Court review. The groups that favor the Supreme Court's preferred position, in turn, are better off filing first in more favorable circuits rather than in less favorable ones. This sequencing would lead to the first circuit considering an issue to be *more* likely to agree with the Supreme Court rather than less.

Imagine a game with two players (a liberal group and a conservative group), two appellate courts (a liberal appellate court and a conservative appellate court), and a conservative Supreme Court. There are two periods. To simplify, we will assume that in both periods either group can bring an appeal of an unfavorable trial court decision in either appellate court. In the first period either group can bring a claim in either court or choose not to bring a claim; in the second period the choices are the same, except if one court ruled in the first period, it is no longer available in the second period, and if both courts ruled in the first period, the second period is skipped and we move immediately to the payoffs. The assumption that each group can only file in one court per period reflects the oftentimes very high cost of litigation and that the players therefore are operating under resource constraints.

The payoff for each group is as follows:

$$U = w_1 A + \delta_g w_2 A + \delta_g^p w_3 S$$

where A is the value of winning at the appellate court level and S is the value of winning nationwide. w_1 and w_2 are variables that take on the value of 1 if the group wins in the subscripted period, -1 if the group loses, and 0 if no case was brought. w_3 is a variable

that takes on the value 1 if a split developed and the Supreme Court rules in a group's favor, -1 if a split developed and the Supreme Court rules against a group, and 0 otherwise. We will assume that winning nationwide is better than winning at both appellate levels, so $S > 2A$. p is the number of periods that elapsed before the game ends (1 or 2). δ is the discount factor, which for convenience we will assume is greater than 0.5. Finally, the courts rule deterministically in line with their ideology.

The best way to understand this game is to begin by considering the conservative group's strategy in the absence of the liberal group. The conservative group cannot get a better payoff than by bring a case in the first period in the conservative court and in the second period in the liberal court. This leads to the payoff $(1-\delta)A + \delta^2 S$.[1]

When we add in the behavior of the liberal group, we see that the best response of the liberal group to this proposed strategy by the conservative group depends on both the relative value of S relative to A and the discount factor. If the liberal group does not bring a case in either period, its payoff is $(\delta - 1)A - \delta^2 S$ and if it brings a case in the liberal court in period 1 its payoff is $A - \delta S$. Either way, the conservative group cannot improve its payoff by changing its strategy, so this represents a best response to a best response. These are the only equilibria (depending on the values of S, A, and δ) except for trivial deviations (e.g., the liberal group choosing to bring a case at the same circuit at the same period as the conservative group, which affects neither group's payoffs). The conservative group is always best off inducing a split than in failing to do so, and it is always better off with this order of bringing the cases; conversely, the liberal group cannot affect this dynamic in any meaningful way other than by moving quicker, which may make sense if a victory today is worthwhile enough relative to the loss tomorrow.

[1] Since $S > 2A$ and $\delta > 0.5$, the strategy whereby the conservative group takes a victory in the first period and does not move in the second period is dominated by the conservative group seeking a circuit split. This restriction makes real-world sense; legal groups generally would prefer victory nationwide to victory in a single circuit, even if that involves some temporary losses along the way.

With neither behavior by the liberal group do we observe a pattern of cases that would pose a threat to our empirical strategy. If the liberal group brings a case in the first period, then both groups have acted in that period and it is a coin flip which one is actually "first." If the liberal group does not bring a case, then the first case agrees with the Supreme Court's resolution and the second case disagrees, which is the opposite of the pressure we expect to see from judges who are motivated by fear of reversal.

C.4 There Isn't a Linear Trend in Agreement as a Circuit Split Develops

Figure C.2 shows the cumulative proportion of cases in a circuit split that take the liberal position on the issue as a function of the order the case appears within the split. The first case chronologically we have in the dataset for a given circuit split is ordered as '1', the second one as '2,' and so on.[2] The figure plots two curves: a loess curve for all the splits where the ultimate resolution by the Supreme Court was in a liberal direction and another for where the resolution was conservative.

The striking result in Figure C.2 is that for the first case to hear an issue, there is about a 50% chance that case has a liberal outcome, regardless of what the Supreme Court's ultimate resolution of the issue ended up being. But as more cases get heard at the appellate level, the proportion of the cases going in a liberal direction begins to track the Supreme Court's final decision. This fits with what we saw in Figure 3.4.

One may wonder whether the difference in agreement between the first case in a split and subsequent cases was masking a more linear trend. If so, this would call into question the mechanism where the first case did not appear to have a high chance of review but the second and subsequent cases did. In fact, it would support a different mechanism, where cases later in the development of a split either have greater legal knowledge or greater ability to predict the Supreme Court's resolution and thus are more likely to vote in line with the Supreme Court. However, Table C.3 shows that the order a case appeared in a split has no additional predictive power beyond simply knowing whether a case appears first.

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[2] In general, given the citation practices of the Supreme Court in referencing circuit splits, this will lead to only a single case per circuit.

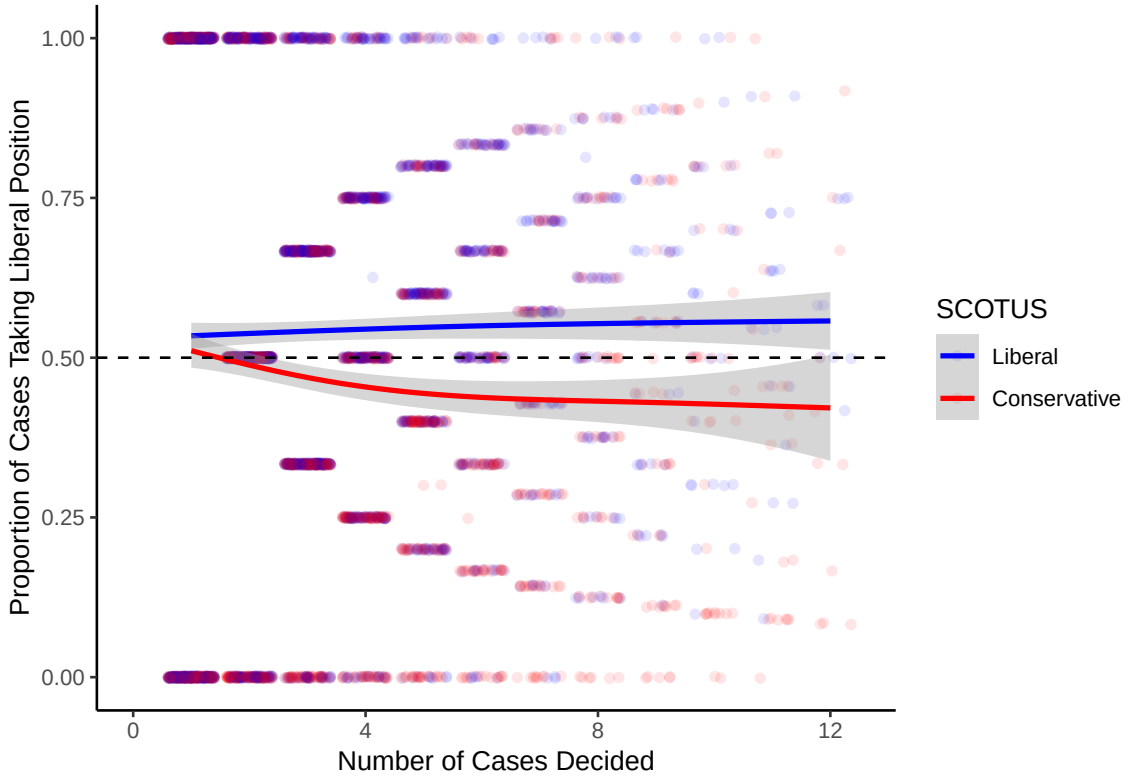

Figure C.2: Cumulative proportion of cases in a split taking the liberal position as a function of the order a case appears within the split

Table C.3: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative), taking into account the order of the case in the split

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.05^{**}	0.05^{**}	0.08^{***}
	(0.02)	(0.02)	(0.03)
Order of case in split	-0.001	-0.0004	-0.004
	(0.003)	(0.003)	(0.005)
Expected agreement	0.10^{***}	0.09^{***}	0.11^{***}
	(0.02)	(0.02)	(0.02)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4032	4032	2742
Adj. R-squared	0.01	0.02	0.08

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

C.5 Limiting the Data to Splits where the Second Case Emerged within Two Calendar Years

Table C.4: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Limited to cases where the second case was decided two or fewer calendar years after the first.

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.06*	0.06*	0.06**
	(0.03)	(0.03)	(0.03)
Expected agreement	0.09***	0.08***	0.11***
	(0.02)	(0.02)	(0.02)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	2560	2560	1825
Adj. R-squared	0.01	0.02	0.11

***p $<$ .01; **p $<$ .05; *p $<$ .1

C.6 There Is No Clear Change in the Magnitude of the Effect over Time

Figure C.3 investigates whether the effect of a case not being first has increased or decreased over time. The coefficients are from a regression like Model 2 in Table 3.2, but with the data subsetted to five year periods based on when the Supreme Court heard the case. There appears to be no clear pattern over time, which fits with the basic proposed mechanism since the rate of review for cases with circuit conflict far exceeded other cases for the whole period. Of course, the small sample size for each five-year period means that it is difficult to make precise inferences. However, in a model with all the data and an interaction between the year the Supreme Court took the case and *Not first case*, the coefficient on the interaction is -5×10^{-4}, s.e. 0.0025. This is both substantively small and statistically insignificant, suggesting that there has been no linear trend over the time of the study.

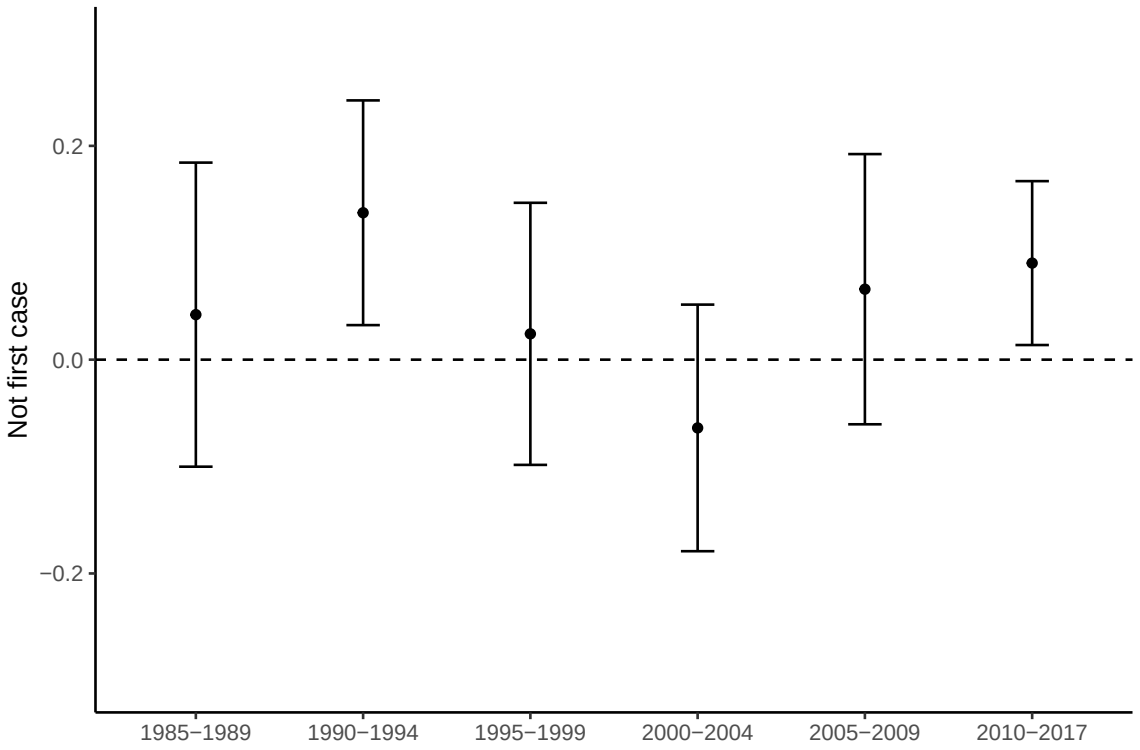

Figure C.3: Effect of a case not being the first within a split, subsetted by five-year periods, based on when the Supreme Court took the case

C.7 Logistic Regression

Table C.5: Agreement with the Supreme Court's decision in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Coefficients are from a logistic regression.

	Agrees with Supreme Court		
	Model 1	**Model 2**	**Model 3**
Not first case	0.19^{**}	0.21^{**}	0.37^{**}
	(0.09)	(0.09)	(0.14)
Expected agreement	0.39^{***}	0.38^{***}	0.59^{***}
	(0.08)	(0.08)	(0.12)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4032	4032	2742
Log Likelihood	−2760.96	−2712.20	−1530.20

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

C.8 Alternative Dependent Variable: Number of Supreme Court Votes

Table C.6: Number of Supreme Court votes for the circuit's position in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Coefficients are from a linear regression.

	Supreme Court votes		
	Model 1	**Model 2**	**Model 3**
Not first case	0.34^{**}	0.36^{***}	0.42^{**}
	(0.13)	(0.14)	(0.18)
Expected agreement	0.61^{***}	0.57^{***}	0.68^{***}
	(0.14)	(0.13)	(0.18)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4027	4027	2738
Adj. R-squared	0.01	0.02	0.08

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table C.7: Number of Supreme Court votes for the circuit's position in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Coefficients are from a Poisson regression.

	Supreme Court votes		
	Model 1	**Model 2**	**Model 3**
Not first case	0.07^{**}	0.08^{***}	0.08^{**}
	(0.03)	(0.03)	(0.04)
Expected agreement	0.13^{***}	0.12^{***}	0.14^{***}
	(0.03)	(0.03)	(0.04)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4027	4027	2738
Log Likelihood	−12172.31	−12044.97	−7459.99

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table C.8: Number of Supreme Court votes for the circuit's position in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Coefficients are from a negative binomial regression.

	Supreme Court votes		
	Model 1	**Model 2**	**Model 3**
Not first case	0.07^{**}	0.08^{**}	0.10^{**}
	(0.03)	(0.03)	(0.04)
Expected agreement	0.13^{***}	0.12^{***}	0.15^{***}
	(0.03)	(0.03)	(0.04)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4027	4027	2738
Log Likelihood	−10562.71	−10528.79	−6953.72

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table C.9: Number of Supreme Court votes for the circuit's position in a circuit split case as a function of whether it is the first circuit in the split and whether the median judge's partisanship aligns with the ideological direction of the Supreme Court's ultimate decision (Democrat-liberal or Republican-conservative). Coefficients are from an ordered logit regression.

	Supreme Court votes		
	Model 1	**Model 2**	**Model 3**
Not first case	0.18^{***}	0.19^{***}	0.23^{**}
	(0.07)	(0.07)	(0.09)
Expected agreement	0.31^{***}	0.29^{***}	0.38^{***}
	(0.07)	(0.07)	(0.09)
Circuit-decade fixed effects		✓	✓
Legal issue fixed effects			✓
N	4027	4027	2738

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

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