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Judges Judging Judges: Polarization in the U.S. Courts of Appeals

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Judges Judging Judges: Polarization in the U.S. Courts of Appeals*

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Abstract

We study how politicization and polarization shape judicial review within U.S. Federal appellate courts. Using a dataset of more than 400,000 cases from 1985 to 2020, we show that the political alignment of appellate judges with trial judges increasingly influences reversal decisions — especially after 2000 and in published, precedential cases. Appellate panels with three Republican- or Democratic-nominated judges are 3.5 percentage points more likely to reverse trial judges nominated by the opposing political party (where the symmetry is not mechanical). Moving from a three-Republican to a three-Democratic nominated panel, each additional Democratic-nominated judge increases reversal rates of Republican trial judges by 2.4, 6.5, and 13.3 percentage points, respectively. The gradient is more than halved when the trial judge is also Democratic-nominated. These patterns persist across both ideological and non-ideological cases, suggesting political affinity, not just judicial philosophy, drives outcomes. Polarization has intensified even among pre-2000 appointees, indicating behavioral shifts beyond appointment changes. While evidence of gender-based polarization is limited, panels with a majority of minority-identity judges are 10 percentage points less likely to reverse minority trial judges. Importantly, when modeling the joint decision to reverse and to publish a case, we find that polarization effects are present in both published and unpublished reversals.

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1 Introduction

Politicization and polarization are increasingly prevalent across many domains of decision-making in the United States and globally.[1] We use the term *politicization* to refer to the impact of political affiliation on decisions, and *polarization* to refer to divergence of viewpoints and the extent to which that divergence shapes the outcome of decisions. These phenomena are particularly evident in the judiciary, where ideological divides have seemingly intensified. The recent shift toward a conservative supermajority on the Supreme Court and a spate of contentious decisions — for example, on abortion, presidential immunity, environmental law, and recently on the extent of presidential powers — has ignited a debate on these trends.

Using a comprehensive dataset of over 400,000 federal appellate cases from 1985 to 2020, we provide systematic evidence of increasing polarization within the federal judiciary that extends beyond the Supreme Court. While prior research has documented politicization — the tendency of judges' own political affiliations to influence their decisions — using limited subsets of the data, segmented by year, case type, publication status, or Circuit, the question of polarization — whether political alignment between appellate and trial judges affects outcomes — has received limited attention, with the few prior studies finding no effect.

Our analysis documents a shift in judicial behavior beginning around the year 2000, during the same period when polarization intensified at the Supreme Court (Bonica and Sen, 2021). We show that during this period Circuit Court panels increasingly favor trial judges who share their political affiliation, and the effect is most pronounced in published decisions that establish legal precedent. Published cases have greater visibility and precedential impact, making them a natural setting for asserting judicial viewpoints during a period of heightened polarization. This pattern is observed both in ideologically charged cases and in those without ideologically contested issues, and also appears among judges appointed before the period of heightened polarization,

These politicization and polarization effects are especially relevant given the institutional context of the Circuit Courts. While the Supreme Court garners the lion's share of public attention, it has only nine judges and selectively chooses 70 to 80 cases each year. In contrast, the Circuit Courts of Appeals — the intermediate federal appellate courts — handle tens of

[1] Kastellec (2011); Coffey and Joseph (2013); Epstein et al. (2013); Pew Research Center (2014); Gentzkow (2016); Allcott et al. (2020); Boxell et al. (2021); Duchin et al. (2023); Cohen (2025).

thousands of cases annually and exert substantial influence over American law. These courts have limited discretion over their dockets and cover a wide range of issues. Federal courts address civil matters, such as breach of contract and discrimination; bankruptcy; federal crimes; international trade; and regulatory matters. Their decisions shape the economic, regulatory, and social environment in the U.S.[2] The Circuit courts consider appeals from Federal trial courts and Federal agencies, meaning they often handle cases of particular legal or societal importance. Thus, for any case not appealed to the Supreme Court, which is the vast preponderance of cases, Circuit Courts have the final say. As such, the politicization and polarization of the court system can affect its consistency, predictability, and effectiveness, for example, by shifting regulatory interpretations and enforcement and by reinforcing the also documented politicization of trial court decisions (Cohen and Yang, 2019; Kritzer et al., 2017).

Prior literature on Circuit Courts has established that political composition affects judicial decision-making through both direct and indirect channels. Studies examining direct politicization effects find significant differences in voting based on political affiliations. For example, Songer and Davis (1990), focusing on specific issues (labor relations, criminal appeals, First Amendment, and civil rights) and time periods, document systematic differences in how judges vote. Epstein et al. (2013), using an extended version of the dataset assembled by Sunstein et al. (2004, 2006) that encompasses a selection of published cases, find significant differences in reversal rates between panels with more Democratic-nominated versus Republican-nominated judges. Kastellec (2011) confirms significant variation in the political composition of the panel over time and finds modest but increasingly pronounced differences in voting behavior. Most recently, Cohen (2025), using a comprehensive dataset of about 640,000 circuit court decisions from 1985 to 2020, demonstrates that political composition effects extend to about 90 percent of all federal circuit court cases—vastly broader than prior work appreciated. Beyond direct effects, the literature shows that political ideology shapes outcomes indirectly through strategic behavior in response to the ideological composition of the full circuit (Kim, 2009), the configuration of appellate panels (Revesz, 1997), and collegial dynamics among judges (Kastellec, 2011). Berdejo and Chen (2017) explore how

[2]Examples of significant appellate cases include antitrust cases such as *United States v. Microsoft* (D.C. Circuit, 1998), *SEC v. Texas Gulf Sulphur* (2nd Circuit, insider trading), and *Perry v. Schwarzenegger* (9th Circuit, same-sex marriage, 2012). A more recent example is *McMahon v. World Vision* (9th Circuit, 2024), involving religious employer discrimination.

election cycles further influence decision-making.

Recent work has begun examining non-political identity effects in judicial decision-making. In the Circuit Courts, Ash et al. (2024) use text analysis to show that judges' gender attitudes predict the likelihood of reversing female trial judges, while Battaglini et al. (2023) demonstrate that exposure to female colleagues increases female law clerk hiring. These identity-based effects parallel broader patterns of increasing polarization documented in the Supreme Court, where Democratic- and Republican-nominated justices increasingly concentrate on opposing sides of divided cases (Epstein et al., 2015; Devins and Baum, 2016; Hasen, 2019). Ash et al. (forthcoming) show that exposure to economics training from the Manne Institute led to the increased use of economic language in opinions. While this literature has established that judges' own political affiliations and training influence their decisions across nearly all case types, the question of polarization — whether political alignment between appellate and trial judges affects outcomes — remains largely unexplored.[3]

Our paper fills this gap by investigating whether political alignment between appellate panels and trial judges influences the likelihood of reversal in the U.S. Circuit Courts of Appeals. We proxy for political affiliation using the political party of the nominating president and capture polarization effects by examining whether partisan alignment — or lack thereof — affects outcomes. In particular, we examine whether Republican- (Democratic-) nominated judges are more (less) likely to reverse decisions made by Democratic-nominated trial judges. In keeping with the empirical literature, we treat the assignment of trial cases to appellate panels as random.

Our approach builds on and extends prior work in several key ways, making five primary contributions to the literature. First, unlike prior studies examining politicization that focused on subsets of published cases, subsets of years, specific circuits, or cases addressing specific questions in specific years, we examine the universe of both published and unpublished cases from 1985-2020. This comprehensive dataset allows us to track pairings of trial judges and appellate panels over time, enabling fine-grained controls through Circuit

[3]The closest studies we are aware of are Epstein et al. (2013), Berdejó (2013), and Boyd (2015). Epstein et al. (2013), drawing on the dataset developed by Sunstein et al. (2004, 2006), which includes approximately 4,500 appellate cases, find that the ideological identity of the district judge does not significantly influence the appellate outcome; rather, it is the ideological direction of the district court's decision that matters. Berdejó (2013), using data from the Ninth Circuit between 1991 and 2006, similarly finds no significant effect of panel-trial judge alignment. Boyd (2015) examines a subset of cases from 2000 to 2004 and explores how ideological distance between the appellate panel and the district court judge affects the likelihood of reversal.

× Year fixed effects, both Appellate Panel and Trial Court Judge fixed effects, and in our most stringent specification Appellate Panel and Trial Judge fixed effects to control for unobserved, time-invariant covariates. Second, we distinguish between ideological and partisan motivations by comparing results across ideological and non-ideological cases — if polarization were purely ideological, we would expect stronger effects in ideological cases. Third, given the documented rise in political polarization around the year 2000 (Sunstein et al. (2004); Bartels (2009)), we examine the pre- and post-2000 periods separately and analyze published cases (which have precedential value) and unpublished cases (which tend to be routine) separately, as published cases present a more visible arena for polarization. Fourth, beyond political affiliation, we assess the relative importance of other judge characteristics (gender and minority status) in judicial decision-making by examining interactions between these trial judge characteristics and appellate panel composition. Fifth, we explore whether the publication decision itself represents an additional margin for political influence, using bivariate probit analysis and Lee (2009)-style bounds to address selection effects.

We find robust evidence of judicial politicization and polarization: appellate panels' political composition significantly affects reversal rates of trial court decisions (politicization) and panels increasingly favor trial judges who share their political affiliation (polarization). The politicization effect is observed in the full sample, but is stronger post 2000 and for published decisions (see also Boyd (2015)). In published cases post 2000, panels with two Republican-nominated judges and one Democratic-nominated judge (RRD) are 2.4 percentage points more likely to reverse a prior ruling by a Republican-nominated trial judge than panels composed entirely of Republican-nominated judges (RRR) (with effects of 6.5 and 13.6 percentage points for RDD and DDD panels, respectively). For comparison, the baseline reversal rate is 37 percent. The polarization effect is equally striking: RRD, RDD, and DDD panels are substantially less likely to reverse Democratic-nominated trial judges, with reductions in reversal rates of 3.1, 4.4, and 7.0 percentage points respectively. Conversely, all-Republican panels (RRR) are 3.5 percentage points more likely to overturn Democratic-nominated trial judges.

We investigate several mechanisms that could underlie these results. These results could be driven by increasing polarization among judges or by the appointment of increasingly polarized judges; we split our analysis and focus on post-2000 cases decided by judges nominated prior to 2000. We continue to find the polarization effect in this subset of cases,

suggesting that judges have become more polarized over time (although this does not preclude that judges appointed post-2000 are also more polarized). We also investigate whether our polarization results reflect divergent judicial philosophy or political affinity. To distinguish between these mechanisms, we split our analysis into ideological and non-ideological cases. If polarization were driven primarily by judicial philosophy, we would expect stronger effects in ideological cases, where legal doctrine is more contested. However, if political affinity plays a role, we would expect similar effects across both types of cases. We find similar polarization effects in both ideological and non-ideological cases, suggesting that our results are driven at least in part by political affinity rather than solely by differences in judicial philosophy.

Beyond political alignment, we examine whether similar patterns emerge along other identity dimensions and in the publication process itself. Notably, we do not find a polarization effect with respect to gender — female appellate panels do not treat female trial judges any differently. However, we do find that appellate panels with a majority of minority-identity members are less likely to reverse a minority-identity trial judge, even after accounting for the independent effects of political affiliation. The effect is substantial: a 10 percentage point reduction in the likelihood of reversal. We also examine whether the publication decision itself is politicized by modeling reversal and publication as joint outcomes using a bivariate probit analysis. We find that when we endogenize the publication decision, polarization effects are found both in publish and unpublished cases. This suggests that polarization is not only confined to cases that establish a public record; it is also observed in more routine decisions.

The balance of the paper is organized as follows. Section 2 provides institutional context for the Circuit Courts. Section 3 discusses the construction of the dataset and provides descriptive summary statistics. Section 4 presents our main results. Section 6 concludes.

2 Institutional Background

The U.S. federal court system has three branches: the trial courts (primarily District Courts, along with a few specialized courts); the Circuit Courts, which are the intermediate appeal courts; and the Supreme Court, which is the final level of appeal.

All federal judges, under Article III of the Constitution,[4] are selected by the President and confirmed by the Senate. Federal judges are appointed for life and may resign or retire earlier, and in rare cases, they can also be removed by impeachment. There are two types of judges who hear cases: active and senior judges. Active judges are judges who are currently serving full-time. Senior Judges are judges who retire but choose to remain authorized to hear and decide cases. Senior judges have the same responsibilities as active judges, but they have a reduced caseload and more flexibility in how they manage their workload.[5]

A. The Federal District Courts

There are 94 District Courts, and as of 2020, there were 670 authorized District Court judges. Most of the cases brought in the District Courts are heard by a single judge randomly assigned to the case (Copus et al., 2025). Each final ruling by a District Court can be appealed to the United States Courts of Appeals for the judicial circuit in which the District Court is located (with the exception of cases involving patents and certain other specialized matters where there is a specialized intermediate appellate court). In rare cases, an appeal can be brought directly to the United States Supreme Court.

B. The Federal Circuit Courts of Appeals

There are 13 Federal Circuit Courts of Appeals and as of 2020, there were 180 active circuit court judges. Most cases heard in the Courts of Appeals are decided by a panel of three judges. A small number of cases are heard "en banc," which means that the cases are reviewed by all active judges in the specific Circuit. The panels consist of active and senior judges, and in some cases the panels also include a visiting judge from another Circuit or District Court who is assigned temporarily, for a specific case or for a specific period of time.

When judges hear a case, they review all the relevant evidence and arguments presented to them by both sides. The judges' decisions are expected to be based on their interpretation of the law and the facts of the cases. In some cases, the decision of the panel is unanimous, meaning that all judges on the panel agree on the outcome. In other cases, the decision is split. In such cases, the judge who disagrees with the majority's decision may write a "dissent" explaining their disagreement. Once a panel reaches a decision, it can choose

[4]Article III of the Constitution applies to the U.S. Supreme Court, the Courts of Appeals, and the District Courts. However, not all federal courts are Article III courts. For example, the U.S. Tax Court and the U.S. Court of Federal Claims are Article I courts or Article IV territorial courts where judges are appointed for a fixed term, which may be renewed or extended. These courts are created by Congress to handle specialized areas of federal law.

[5]Supreme Court Justices do not take senior status; they fully retire or remain active.

whether to publish its opinion. Published opinions are typically issued in cases that involve novel or significant legal issues or when there is a split among the judges on the panel. Published opinions are binding precedent for future cases in that Circuit. Unpublished opinions are typically issued in cases that are routine, involve well-settled legal principles, or do not raise novel legal issues (see Brown et al. (2022) for further discussion, and Lu and Chen (2025) for an analysis of politically motivated reasoning in the text of published cases). Unpublished opinions may be cited as persuasive authority in future cases but are not binding in future cases. Over the years the use of unpublished opinions has increased dramatically.

C. Random Assignment of Circuit Court Judges to Panels

The premise of many empirical studies (e.g., Tiller and Cross (1999); Sunstein et al. (2004, 2006); Abramowicz and Stearns (2005); Sunstein and Miles (2008); Epstein et al. (2011); Kastellec (2011); Chen and Sethi (2018); Cohen and Crystal (2019)), including this paper, is that judges and cases are randomly assigned to circuit court panels. Some recent studies that examined this assumption (Chilton and Levy, 2015; Levy, 2017; Fischman, 2011) concluded that the assignment of judges to panels, even if not purely random, deviates from perfect randomness for technical reasons that are generally independent of political polarization.

In Section 4.B, below, we present balance checks which confirm that appaellate panels appear to be randomly assigned to cases.[6]

3 Data

A. Construction of the Data

The paper uses data compiled from three data sources.[7] The first is the Administrative Office of the U.S. Courts (AOC) Database, which allows users to obtain case and docket information from U.S. Federal Court documents. This database provides comprehensive information on a large portion of all cases handled by the various U.S. federal courts. We

[6] Of course, we are unable to test whether appeals themselves are non-random since our data set includes only cases that were appealed from the trial court. At the same time, it is worth noting that at the time appeal decisions are made, the composition of the appellate panel would be unknown, only the political composition of the circuit, for which we control with Circuit × Year fixed effects.

[7] Cohen (2025) uses an overlapping data set, which does not however contain information on the trial judges.

use it to obtain rich information about Circuit Court cases, including the Circuit Court where the case was heard, docket number, the District Court whose case is reviewed, dates, panel decision, case type, whether there was an en banc decision, etc.

Complementing this data, we use LexisNexis for information on the identities of the three panel judges and the lower court judge under review. LexisNexis has the world's largest electronic database for legal and public-records-related information. We also use this source to flag whether cases are ideological or not. The third data source is the Federal Judicial Center (FJC) Biographical Directory of Federal Judges, which offers biographical information on all current and former US federal court judges with life tenure. We use this directory to add information about the judges beyond their names, information such as gender, race, age, tenure, nominating president, and date of nomination, for both Circuit and District Courts.

The merger between AOC Integrated Database and LexisNexis had a match success rate of about 50 percent. The 50 percent of cases appearing in AOC but not in LexisNexis are mostly cases that were terminated on procedural grounds, such as late filing, and for which, in most cases, information on the case decision is missing.

The final data do not include cases from the Twelfth Circuit (the Federal Circuit), which differs from the other Circuits in that it exercises subject-matter jurisdiction rather than geographic jurisdictions. The also do not include cases from the District of Columbia Circuit because for most of the cases we could not identify the trial court judge. Altogether, the final data contain about 440,000 appeals for the period 1985 to 2020 for which we identify the three members of the panel and the trial court judge.[8]

The final data contain about 400,000 circuit court decisions for which we have information on each of the three judges on the panel, the trial court judge, and the outcome of the case.

[8]The merger yields about 810,000 cases. After excluding Federal Circuit Court and D.C. Circuit Court cases, and keeping only cases for which termination was on the merits, we are left with 750,000 cases for the period 1985 until 2020. For 84 percent of these cases we have information on the three-panel judges (for 13% of the cases, we were unable to identify the name of any judge, including en-banc cases). Out of the cases for which we have information on the identity of the three-judge panel, for 400,000 we also have information on the identity of the trial court judge. For most of the remaining cases, the only information that appears in the xml about the judge in the trial court is the name of the special court (special court such as Board of Immigration Appeals or Tax Court) in which the case was heard before reaching the Court of Appeals. Note: in 2024, about three-quarters of appeals are direct appeals from District Court rulings, where the remaining 25 percent comes from administrative agencies, original proceedings, and other miscellaneous filings (see uscourts.gov).

B. Variables of Interest and Descriptive Statistics

Our primary outcome of interest is reversal,[9] which is equal to 1 if the appellate panel made any change to the District Court's original ruling and reversed a case (= 1) or upheld the District Court's ruling (= 0) and the original ruling remained unchanged. We present results on both published and unpublished[10] cases, and also examine the joint decision among these four alternative outcomes for a case.[11]

We measure politicization through the political affiliation of the appellate panel judges and the trial judge, which we proxy using the political affiliation of the president who nominated them. Hence, each three-judge panel can range from being uniformly nominated by Republican presidents (RRR) to uniformly nominated by Democratic presidents (DDD), with combinations such as RRD and RDD representing intermediate cases. Each trial court judge is classified as either Republican- or Democratic-nominated, based on the party of the president who appointed them. This approach is widely used in the literature (Landes and Posner, 2009; Segal and Spaeth, 2002; Sunstein et al., 2004).

We measure polarization by the extent to which Circuit Court judges' decisions defer to, or align with, District Court judges of the same political affiliation, i.e., the extent to which Republican- (Democratic-) nominated appellate judges are more (less) likely to reverse a lower court decision if that decision was reached by a Democratic-nominated trial judge.

We also examine whether other dimensions of identity of the panel and trial court judges affect the outcome, including sex and minority status. Both of these are coded using the FJC Biographical Directory of Federal Judges, with minority status indicating justices who are Black or Hispanic.

In some of our analyses, we compare ideological cases and non-ideological cases. We

[9] We use the variable OUTCOME, as reported by AOC, to define reversal. The variable reversal is coded as to 1 if the appellate panel fully reversed, affirmed in part and reversed in part, or remanded the district court's decision. It is coded as 0 if the panel fully affirmed, dismissed or otherwise upheld the lower court's ruling. Our results are robust to defining Reversal more narrowly — as only full reversals — but we chose to treat any modification of the original ruling as meaningful for our analysis.

[10] We use the variable PUBSTAT, as reported by AOC, to define whether a case is published. The variable published is coded as 1 if the case was published and written — regardless of whether it was signed, unsigned with reason, or unsigned without comment.

[11] The literature on judicial decision making suggests that these decisions are made simultaneously, rather than sequentially. Judges typically confer on both the outcome and the publication status of a case during the same deliberation, often before any draft opinion is written (Wasby, 2004; Merritt and Brudney, 2001; U.S. Court of Appeals for the Ninth Circuit, 2024). This understanding of the process is corroborated by interviews with judges as well as empirical work showing that factors influencing the decision to publish often overlap with factors deciding the case's outcome (Wasby, 2004; Merritt and Brudney, 2001).

define ideological cases using Cohen (2025)'s extension of the procedure proposed by Sunstein et al. (2004). Specifically, a case is identified as ideological if the opinion includes keywords or cites key Supreme Court cases intersecting with fourteen issues identified as ideologically salient by Sunstein et al. (2004) (e.g., abortion, capital punishment)[12] and two additional issues added by Cohen (2025) (LGBTQ- and Second Amendment-related cases). See Cohen (2025) for additional details.

Table [1] presents summary statistics for our outcome of interest, as well as key characteristics of the case, the appellate panel, and the trial judge. The table breaks down the data along three dimensions: political affiliation of the trial judge (59 percent Republican-nominated, 41 percent Democratic-nominated), publication status of the case (72 percent unpublished, 28 percent published), and time period (44 percent pre-2000, 56 percent post-2000).

Our outcome of interest, which is whether the appellate panel reverses the trial court ruling, occurs in approximately 18 percent of cases overall. Notably, this percentage varies considerably across subgroups. It is much higher for published cases compared to unpublished cases (35-37 percent, compared to 10-12 percent, depending on the time period). Cases are also categorized by the type of issue they address: ideological, civil, criminal, or other. Ideological cases constitute one third of all cases but represent half of published cases.

Focusing on the political composition of appellate panels, Table [1] panel B, shows that one tenth are DDD, 18 percent are RRR, 40 percent are RRD, and the remaining 33 percent are RDD. These percentages vary somewhat when compared across cases with a Republican-nominated or Democratic-nominated trial court judge. However, the differences are relatively small, e.g., 17 percent of panels are RRR when the trial judge is Democratic-nominated, compared to 18 percent when the trial judge is Republican-nominated. Nearly half of panels include at least one female judge, with the share increasingly markedly after 2000. Additionally, 33 percent of panels have at least one minority judge, with a modest increase in the post-2000 period.[13]

Finally, Table [1] Panel C provides trial judge characteristics. We note that 17 and 14 percent of judges respectively are women and minorities, with both percentages increasing

[12]The full list of key words includes: abortion, affirmative action, campaign finance, capital punishment, commercial speech, criminal procedure, establishment clause, federalism, free exercise clause, gender discrimination, race discrimination, second amendment (gun control), takings clause (property rights), and voting rights.

[13]We identify minority judges as those who are Black or of Hispanic origin.

significantly post-2000. With lifelong tenure, it is not surprising that the average age of judges is more than 60, with an upward trend post-2000. Note that the fractions of women and minority trial judges are higher among trial judges who were nominated by Democratic presidents. While only 11 and 9 percent of the Republican-nominated trial judges in our sample are women and minorities, these percentages increase to 26 and 21 for Democratic-nominated trial judges.

4 Results

In this section, we present results using our reference specification. While we report effects for the full sample and separately for the pre- and post-2000 periods, our main specification also splits the sample by published and unpublished cases. In a subsequent section, we examine whether the decision to publish or not publish affects our results, both as a robustness check to assess whether it changes the findings presented in this section and also as a substantive and interesting choice joint with the reversal/affirmation decision, which has not been previously investigated in the literature.[14]

A. Specification

We regress Circuit Court reversal of the trial court ruling on indicators for the political composition of the appellate panel (with RRR as the omitted category), along with its interaction with the political affiliation of the District Court judge:

$$\begin{aligned}\text{Reversal}_{ict} = \beta_0 + \beta_1 RRD_{ict} + \beta_2 RDD_{ict} + \beta_3 DDD_{ict} + \beta_4 RRD_{ict} \times \text{Trial Judge Dem}_{ict} \\ + \beta_5 RDD_{ict} \times \text{Trial Judge Dem}_{ict} + \beta_6 DDD_{ict} \times \text{Trial Judge Dem}_{ict} + \\ \beta_7 \text{Trial Judge Dem}_{ict} + \beta_8 X_{ict} + h_{ct} + j_d + \delta_{at} + \gamma_{nos} + \theta_{ot} + \varepsilon_{ict}\end{aligned} \tag{1}$$

The unit of observation is case i, in circuit c, in year t. Since judges are essentially randomly assigned to panels and panels to cases, we can treat the estimates of the coefficients as causal. In addition to these, we include case-level controls, X_{ict}, such as the political affiliation of the trial court judge, indicators for whether they are female or minority, and

[14] While the prior literature has examined the decision to publish or not (e.g., Revesz (1997)) and reverse/affirm (e.g, Songer et al. (1994); Epstein et al. (2011) etc), the joint decision has, to the best of our knowledge, not previously been examined.

their seniority status (chief justice, senior justice, tenure, tenure squared). We also include appellate panel-level controls for the proportion of women, proportion of minorities, and average tenure. Finally, we include fixed effects at the levels of Circuit × Year, the district from which the trial case was appealed, appeal type, nature of the suit (for civil cases), and offense type (for criminal cases).[15] The inclusion of these fixed effects allows us to control for time-varying confounders within circuits over time. For example, prior literature has suggested that shifts in the political composition of Circuit over time could induce strategic behavior by trial court judges. The richness of our data allow for the inclusion of more fine-grained fixed effects, which we do as robustness checks, including Appellate Panel fixed effects (so focusing just on variation in trial judges within a given three-judge panel), Trial Judge fixed effects (for a given trial judge examining the effect of variation in the appellate panel), and both of these together.

B. Balance Checks

In this section, we confirm that, for our data, there is negligible evidence of nonrandom assignment of appellate judges to panels.

In Figure [1] we provide an overview of the balance of case attributes with respect to the political composition of the appellate panels. Using the specification outlined in Section 4.A, we regress case outcomes (whether the case was reversed, published, or a judge dissented) and a range of case attributes (e.g., whether the case was ideological or characteristics of the trial judge such as political affiliation, gender, minority status) on indicators for panel composition (RRD, RDD, and DDD, with RRR as the omitted category), along with Circuit × Year and district fixed effects. Within each of the three panels, we depict the coefficients for RRD, RDD, and DDD separately (so the coefficient for a given attribute in each panel, e.g., age of the trial court judge, is from a single regression).

The first three coefficients are outcomes (reversed, published, dissent), so we would not expect these to be balanced. These coefficients shows that Democratic-nominated judges are significantly more likely to reverse a trial court's decision. Consistent with this, panels with more Democratic-nominated judges are also more likely to publish cases. The pattern of dissent, also varies with panel composition, although less dramatically.

The subsequent coefficients allow us to assess balance across observable case and trial

[15]For nature of the suit and offense type, we use two-digit AOC codes derived from the three-digit NOS codes and the four-digit OFFENSE code, respectively.

judge attributes. These coefficients are generally small and none are statistically significantly different from zero at standard levels. This includes attributes of the case such as case type (civil or criminal), and attributes of the trial court judge (Democratic- vs. Republican-nominated, gender, seniority, race, age, and tenure).

Overall, the evidence in Figure [1] corroborates the view that cases are randomly assigned to appellate panels. This allows us to sidestep issues of selection and non-random assignment of judges, and to treat the estimates of panel composition and their interaction with trial court judge characteristics as causal.

C. Main Results

In Table [2], we present our main results: column 1 reports estimates for the full sample; columns 2 and 3 split the sample into pre- and post-2000; and columns 4 to 7 further divide the data by publication status (published and unpublished) within each time period (pre- and post-2000 periods).

We begin by examining the effect of the political composition of the appellate panel on reversal rates — what we refer to as *politicization*. We first focus on reversals of Democratic-nominated trial judges by RRR panels (which serves as the omitted category and is reflected in the coefficient for Democratic trial judge). While we find no effect in the overall sample, column 1, in the post-2000 period (column 3), a period in which politicization intensified, we find that RRR panels are one percentage point more likely to reverse Democratic-nominated trial judges. In columns 4 to 7, when unpacking by pre- vs. post-2000 and published vs. unpublished and by publication status, we find that post-2000, the effect is more pronounced among published cases (3.5 percentage points, significant at the one percent level).

We next examine reversals for Republican-nominated trial judges by appellate panels containing at least one Democratic-nominated judge (β_1 through β_3) (we will discuss the interaction effects below). Panels with an increasing number of Democrats-nominated judges are increasingly likely to reverse rulings by Republican-nominated trial judges than all-Republicans panels, with a positive gradient from RRD, to RDD, to DDD. In the full sample, adding one Democratic-nominated judge to the panel increases the reversal rate by one percentage point, with the reversal rate three and six percentage points higher for RDD and DDD panels (all significant at the 1 percent level). The difference in outcome from adding a single Democrat to a Republican-majority panel (or likewise RDD vs. DDD) is notable (see for example Revesz (1997)). The gradient is similar in magnitude pre- and post-2000 but is

stronger in published cases compared to unpublished cases, both pre- and post-2000. Note, however, that the base mean of reversal for published cases is higher than for unpublished cases, regardless of how we split the data. Reversal rates for published cases range from 35.5-37.4 percent, while they range from 9.75-11.5 percent for unpublished cases.

Turning to published vs. unpublished cases, in unpublished cases pre-2000, the RRD effect is not statistically significant, while the RDD and DDD are (1.4 and 4.4 percentage points, respectively). For published cases pre-2000, the gradient is slightly steeper, with RRD, RDD, and DDD panels increasing reversal rates by 1.6, 4, and 5.6 percentage points, respectively. The contrast between unpublished and published cases becomes even more pronounced post-2000. While the pattern in unpublished cases remains similar to the earlier period (1.4 and 3.4 percentage points for RDD and DDD), in published cases, the political composition variables trace out a much steeper gradient: RRD, RDD, and DDD panels increase reversals of Republican-nominated trial judges by 2.4, 6.5 and 13.3 percentage points, respectively (all significant at the 1 percent level).

We now turn to examine *polarization*, which we define as the alignment between the political affiliation of the appellate panel and that of the trial judge, captured by the interaction terms between appellate panel composition and the political affiliation of the trial judge (β_4 through β_6). This captures whether Democratic judges moderate their greater tendency to reverse cases (compared to Republican appellate judges) when the trial judge shares their political affiliation. This was previously investigated by Epstein et al. (2013) and Berdejó (2013), who find that the political affiliation of the trial court judge does not matter.[16] As Epstein et al. (2011) note, "[t]hat the ideological direction of the district court decision but not the ideological identity of the district judge influences the likelihood of a reversal makes sense." Given our extended and expanded time window, we revisit this question.

We first focus on reversals of Democratic-nominated trial judges by RRR panels (which serves as the omitted category and is reflected in the coefficient for Democratic trial judge, β_7). While we find no effect in the overall sample in Table [2], column 1, in the post-2000 period (column 3), a period in which politicization intensified, we find that RRR panels are one percentage point more likely to reverse Democratic-nominated trial judges. In columns 4 to 7, when unpacking by pre- vs. post-2000 and published vs. unpublished and by publication

[16] Berdejó (2013) finds that trial judge ideology, interacted with appellate panel composition, does not predict sentencing outcomes. Some evidence indicates that the ideological orientation of the trial court's decision may matter for how cases are treated on appeal.

status, we find that post-2000, the effect is more pronounced among published cases (3.5 percentage points, significant at the one percent level).

In contrast, DDD panels reverse Republican-nominated trial judges at a rate that is 6.3 percentage points higher than RRR panels in the full sample (β_3, column 1) and 13.3 percentage points higher in published cases post 2000 (β_3, column 7).

From column 7, DDD panels reverse Democratic-nominated trial judges at a rate of 9.8 percentage points ($\beta_3 + \beta_6 + \beta_7 = 13.3 - 7.0 + 3.5 = 9.8$) and reverse Republican-nominated trial judges at a rate of 13.3 percentage points (β_3). This implies that DDD panels are 3.5 percentage points more likely to reverse Republican-nominated trial judges than Democratic-nominated trial judges (given by $-\beta_3 - \beta_7 = 7.0 - 3.5 = 3.5$). Similarly, RRR panels reverse Democratic-nominated trial judges at a rate that is 3.5 percentage points, β_7, higher than Republican-nominated trial judges (the default group).

In other words, the 3.5 percentage point reversal disadvantage faced by Democratic-nominated trial judges under RRR panels is symmetrically offset for Republican trial judges under DDD panels, indicating a partisan symmetry in reversal behavior. (Note that the symmetry is not mechanical, since β_7 need not be equal to $-\beta_3 - \beta_7$.) Another view of differential reversal rates is to consider how a Republican or Democratic-nominated trial judge fares across RRR vs. DDD panels. Here, Democratic trial judges are much less likely to be reversed, because Democratic appellate judges are more likely to favor reversal on average (compared to Republican appellate judges) but at a much lower rate for Democratic-nominated trial judges ($\beta_6 = -7.0$ in column 7). This tends to favor Democratic trial judges over Republican trial judges.

Turning next to mixed panels, in Table [2], column 1, for the full sample, we do not find an effect of RRD or RDD panels (relative to RRR), but we do find that a fully Democratic panel is 1.6 percentage points less likely to overturn a Democratic trial judge, relative to a mean reversal rate of 17.9 percent. When we split the sample by time period, we find that this effect is concentrated in the post-2000 period. There are no effects pre-2000, while post-2000, an RDD panel is 1.1 percentage points less likely to reverse a Democratic judge, with a 2.2 percentage point effect for DDD panels (effects significant at 5 and 1 percent, respectively).[17]

[17]While the sample size is too small to present estimates by year, in Figure [B.2] we present the main coefficients of interest (RRD, RDD and DDD interacted with Trial Judge Dem) in rolling five-year windows from 1987 to 2018. For RRD, we find most of the significant effects are post-2000. For RDD, the first significant effect is in 1996 (i.e., the window spanning 1994-1998) and for DDD the first significant effect is

In columns 4 to 7, we further split by unpublished and published cases. This has no impact on pre-2000 results, which remain null. However, post-2000, we see that the results are driven by published cases. A single (minority) Democratic justice on the panel, RRD, reduces the reversal rate for a Democratic-nominated trial judge by 3.1 percentage points (significant at the 5 percent level). With a second Democratic-nominated appellate court judge, RDD, the effect increases to -4.4 percentage points, and for a DDD panel with three Democratic-nominated appellate judges, the effect is -7 percentage points. So, while panels with all Democratic-nominated judges are more likely to reverse trial court decisions than panels with all Republican-nominated judges when the trial judge is Republican-nominated, this effect is approximately halved if the trial judge is Democratic-nominated (i.e., a 13.3 percentage point increase in reversals from a DDD panel of a Republican trial court judge vs. 6.3 percentage points for a Democratic trial court judge).

Published cases have higher visibility and are more easily accessible to the public, legal professionals, media, and academia. This visibility means that decisions in these cases can have a broader impact and receive more attention. The combination of visibility, the potential for setting precedents, and the impact on regulations and legal doctrines makes published cases more critical for judges who wish to assert their ideological views and influence the legal landscape, specifically in a period of growing polarization. This consideration can lead judges to more frequently side with trial judges from their own political party in published cases.

Other effects of interest in Table [2] include a reduction in reversals for female trial court judges and an increase in reversals for minority trial court judges (-0.7 and 1.7 percentage points, respectively, both significant at the 1 percent level). These effects are similar pre- and post-2000, but the minority reversal effect is stronger in published cases post 2000. At the same time, panels with at least one female or minority-identity judge are somewhat less likely to reverse trial court decisions. In the next section, we investigate whether there is evidence of partisanship with respect to the female or minority composition of the appellate panel and the identity of trial court judge, or in other words whether gender or minority identity alignment between the appellate panel and trial court judge matters, as it does for political identity, and if so, which is more important when allowing for multiple channels of identity effects.

1998 (the 1996-2000 window). Overall, the effect is typically significant post-2000 for all three interactions.

In columns 8 to 10, we investigate whether our result on political partisanship, focusing specifically on post-2000 published cases, survives more stringent specifications, specifically controlling for additional panel and trial judge characteristics using fixed effects. In column 8, we include trial judge fixed effects, so we identify β_4 to β_6 from variation in the composition of the appellate panel for a given trial judge. Our effects are essentially unchanged from our main specification in column 7. In column 9, we instead control for Appellate Panel fixed effects to see whether for a given appellate panel the political affiliation of the trial judge matters. Again, our results are unchanged. Finally, in column 10, we include both sets of fixed effects and again find that our results for the interaction effects (β_4 to β_6) are unchanged.

To conclude, we have found evidence both for politicization and polarization of the judiciary: both the political composition of the appellate panel and the political alignment of the appellate panel with the trial judge affect the outcome. These results are present in the full sample, stronger post-2000, and strongest in post-2000 among published cases.

D. Interpretation and Extensions

While our use of the term polarization is in keeping with the literature, the phenomenon we call polarization is consistent both with polarization in the narrow sense of increasingly divergent viewpoints or legal philosophies between Republican and Democratic judges and with partisanship in the sense of like sticking with like, based on political identity. In order to investigate this further, in Table [3], columns 1 and 2, we split our results by ideological cases and non-ideological cases (as defined in Section 3), focusing on post-2000 published cases. For non-ideological cases, one would expect judicial philosophy to be less relevant as these cases deal with more technical matters, in which case whatever effect we observe would be due to partisanship rather than polarization in legal outlook. One would expect differences in judicial philosophy to express themselves more strongly in ideological cases. We find that our results are similar for both ideological and non-ideological cases, with differences that are not statistically significant, including notably the interaction effects. This suggests that the phenomenon we are observing is at least in part due to increased partisanship, although we cannot rule out that polarized legal or judicial philosophy could also play a role.

Since increased polarization is observed primarily post-2000, it raises the question of whether judges appointed prior to 2000 have become increasingly partisan or whether more partisan judges have been appointed to the bench post-2000. In Table [3] column 3, we

restrict the sample to judges who were nominated prior to 2000 and who remained active post-2000, i.e., excluding panels with judges who became inactive prior to 2000 or who were nominated post-2000.[18] We continue to find that RDD and DDD panels are significantly more likely to reverse trial rulings by Republicans (4.3 and 11.5 percentage points), with magnitudes similar to Table [2], column 7. This effect is essentially fully offset if the trial court judge was Democratic. We continue to find a large spread of approximately 8.1 percentage points ($\beta_3 - \beta_7 = 11.5 - 3.4 = 8.1$) in reversal rates between RRR panels of Democratic trial judges and DDD panels of Republic trial judges. Hence, our results suggest that, at least in part, the same judges have become more partisan over time. This does not preclude that the justices appointed post-2000 are also more partisan.

Thus far we would argue that our results suggest increased political polarization among appellate panels that reflects a change in judicial behavior over time and that this phenomenon is broad based, rather than confined only to contentious ideological cases.

At same time, we know that political affiliation is correlated with other elements of identity. While Figure [3] shows a relatively stable political composition of panels over time, in Figures [4] and [5], we see that the number of female and minority judges has increased over time. These newly appointed female and minority judges are disproportionately Democratic. As shown in Figure [6a], panels with more minority judges contain a larger share of Democrats, and a similar pattern holds for gender in Figure [6b], where panels with more women are also more heavily Democratic. Hence, it is possible that the interaction of appellate panel political composition and trial judge political affiliation could be picking up other elements of identity. We investigate in Tables [4] and [5].

In Table [4], we investigate whether a trial judge's gender (column 1) or minority identity (column 2) matters beyond political identity, specifically with respect to the political composition of the appellate panel. We do so by introducing interactions between these characteristics and the political composition of the appellate panel, in addition to the interactions with the political affiliation of the trial judge. In columns 1 and 2, we see that none of the interactions between the political composition of the appellate panel and the gender or minority identity of the trial judge are statistically significant, and likewise in

[18] While it would be interesting to slice this further and consider judges from more finely defined cohorts, we do not have the sample size for such an exercise. With panel fixed effects, we need three judges nominated from a particular Circuit and cohort to remain active and appear frequently enough post-2000, which does not occur sufficiently frequently to estimate our effect of interest with reasonable precision.

column 3 when we introduce both sets of interactions. It is notable that the direct effect of minority status remains positive: a minority trial judge is more likely to be reversed, but this does not seem to be related to the political composition of the appellate panel. Column 4 presents another test of the relevance of politics over and above gender or minority status by excluding observations where the trial judge is female or minority. We continue to effects comparable in magnitude to our results in Table [2].

Another channel through which gender or minority identity could affect reversal rates is through the gender and minority composition of the appellate panel and how it aligns with the identity of the trial judge. We investigate in Table [5].[19] In column 1, we introduce indicators for the gender composition of the appellate panel[20] and its interaction with whether the trial judge is female, focusing on the post-2000 published cases. The main effect of a female trial court judge is negative; they are 1.8 percentage points less likely to be reversed by the appellate panel. However, the gender composition of the panel does not have a significant interaction effect on this. In column 2, we investigate whether minority status matters. Minority trial judges are 1.8 percentage points more likely to be reversed. At the same time, minority appellate panels are more likely to reverse trial court decisions. With one minority judge the effect is 1.1 percentage points, and with two or three minority judges the effect is 3.6 percentage points. But the interaction of these two is negative and significant both statistically and in magnitude. Specifically, a panel with two or more minority judges is 6.6 percentage points less likely to reverse a trial court judge who is also from a minority group.

In columns 3 and 4 we add back in variables for the political identities of the panel and trial judges. This has no effect on our results for female judges; the political interactions remain significant and not much changed while the panel gender composition interactions remain insignificant. In contrast, in column 4, we find that when adding controls for political composition, the interaction effect between trial judge minority status and an indicator for a majority minority-identify panel remains negative, statistically significant, and meaningful in magnitude (4.9 percentage points). Thus our results indicate that there is polarization along both political identity and minority identity lines.

As a further robustness check, we would ideally consider a fully interacted specification

[19] Extended versions of these regressions are presented in Appendix Tables [A.1] to [A.5].

[20] We group together panels with two and three female judges since there are very few panels with three female judges.

with all interactions among political, gender, and minority identity variables both for the trial judge and the composition of the appellate panel. This, however, is challenging given the high degree of multicolinearity between political (Democratic) identity and minority status and gender. To address this challenge we employ a double-selection Lasso approach (Belloni et al., 2014). We include the political composition terms, add a full set of interactions of additional identity variables, and allow the data to choose the most important of these additional identity terms. (Details of this procedure, including the variable selection process and estimation procedure, are in Appendix C.) The results are presented in Table [6]. We present coefficients for our main effects of interest and the other identity interactions that are selected by the Lasso procedure. The core patterns remain unchanged. In the full sample, political composition effects are significant along with a negative effect of a DDD panel on reversals for Democratic trial judges. The effects are again most pronounced post-2000 in published cases, where magnitudes are similar to our main results. Consistent with Table [4], the Lasso does not select any interactions of gender composition of the panel or of the trial judge. However, the procedure continues to identify the relevance of minority above and beyond political identity. Interestingly, unlike Table [4], the procedure selects the interaction of minority composition of the appellate panel and the indicator for a Democratic trial judge. These interactions continue to be negative, statistically significant, and meaningful in magnitude (-1.7 percentage points for one minority-identity appellate panel member and -4.2 percentage point for two or more minority-identity panel members when facing a Democratic trial judge). Of course, most minority judges are Democratic, but evidently minority appellate panels are also less likely to reverse Democratic trial judges.

Overall, these results suggest that allowing for other dimensions of identity beyond politics does not erode the importance of political identity. Although gender identity does not seem to affect the reversal decision, we do find consistent evidence that minority status matters, over and above political identity.

5 Endogenizing the Publication Decision: Multivariate Probit and Lee Bounds

While we do find effects for politicization and polarization in the full sample, our results on the latter are most pronounced in the subsample of post-2000 published results. Our

justification for the pre- vs. post-2000 sample split is based both on prior literature (as cited above) and on the data (see Appendix B). In contrast, splitting the data by whether a case was published or not sidesteps potentially delicate questions of selection as it treats the reverse/affirm decision as separate from the decision to publish or not publish a case. This is particularly relevant if the two decisions are correlated through the political composition of the appellate panel and the political identity of the trial judge.

To address this concern, we consider two complementary approaches. First, since publication and reversal are simultaneous decisions by the appellate panel (see for example Wasby (2004) and Merritt and Brudney (2001)), we view them as potentially correlated outcomes and model them using a bivariate choice framework. Notably, the role of politicization and polarization in the joint publish/ reverse decision has to our knowledge not been explored in prior work.[21] Second, we can undo some of the selection into publication using Lee-style bounds. We present these approaches in turn.

A. Bivariate Probit

We consider two key decisions made by the appellate panel: whether to reverse or uphold the trial court decision, and whether to publish or not to publish the opinion. Since both decisions are part of the panel's deliberation and there is no evident sequencing of these choices, we model them as simultaneous, correlated decisions using a bivariate probit specification. We continue to split the data pre- and post-2000.

The specification includes the same covariates as our main regression in addition to our regressors of interest indicating political composition of the appellate panel. The covariates included are indicators for whether the trial judge is female, minority, chief, or senior; measures of the trial judge's tenure and its square; panel-level indicators for whether at least one female or minority judge is present on the panel; and the average tenure of the panel. We include fixed effects at the year and circuit level. (More granular fixed effects, e.g., Circuit $\times$ Year interactions or appeal-type fixed effects which are include in our main specification above, prove to be computationally intractable.)

Table [7] presents marginal coefficients for the two outcomes, split pre- and post-2000. Pre-2000 Democratic judges are more likely to chose to reverse and to publish cases. None of the interactions between appellate panel composition and political identity of the trial court

[21] Boyd (2015) uses a multinomial choice framework to examine the effect of the ideological difference between the trial court judge and the appellate panel on the specificity of the appellate court's decisions with respect to the district court, alongside the publication decision.

judge is significant, with the exception of the main effect for an indicator of a Democratic trial judge (hence corresponding to an RRR panel and a Democratic trial judge) which is negative for publication. Post-2000, we continue to find positive effects on reversal among Democratic appellate panels, and mirroring our main results the interactions between appellate panel composition and trial judge political affiliation are now negative and statistically significant both for reversal and publication. For the former the effects are statistically significantly negative for RDD and DDD panels and an indicator for a Democratic trial judge, and for the latter only the DDD interaction is significant.

While the coefficients on reverse and publish are useful to get a sense of the importance of political factors in the publication decision, alongside reversal, the bivariate probit also estimates the correlation across the two decisions, which we find to be positive and significant both in magnitude (approximately 0.5) and statistically. Using the the estimated coefficients and the correlation coefficient allows us to estimate the marginal effects of our variables of interest for the four joint outcomes: reversed and unpublished; not reversed and unpublished; reversed and unpublished; and reversed and published. Results are presented in Table [8]

In the pre-2000 period, we observe that increasing the number of Democratic-nominated judges on the appellate panel leads to a greater likelihood of a case being reversed. However, consistent with Table x, this politicization in reversal behavior is not confined to published cases; it is also observed for unpublished cases. The interaction terms between panel composition and trial judge political affiliation are generally small and not statistically significant, indicating that political alignment between the panel and the trial judge did not meaningfully affect outcomes in this earlier period. We find that RRR panels (the omitted category, hence reflected in the coefficient for Trial Judge Democratic) are more likely to not reverse and not publish, and less likely to reverse and not publish.

In contrast, while the post-2000 period reveals a similar pattern in the politicization effects, we find a striking shift in polarization. For the main effects, similar to pre-2000, we find that Democratic panels are more likely to reverse trial court decisions, both in published and unpublished cases. Instead, in contrast with the pre-2000 results, the interaction between the panel's political composition and the political affiliation of the trial judge becomes both negative and statistically significant and substantively large, both through reversed and published and reverse and unpublished cases. While the effect is not significant for one Democratic judge on the panel, it is for two and three-Democratic judge panels. We find

that RDD and DDD panels are more likely to choose reversals, whether published or unpublished, for Democratic trial court judges. RRR panels are more likely to publish and reverse Democratic trial judges, an effect which is similar to that for DDD panels and also significant at the one percent level. The effect is also present for published non-reversals, although only significant at the ten percent level. RRR panels are less likely to have unpublished non-reversals for Democratic trial judges.

To investigate the robustness of our findings, we also estimate multinomial probit and logit models using the same specification as in the bivariate probit. Table [A.6] presents the results for the multivariate probit. The results are broadly similar, with politicization appearing both pre- and post-2000, but the polarization effect observed only post-2000. Table [A.7] presents results from the multinomial logit specification, in which we can also control for Circuit $\times$ Year fixed effects. The results are similar to those of the bivraite and multinomial probit models. However, formal tests of the Independence of Irrelevant Alternatives (IIA) assumption using both the Hausman and McFadden (1984) and Small and Hsiao (1985) procedures, presented in Table [A.8], yield mixed and often contradictory results. This is not surprising and is well documented in the literature.[22]

A final supplemental result is presented in Appendix Table [A.9] where reproduce Table [2] using *Published* as the dependent variable rather than *Reversed*. The results mirror our main results in the expected direction, albeit with a weaker pattern. Post-2000, DDD panels are less likely to publish decisions when the trial judge is also Democratic (column 2, a one percentage point effect, significant at the 10 percent level). Focusing on the subset of reversed cases post-2000, column 7, this pattern is much stronger: it is sizable and significant for both RDD and DDD panels (2.7 and 4 percentage points respectively, both significant at the five percent level). As discussed above, we believe that a multivariate approach offers a more principled solution to the concern of simultaneity of the publication and reversal decisions, since there is no evident sequencing of these.

B. Lee-Style Bounds

An alternative approach for addressing sample selection when focusing on reversal vs. non-reversal among published cases is to use Lee (2009)-style bounds. Lee's approach,

[22]See, for example, the discussion in Fry and Harris (1996), Fry and Harris (1998), Cheng and Long (2007) and subsequent simulation-based evaluations, which show that both the Hausman-McFadden and Small-Hsiao tests can suffer from severe size distortions and produce inconsistent outcomes across different implementations, particularly depending on the structure of the dependent variable.

adapted to our setting, involves first estimating the probability that a case is published (vs. unpublished) in the full sample, and then based on that estimate trimming the full sample to a range where one is focused on cases without extremely low or high probabilities of publication (e.g., 25th to 75th percentile or 20/80 or 10/90). Within this trimmed sample, we then rerun our original specification. These results are presented in Table [9], focusing just on the interaction effects from Table [2] and the post-2000 sample. Column 1 presents results for the full sample post-2000 sample and column 2 for the subset of published cases post-2000. These results mirror those in Table [2], column 10. When we trim the sample to the inter-quartile range of estimated probability of publication, our results are very similar and indeed larger in magnitude. Trimming to the 20-80 range in column 3 and the 10-90 range in column 4 again leads to similar results.

In conclusion, the Lee-style bounds, like our multivariate probit results, suggest that our previous findings on political polarization are not driven by selection into publication.

6 Conclusion

We have documented a significant increase in the politicization and polarization of decisions of U.S. Circuit Court appellate panels. The politicization effect is broad, appearing across the entire sample period and in both published and unpublished cases. However, the effect is notably more pronounced among published cases post-2000, consistent with evidence pointing to a rise in politicization around the year 2000. While politicization has been previously documented in the literature for a limited set of published cases pertaining to ideologically salient issues, we show that it extends across the full period we examine, for both published and unpublished cases, as well as both ideological and non-ideological cases. We also provide new results on political polarization, the lack of effects based on gender-identity polarization, a significant polarization effect based on minority identity, and the effect of politics on the joint decision of whether to publish (not publish) or reverse (affirm) a case.

When examining polarization, captured by the interaction between the political composition of the appellate panel and the political affiliation of the trial judge, we find effects in the full-sample but these are stronger in post-2000 published cases. Turning to identity, we investigate two other potentially salient dimensions: gender and minority status. For

gender, we do not observe any effects over and above politics. In contrast, for minority status, we find consistent evidence that, in published cases post 2000, panels with a majority of minority-identity judges are less likely to reverse decisions by minority trial court judges.

We investigate whether the decision to publish a case is itself politicized or polarized. This serves both as a robustness check and as a substantive question for our polarization results, which are driven by the sample of published cases. In the pre-2000 period, the main effect we find of politics on publication is the decision to reverse a case, which is more favored by Democratic-leaning panels. Post-2000, we find that politics matters in two ways. Democratic leaning panels are more likely to choose to reverse trial court judges, whether through published or unpublished cases. Democratic-leaning panels are less likely to reverse a Democratic trial judge (compared to a Republican trial judge) in both published and unpublished cases. This polarization effect attenuates (between one third and almost fully) Democratic judges' tilt toward reversing cases, when they face a Democratic rather than Republican trial judge.

The terms politicization and polarization are often used in a normatively negative sense. Instead, this paper uses these terms in a descriptive sense. Differing political viewpoints imply different judicial philosophies, and there is no sense in which the different reversal rates reflect incorrect decisions. Similarly, the fact that appellate panels are more likely to side with trial judges of the same political affiliation could reflect increasing within-party alignment of increasingly divergent political and judicial philosophies across the two parties, and this in turn could stem from shifts in either appellate court or trial court judges' behavior. Of course, to the extent that trial court decisions have been increasingly politicized, post-2000 the process of appellate review does not attenuate this to the same extent.

There are two mechanisms that could underlie our result: changes in behavior among trial judges and changes in behavior among Circuit Court judges. Increasingly polarized trial court judges could reach decisions more closely aligned with their Circuit Court, leading to lower reversal rates along political lines, or Circuit Court judges could exhibit greater affinity toward judges of their own political affiliation and become less likely to reverse their decisions. Since trial court judges are not certain whether their rulings will be appealed and subsequently published, we would expect to see the former effect, if it were present, in both published and unpublished cases. Since we are seeing the polarization effect only in published cases, it suggests that the latter mechanism is more pronounced.

Our results are significant for several reasons. First, they imply increased uncertainty around court decision-making, with the outcomes of appeals hanging in the balance based on the coin toss of judicial assignment to panels. Second, they underline that political trends in judicial appointments have a significant impact on the behavior of courts. Third, they offer a unique window into the increased politicization and polarization of decision making in a setting that has a broad, profound, and lasting impact.

In future work, we plan to investigate the interactions of judges in more detail, in particular to examine whether there is strategic behavior or history-dependent decision making, and whether other dimensions of affinity among justices (beyond politics, sex, and race, which we have explored in this paper) affect their decisions.

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Tables and Figures

Table 1: Summary Statistics

Variable	All Cases			Trial Judge		Unpublished		Published	
	All	Pre-2000	Post-2000	Republican	Democrat	Pre-2000	Post-2000	Pre-2000	Post-2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Case Characteristics									
Reversal	0.18	0.19	0.17	0.17	0.19	0.10	0.12	0.35	0.37
	(0.38)	(0.39)	(0.38)	(0.38)	(0.39)	(0.30)	(0.32)	(0.48)	(0.48)
Published	0.28	0.35	0.22	0.27	0.29				
	(0.45)	(0.48)	(0.41)	(0.45)	(0.45)				
Dissent	0.03	0.03	0.03	0.03	0.03	0.01	0.01	0.08	0.09
	(0.17)	(0.18)	(0.16)	(0.17)	(0.18)	(0.10)	(0.11)	(0.27)	(0.28)
Ideology	0.37	0.43	0.32	0.38	0.36	0.39	0.28	0.51	0.48
	(0.48)	(0.50)	(0.47)	(0.48)	(0.48)	(0.49)	(0.45)	(0.50)	(0.50)
Civil	0.41	0.44	0.39	0.40	0.44	0.38	0.35	0.56	0.54
	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)	(0.48)	(0.48)	(0.50)	(0.50)
Criminal	0.33	0.29	0.35	0.34	0.30	0.30	0.37	0.28	0.30
	(0.47)	(0.45)	(0.48)	(0.47)	(0.46)	(0.46)	(0.48)	(0.45)	(0.46)

Variable	All Cases			Trial Judge		Unpublished		Published	
	All	Pre-2000	Post-2000	Republican	Democrat	Pre-2000	Post-2000	Pre-2000	Post-2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel B: Panel Characteristics									
RRR	0.18	0.24	0.13	0.18	0.17	0.23	0.11	0.25	0.18
	(0.38)	(0.43)	(0.34)	(0.39)	(0.37)	(0.42)	(0.32)	(0.43)	(0.39)
RRD	0.40	0.44	0.36	0.40	0.39	0.45	0.35	0.44	0.39
	(0.49)	(0.50)	(0.48)	(0.49)	(0.49)	(0.50)	(0.48)	(0.50)	(0.49)
RDD	0.33	0.26	0.38	0.32	0.33	0.26	0.39	0.26	0.32
	(0.47)	(0.44)	(0.48)	(0.47)	(0.47)	(0.44)	(0.49)	(0.44)	(0.47)
DDD	0.10	0.06	0.14	0.10	0.11	0.06	0.15	0.06	0.11
	(0.30)	(0.23)	(0.34)	(0.30)	(0.31)	(0.23)	(0.35)	(0.23)	(0.31)
MMM	0.51	0.66	0.39	0.52	0.49	0.61	0.38	0.74	0.40
	(0.50)	(0.47)	(0.49)	(0.50)	(0.50)	(0.49)	(0.49)	(0.44)	(0.49)
MMF	0.39	0.30	0.47	0.39	0.40	0.34	0.47	0.24	0.46
	(0.49)	(0.46)	(0.50)	(0.49)	(0.49)	(0.47)	(0.50)	(0.43)	(0.50)
MFF	0.09	0.04	0.13	0.09	0.10	0.05	0.14	0.02	0.13
	(0.29)	(0.19)	(0.34)	(0.28)	(0.30)	(0.21)	(0.34)	(0.15)	(0.33)
FFF	0.01	0.00	0.01	0.01	0.01	0.00	0.01	0.00	0.01
	(0.08)	(0.03)	(0.11)	(0.08)	(0.09)	(0.03)	(0.11)	(0.03)	(0.10)
NNN	0.66	0.78	0.57	0.67	0.65	0.79	0.56	0.78	0.63
	(0.47)	(0.41)	(0.49)	(0.47)	(0.48)	(0.41)	(0.50)	(0.41)	(0.48)
NNMin	0.29	0.20	0.36	0.28	0.30	0.20	0.37	0.21	0.33
	(0.45)	(0.40)	(0.48)	(0.45)	(0.46)	(0.40)	(0.48)	(0.40)	(0.47)
NMinMin	0.04	0.02	0.06	0.04	0.05	0.02	0.07	0.01	0.05
	(0.20)	(0.12)	(0.25)	(0.20)	(0.21)	(0.13)	(0.25)	(0.12)	(0.21)
MinMinMin	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00
	(0.05)	(0.02)	(0.07)	(0.05)	(0.06)	(0.02)	(0.07)	(0.01)	(0.05)
Panel Age	64.10	62.27	65.52	63.91	64.36	62.02	65.39	62.75	65.99
	(6.13)	(5.73)	(6.05)	(6.07)	(6.20)	(5.80)	(6.13)	(5.55)	(5.73)
Panel Tenure	16.30	14.35	17.83	16.07	16.64	14.21	17.56	14.62	18.77
	(6.04)	(4.81)	(6.45)	(5.88)	(6.25)	(4.75)	(6.41)	(4.89)	(6.51)

Variable	All Cases			Trial Judge		Unpublished		Published	
	All	Pre-2000	Post-2000	Republican	Democrat	Pre-2000	Post-2000	Pre-2000	Post-2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel C: Trial Judge Characteristics									
TJ Democrat	0.41	0.36	0.45	0.00	1.00	0.35	0.44	0.38	0.48
	(0.49)	(0.48)	(0.50)	(0.00)	(0.00)	(0.48)	(0.50)	(0.48)	(0.50)
TJ Female	0.17	0.11	0.22	0.11	0.26	0.11	0.22	0.11	0.22
	(0.37)	(0.31)	(0.41)	(0.31)	(0.44)	(0.32)	(0.41)	(0.31)	(0.41)
TJ Minority	0.14	0.11	0.17	0.09	0.21	0.11	0.17	0.11	0.17
	(0.35)	(0.31)	(0.37)	(0.29)	(0.41)	(0.32)	(0.37)	(0.31)	(0.38)
TJ Chief	0.15	0.17	0.14	0.16	0.14	0.17	0.14	0.18	0.14
	(0.36)	(0.38)	(0.35)	(0.37)	(0.34)	(0.37)	(0.35)	(0.38)	(0.35)
TJ Senior	0.27	0.23	0.31	0.26	0.29	0.24	0.31	0.22	0.30
	(0.45)	(0.42)	(0.46)	(0.44)	(0.45)	(0.43)	(0.46)	(0.42)	(0.46)
TJ Age	63.18	61.13	64.78	62.75	63.80	61.21	64.81	61.00	64.68
	(9.92)	(9.57)	(9.90)	(9.90)	(9.92)	(9.58)	(9.92)	(9.55)	(9.81)
TJ Tenure	13.77	11.85	15.27	13.69	13.88	11.97	15.20	11.62	15.55
	(8.74)	(7.31)	(9.44)	(8.49)	(9.08)	(7.30)	(9.48)	(7.32)	(9.29)
N	400526	175866	224660	236532	163994	113689	175335	62177	49325

Table 2: The Effect of Politics and Partisanship on the Reversal Decision

VARIABLES	(1) All Cases	(2) All Cases Pre-2000	(3) All Cases Post-2000	(4) Unpublished Cases Pre-2000	(5) Unpublished Cases Post-2000	(6) Published Cases Pre-2000	(7) Published Cases Post-2000	(8) Trial Judge Fixed Effects	(9) Panel Fixed Effects	(10) Trial Judge + Panel Fixed Effects
RRD	0.010***	0.011***	0.007	0.005	0.002	0.016**	0.024***	0.025***		
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.007)	(0.009)	(0.009)		
RDD	0.031***	0.032***	0.027***	0.014***	0.014***	0.040***	0.065***	0.064***		
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.010)	(0.010)		
DDD	0.063***	0.065***	0.058***	0.044***	0.034***	0.056***	0.133***	0.136***		
	(0.005)	(0.009)	(0.007)	(0.007)	(0.006)	(0.014)	(0.014)	(0.015)		
RRD x TJ Dem	-0.005	-0.004	-0.006	-0.001	0.004	-0.006	-0.031**	-0.035***	-0.033**	-0.038**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.009)	(0.012)	(0.012)	(0.014)	(0.016)
RDD x TJ Dem	-0.005	0.003	-0.011**	0.006	0.002	-0.010	-0.044***	-0.047***	-0.044***	-0.047***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.005)	(0.012)	(0.012)	(0.013)	(0.015)	(0.018)
DDD x TJ Dem	-0.016***	-0.006	-0.022***	0.001	-0.005	-0.026	-0.070***	-0.074***	-0.071***	-0.072***
	(0.006)	(0.010)	(0.007)	(0.010)	(0.008)	(0.018)	(0.018)	(0.018)	(0.023)	(0.025)
TJ Democrat	0.003	-0.002	0.010**	-0.005	-0.002	0.007	0.035***		0.036***	
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.008)	(0.010)		(0.012)	
TJ Female	-0.007***	-0.006**	-0.007***	-0.005	-0.006***	-0.005	-0.010*		-0.011	
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.006)	(0.005)		(0.007)	
TJ Minority	0.017***	0.015***	0.018***	0.014***	0.016***	0.024***	0.032***		0.038***	
	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.007)	(0.006)		(0.008)	
TJ Chief	-0.005**	-0.006**	-0.004	-0.002	-0.003	-0.009*	-0.010	-0.005	-0.005	0.003
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)
TJ Senior	0.001	-0.002	0.003	-0.005	0.003	0.004	-0.003	0.002	-0.004	0.007
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.007)	(0.007)	(0.009)	(0.008)	(0.011)
TJ Tenure	0.001***	0.002***	0.000	0.002***	-0.000	0.001	0.000	-0.013	-0.001	-0.030**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.010)	(0.001)	(0.013)
TJ Tenure^2	0.000**	-0.000	0.000***	-0.000	0.000***	0.000	0.000*	0.000	0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel Female > 0	-0.004**	-0.007*	-0.002	-0.003	0.002	-0.004	-0.007	-0.006		
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.006)	(0.005)	(0.006)		
Panel Minority > 0	-0.007***	-0.015***	-0.004	-0.008***	-0.003	-0.017***	0.003	0.004		
	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.006)	(0.005)	(0.005)		
Panel Tenure	0.000**	0.001***	-0.000	0.000	-0.000	0.002***	-0.000	-0.000	-0.039	-0.153
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.278)	(0.278)
Observations	400,513	175,856	224,648	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.055	0.064	0.050	0.026	0.035	0.050	0.061	0.093	0.279	0.312
Prior Judge FE	No	No	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.188	0.172	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: The table presents results from regressions with *Reversal* as the dependent variable, with specifications as specified in the column headers and notes. Standard errors are in parentheses and are clustered by Circuit×Year. All regressions also include Circuit×Year, District, Appeal Type, Nature of the suit (for Civil cases), and Offense Type (for Criminal cases) fixed effects. Coefficient estimates and standard errors are listed with stars indicating statistical significance (* p<0.1, ** p<0.05, *** p<0.01). The coefficients of interest are those on the political composition of the appellate panel (RDD, RRD, and DDD) and their interaction with the political affiliation of the trial court judge.

Table 3: Robustness Checks for Reversal Decision

VARIABLES	(1) Ideological Cases	(2) Non-Ideological Cases	(3) Only Include Trial Judges Nominated Before 2000
RRD	0.026**	0.020*	0.007
	(0.012)	(0.011)	(0.014)
RDD	0.080***	0.047***	0.043***
	(0.014)	(0.012)	(0.016)
DDD	0.139***	0.120***	0.115***
	(0.019)	(0.018)	(0.021)
RRD x TJ Dem	-0.027	-0.029*	-0.025
	(0.017)	(0.016)	(0.020)
RDD x TJ Dem	-0.048**	-0.033**	-0.046**
	(0.019)	(0.016)	(0.022)
DDD x TJ Dem	-0.051**	-0.080***	-0.101***
	(0.026)	(0.023)	(0.029)
TJ Democrat	0.025	0.034**	0.034**
	(0.016)	(0.014)	(0.017)
TJ Female	-0.017**	-0.010	-0.012
	(0.008)	(0.007)	(0.008)
TJ Minority	0.034***	0.025***	0.026**
	(0.009)	(0.009)	(0.010)
Observations	23,850	25,426	20,076
R-squared	0.074	0.063	0.067
Mean Dep Var.	0.377	0.370	0.367

Notes: The table presents robustness checks for our main specification, with results from regressions with *Reversal* as the dependent variable, with specifications as specified in the column headers and notes. Standard errors are in parentheses and are clustered by Circuit×Year. All regressions also include Circuit × Year, District, Appeal Type, Nature of the suit (for Civil cases), and Offense Type (for Criminal cases) fixed effects. Coefficient estimates and standard errors are listed with stars depicting statistical significance (* p<0.1, ** p<0.05, *** p<0.01). Our main effects of interest are similar.

Table 4: Further Robustness Checks for Reversal Decision

VARIABLES	(1) Add Trial Judge Female Interactions Only	(2) Add Trial Judge Minority Interactions Only	(3) Add Trial Judge Female and Minority Interactions	(4) Excluding Obs. where Trial Judges Female or Minority
RRD	0.025***	0.023***	0.024***	0.024**
	(0.009)	(0.009)	(0.009)	(0.009)
RDD	0.066***	0.065***	0.067***	0.071***
	(0.009)	(0.010)	(0.010)	(0.010)
DDD	0.132***	0.133***	0.133***	0.141***
	(0.014)	(0.014)	(0.014)	(0.014)
RRD x TJ Dem	-0.029**	-0.030**	-0.029**	-0.026*
	(0.012)	(0.012)	(0.013)	(0.015)
RDD x TJ Dem	-0.040***	-0.043***	-0.040***	-0.044***
	(0.012)	(0.012)	(0.012)	(0.015)
DDD x TJ Dem	-0.068***	-0.068***	-0.068***	-0.078***
	(0.019)	(0.018)	(0.019)	(0.022)
TJ Democrat	0.031***	0.032***	0.030***	0.024*
	(0.010)	(0.011)	(0.011)	(0.012)
TJ Female	-0.005	-0.014***	-0.005	
	(0.012)	(0.005)	(0.012)	
TJ Minority	0.029***	0.030**	0.029**	
	(0.006)	(0.013)	(0.013)	
RRD x TJ Fem	-0.007		-0.007	
	(0.014)		(0.014)	
RDD x TJ Fem	-0.017		-0.016	
	(0.014)		(0.014)	
DDD x TJ Fem	-0.003		-0.003	
	(0.020)		(0.020)	
RRD x TJ Min		0.005	0.005	
		(0.016)	(0.016)	
RDD x TJ Min		-0.007	-0.006	
		(0.017)	(0.017)	
DDD x TJ Min		-0.006	-0.006	
		(0.020)	(0.020)	
Observations	49,295	49,295	49,295	32,670
R-squared	0.060	0.060	0.060	0.064
Mean Dep Var.	0.374	0.374	0.374	0.371

Notes: The table presents robustness checks for our main specification, with results from regressions with *Reversal* as the dependent variable, with specifications as specified in the column headers and notes. Standard errors are in parentheses and are clustered by Circuit×Year. All regressions also include Circuit×Year, District, Appeal Type, Nature of the suit (for Civil cases), and Offense Type (for Criminal cases) fixed effects. Coefficient estimates and standard errors are listed with stars depicting statistical significance (* p<0.1, ** p<0.05, *** p<0.01). Our main effects of interest are similar.

Table 5: The Effect of Minority and Gender Identity on the Reversal Decision

	(1) Female	(2) Minorities	(3) Female	(4) Minorities
A. Panel Composition				
RRD	.	.	.0263***	.0211**
	.	.	(.0091)	(.0092)
RDD	.	.	.0698***	.0613***
	.	.	(.0101)	(.0102)
DDD	.	.	.137***	.1271***
	.	.	(.0143)	(.0141)
MMF	.001	.	-.0114	.
	(.0056)	.	(.0069)	.
MFF+FFF	-.0005	.	-.0215**	.
	(.008)	.	(.0102)	.
NNMin	.	.0107*	.	.0143*
	.	(.0058)	.	(.0073)
NMinMin+MinMinMin	.	.0355***	.	.0328**
	.	(.0136)	.	(.0159)
B. Judge Characteristics				
TJ Democrat	.0004	.0003	.0311***	.0389***
	(.0054)	(.0054)	(.0102)	(.0106)
TJ Female	-.0178**	-.0101**	-.0091	-.0094*
	(.0079)	(.0051)	(.0128)	(.0052)
TJ Minority	.0331***	.0341***	.0318***	.0304**
	(.0063)	(.0081)	(.0064)	(.0139)
1. Interaction with Trial Judge Political Affiliation				
RRD x TJ Dem	.	.	-.0301**	-.0244**
	.	.	(.0125)	(.0123)
RDD x TJ Dem	.	.	-.0433***	-.0326***
	.	.	(.0126)	(.0123)
DDD x TJ Dem	.	.	-.0721***	-.055***
	.	.	(.0192)	(.018)
(NNMin) x TJ Dem	.	.	.	-.0307***
	.	.	.	(.0093)
(NMinMin + MinMinMin) x TJ Dem	.	.	.	-.039*
	.	.	.	(.021)
(FMM) x TJ Dem	.	.	.0056	.
	.	.	(.0094)	.
(FFM + FFF) x TJ Dem	.	.	.008	.
	.	.	(.0139)	.
2. Interaction with Trial Judge Gender				
RRD x TJ Fem	.	.	-.0081	.
	.	.	(.0138)	.
RDD x TJ Fem	.	.	-.0196	.
	.	.	(.0146)	.
DDD x TJ Fem	.	.	-.0069	.
	.	.	(.0199)	.
(FMM) x TJ Fem	.0134	.	.0162	.
	(.0106)	.	(.0108)	.
(FFM + FFF) x TJ Fem	.0104	.	.0148	.
	(.0139)	.	(.0142)	.
3. Interaction with Trial Judge Minority				
RRD x TJ Min	.	.	.	.0034
	.	.	.	(.0163)
RDD x TJ Min	.	.	.	-.0077
	.	.	.	(.0169)
DDD x TJ Min	.	.	.	-.0059
	.	.	.	(.0205)
(NNMin) x TJ Min	.	.0075	.	.0186
	.	(.0134)	.	(.0137)
(NMinMin + MinMinMin) x TJ Min	.	-.0662**	.	-.0493*
	.	(.0276)	.	(.0282)
Observations	49304	49304	49304	49304
R-Squared	.0576	.0578	.0609	.0612
Mean Dep. Var.	.3735	.3735	.3735	.3735

Notes: All specifications correspond to the sample of *published* cases, post-2000. All specifications control for panel average tenure and its square, and for chief and seniority status of the trial judge; the coefficients are omitted for parsimony. We include dummies for female and minority presence on the panel, when regressors do not consider female/minority composition. Standard errors are clustered at the Circuit×Year level; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Column (1) maps to column (5) in Table A2; column (2) maps to column (5) in Table A3; column (3) maps to column (5) in Table A4; column (4) maps to column (5) in Table A5.

Table 6: Lasso Variable Selection among Gender and Identity Interactions

VARIABLES	(1) All Cases	(2) All Cases Pre-2000	(3) All Cases Post-2000	(4) Unpublished Cases Pre-2000	(5) Unpublished Cases Post-2000	(6) Published Cases Pre-2000	(7) Published Cases Post-2000	(8) Trial Judge Fixed Effects	(9) Panel Fixed Effects	(10) Trial Judge + Panel Fixed Effects
RRD	0.010***	0.011***	0.006	0.004	0.000	0.017**	0.023**	0.023**		
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.007)	(0.009)	(0.010)		
RDD	0.030***	0.030***	0.026***	0.012**	0.012**	0.042***	0.066***	0.064***		
	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.009)	(0.011)	(0.011)		
DDD	0.062***	0.061***	0.057***	0.039***	0.032***	0.054***	0.132***	0.135***		
	(0.005)	(0.008)	(0.007)	(0.007)	(0.006)	(0.014)	(0.014)	(0.015)		
RRD x TJ Dem	-0.004	-0.001	-0.006	-0.001	0.002	-0.002	-0.024*	-0.029**	-0.027*	-0.033*
	(0.004)	(0.005)	(0.006)	(0.004)	(0.006)	(0.009)	(0.013)	(0.013)	(0.016)	(0.017)
RDD x TJ Dem	-0.003	0.004	-0.009	0.004	0.001	-0.004	-0.032**	-0.036**	-0.034**	-0.039**
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.013)	(0.014)	(0.017)	(0.019)
DDD x TJ Dem	-0.015**	-0.008	-0.020**	-0.003	-0.005	-0.025	-0.058***	-0.061***	-0.064**	-0.065**
	(0.006)	(0.010)	(0.008)	(0.009)	(0.008)	(0.019)	(0.019)	(0.020)	(0.025)	(0.027)
TJ Democrat	0.004	-0.002	0.011**	-0.003	0.000	0.005	0.035***		0.034***	
	(0.003)	(0.004)	(0.006)	(0.004)	(0.005)	(0.008)	(0.011)		(0.013)	
(NMinMin + MinMinMin) x TJ Dem	0.001		0.002		0.009		-0.042**	-0.053**	-0.048*	-0.053**
	(0.007)		(0.007)		(0.007)		(0.021)	(0.023)	(0.026)	(0.027)
(NMinMin + MinMinMin) x TJ Fem	-0.003		-0.002	-0.010		-0.070				
	(0.007)		(0.007)	(0.017)		(0.045)				
(NMinMin + MinMinMin) x TJ Min	-0.011		-0.014	-0.017						
	(0.008)		(0.009)	(0.020)						
(NNMin) x TJ Dem	-0.005*	-0.011**	-0.004	-0.008*	0.003	-0.017*	-0.028***	-0.032***	-0.023*	-0.027**
	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)
(NNMin) x TJ Fem	0.001		-0.001	0.010		-0.005	-0.015	-0.010	-0.009	
	(0.004)		(0.004)	(0.007)		(0.015)	(0.011)	(0.011)	(0.014)	
(NNMin) x TJ Min	0.000	-0.002	-0.001		0.001		0.025*		0.018	
	(0.004)	(0.007)	(0.005)		(0.004)		(0.013)		(0.017)	
Observations	400,513	175,856	224,648	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.003	0.003	0.003	0.002	0.003	0.002	0.006	0.004	0.003	0.001
Prior Judge FE	No	No	No	No	No	No	No	Yes	Yes	Yes
Panel ID FE	No	No	No	No	No	No	No	No	No	Yes
Mean Dep Var.	0	0	0	0	0	0	0	0	0	0
# Lasso Controls	25	17	25	20	20	20	22	17	16	13

Notes: Standard errors clustered at the Circuit×Year level. All specifications include the usual FE and covariates.

Table 7: Marginal Effects - Single Equations

	Pre-2000		Post-2000	
	Reversed	Published	Reversed	Published
	(1)	(2)	(3)	(4)
RRD				
	.0089*	.0042	.0059	-.0069
	(.0051)	(.0105)	(.005)	(.0074)
	[.0822]	[.6917]	[.2407]	[.3532]
RDD				
	.0276***	.0257*	.0224***	-.0082
	(.0066)	(.0132)	(.006)	(.0085)
	[0]	[.052]	[.0002]	[.3349]
DDD				
	.0568***	.0444**	.0532***	-.0024
	(.0111)	(.0219)	(.0079)	(.011)
	[0]	[.0429]	[0]	[.8298]
RRD x TJ Dem				
	-.0044	-.0046	-.0072	-.0026
	(.0051)	(.0073)	(.0053)	(.0057)
	[.3808]	[.5324]	[.1804]	[.6437]
RDD x TJ Dem				
	.001	.0077	-.0124**	-.0074
	(.0059)	(.0081)	(.0053)	(.0058)
	[.8712]	[.3449]	[.0196]	[.2033]
DDD x TJ Dem				
	-.0073	.0075	-.0228***	-.0146**
	(.0087)	(.0131)	(.0067)	(.0067)
	[.403]	[.5694]	[.0006]	[.0284]
TJ Democrat				
	-.0006	-.014**	.0137***	.0159***
	(.0046)	(.0066)	(.0051)	(.0055)
	[.8913]	[.0344]	[.0073]	[.0038]
$\widehat{Prob}$	.181	.324	.166	.194
	N = 175866		N = 224660	
	$\hat{\rho} = .521$		$\hat{\rho} = .507$	
	$\widehat{SE(\rho)} = (.0097)$		$\widehat{SE(\rho)} = (.0084)$	

Notes: The first row under each outcome represents the marginal effect at means estimated using the `margins` Stata command, after running a biprobit regression including fixed effects at the Circuit, Year and Appeal type level, and all usual covariates. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit×Year level), and the third row provides the associated p-values (in square brackets). The $\widehat{Prob}$ row shows the conditional probability of each outcome, divided by single equations, fixed at the means of all the covariates.

Table 8: Marginal Effects Bivariate Probit

	Pre-2000				Post-2000			
	Unpublished		Published		Unpublished		Published	
Outcome	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RRD								
	-.0076	.0035*	-.0013	.0054	.0011	.0058*	-.007	.0001
	(.0101)	(.0019)	(.0067)	(.0045)	(.0076)	(.003)	(.0048)	(.0032)
	[.4519]	[.0618]	[.8491]	[.2252]	[.8894]	[.0523]	[.1487]	[.9741]
RDD								
	-.0336***	.0078***	.006	.0198***	-.009	.0172***	-.0133**	.0051
	(.0129)	(.0023)	(.0082)	(.0059)	(.0092)	(.0031)	(.0052)	(.0039)
	[.009]	[.0006]	[.4653]	[.0008]	[.3248]	[0]	[.01]	[.1883]
DDD								
	-.0616***	.0172***	.0048	.0396***	-.0348***	.0371***	-.0185***	.0161***
	(.0205)	(.0044)	(.0131)	(.0104)	(.0117)	(.0042)	(.0062)	(.0054)
	[.0026]	[.0001]	[.714]	[.0001]	[.0029]	[0]	[.003]	[.0031]
RRD x TJ Dem								
	.0058	-.0012	-.0014	-.0032	.0067	-.0041	.0005	-.0031
	(.0071)	(.0024)	(.0055)	(.0034)	(.006)	(.0035)	(.0042)	(.0026)
	[.4115]	[.6225]	[.8054]	[.3442]	[.2674]	[.2491]	[.9105]	[.2294]
RDD x TJ Dem								
	-.0065	-.0012	.0056	.0021	.0137**	-.0063*	-.0014	-.0061**
	(.0079)	(.0028)	(.006)	(.004)	(.0064)	(.0033)	(.0041)	(.0027)
	[.4094]	[.6698]	[.3572]	[.5941]	[.0333]	[.0585]	[.7385]	[.0248]
DDD x TJ Dem								
	-.0024	-.0051	.0097	-.0022	.0261***	-.0115***	-.0033	-.0113***
	(.0119)	(.0045)	(.0107)	(.0056)	(.0075)	(.0043)	(.005)	(.0031)
	[.8404]	[.2643]	[.3674]	[.694]	[.0005]	[.0083]	[.5078]	[.0002]
TJ Democrat								
	.0113*	.0027	-.0107**	-.0033	-.0207***	.0048	.007*	.0088***
	(.0061)	(.0024)	(.0052)	(.003)	(.0058)	(.0034)	(.004)	(.0025)
	[.0649]	[.2623]	[.0398]	[.2631]	[.0003]	[.1504]	[.078]	[.0004]
$\widehat{Prob}$	.609	.066	.21	.114	.715	.091	.119	.075
	N = 175866				N = 224660			
	$\hat{\rho} = .521$				$\hat{\rho} = .507$			
	$\hat{SE}(\rho) = (.0097)$				$\hat{SE}(\rho) = (.0084)$			

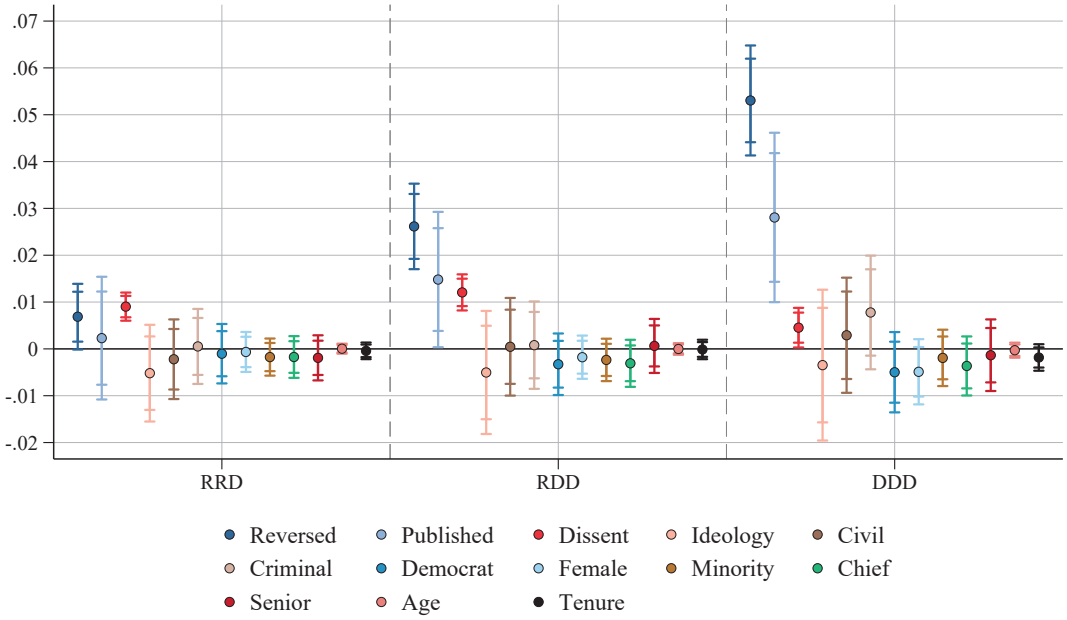
Notes: The first row under each outcome represents the marginal effect at means estimated using the `margins` Stata command, after running a biprobit regression including fixed effects at the Circuit, Year and Appeal type level, and all usual covariates. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit×Year level), and the third row provides the associated p-values (in square brackets). The penultimate row shows the conditional probability of each outcome fixed at the means of all the covariates.

Table 9: Lee Bounds for Publication with Reversal as the Dependent Variable

VARIABLES	(1) Non Trimmed	(2) Trimmed p25-p75	(3) Trimmed p20-p80	(4) Trimmed p10-p90
RRD x TJ Dem	-0.0379**	-0.0784**	-0.0739***	-0.0490**
	(0.0158)	(0.0304)	(0.0243)	(0.0210)
RDD x TJ Dem	-0.0470***	-0.0910***	-0.0802***	-0.0543**
	(0.0179)	(0.0331)	(0.0279)	(0.0234)
DDD x TJ Dem	-0.0724***	-0.112**	-0.101**	-0.0732**
	(0.0253)	(0.0492)	(0.0410)	(0.0314)
Observations	44,969	20,104	25,018	34,916
R-squared	0.312	0.405	0.379	0.336
Prior Judge FE	Yes	Yes	Yes	Yes
Panel ID FE	Yes	Yes	Yes	Yes
Mean Dep Var.	0.369	0.367	0.365	0.367

Notes: Standard errors clustered at the Circuit×Year level. The results report regressions with *Reversal* as the dependent variable and apply various levels of trimming based on the tails of the estimated probability of the case being published. Column (1) corresponds to the same specification as column (10) in Table [2] of our main results.

Figure 1: Balance Checks



Notes: Plotted coefficients correspond to RRD, RDD, and DDD from regressions with CircuitxYear and district fixed effects, clustered at the circuit level, with 99% confidence intervals. Democrat, Female, Minority, Chief, Senior, Age and Tenure refer to trial judge characteristics. Age and tenure are transformed into a share of their respective maximum values.

Figure 2: Number of Cases Over Time

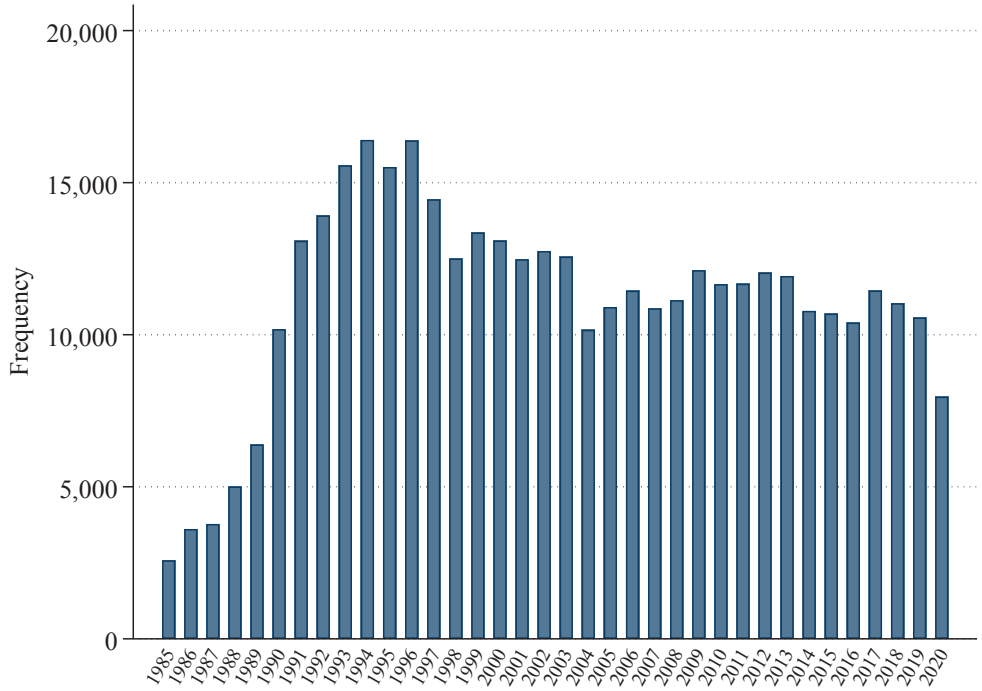


Figure 3: Political Composition Over Time

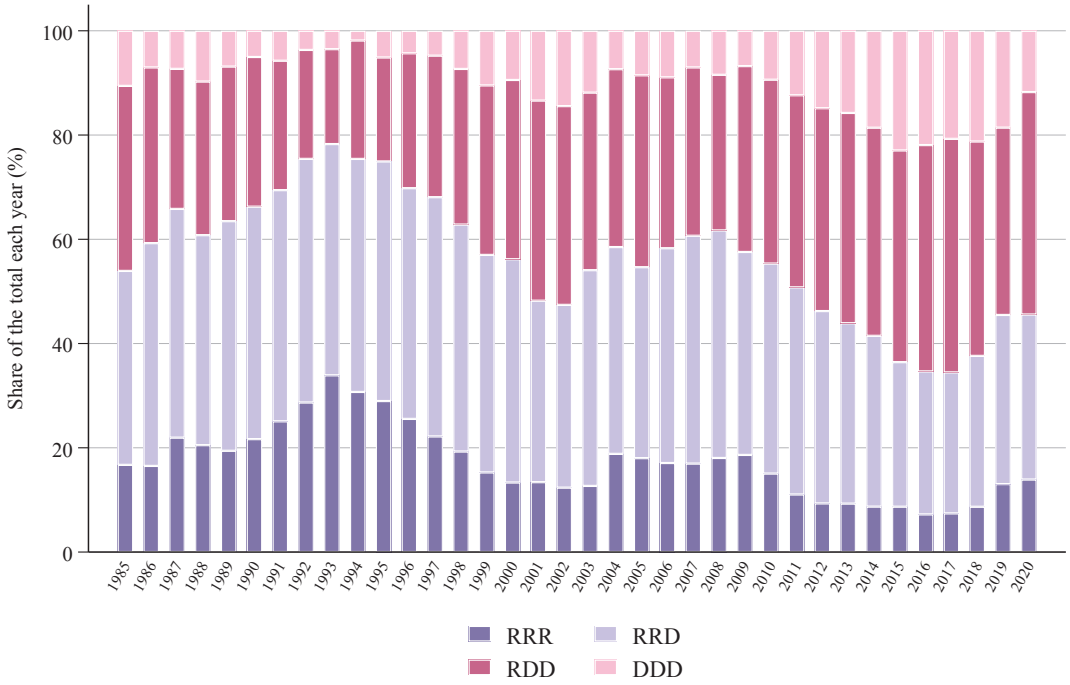


Figure 4: Gender Composition Over Time

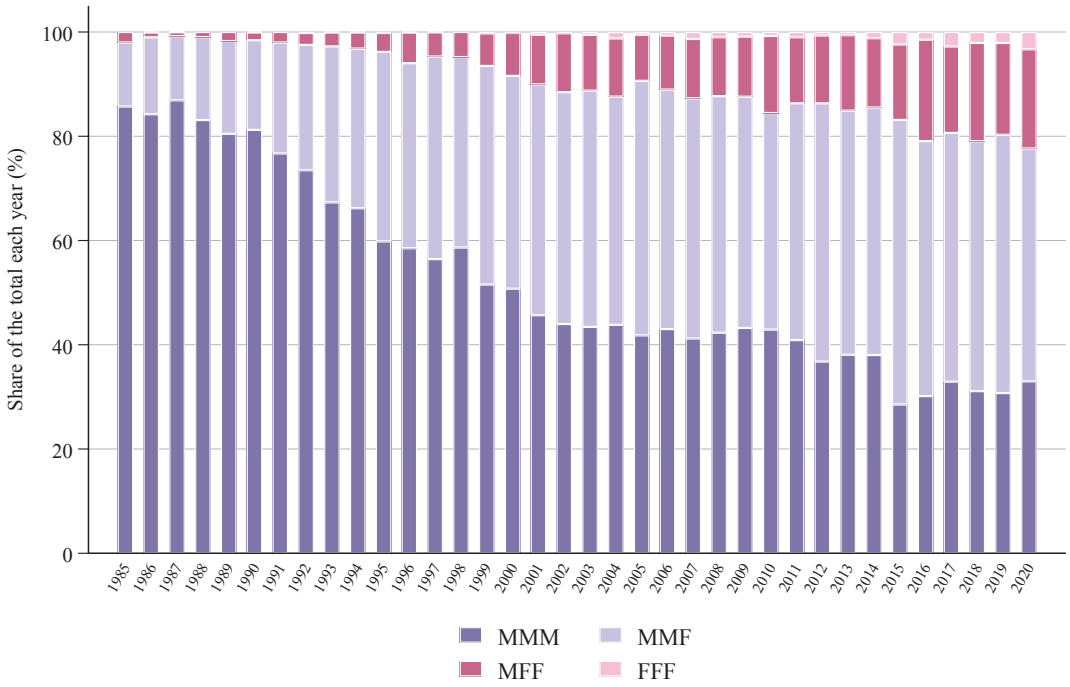


Figure 5: Minorities Composition Over Time

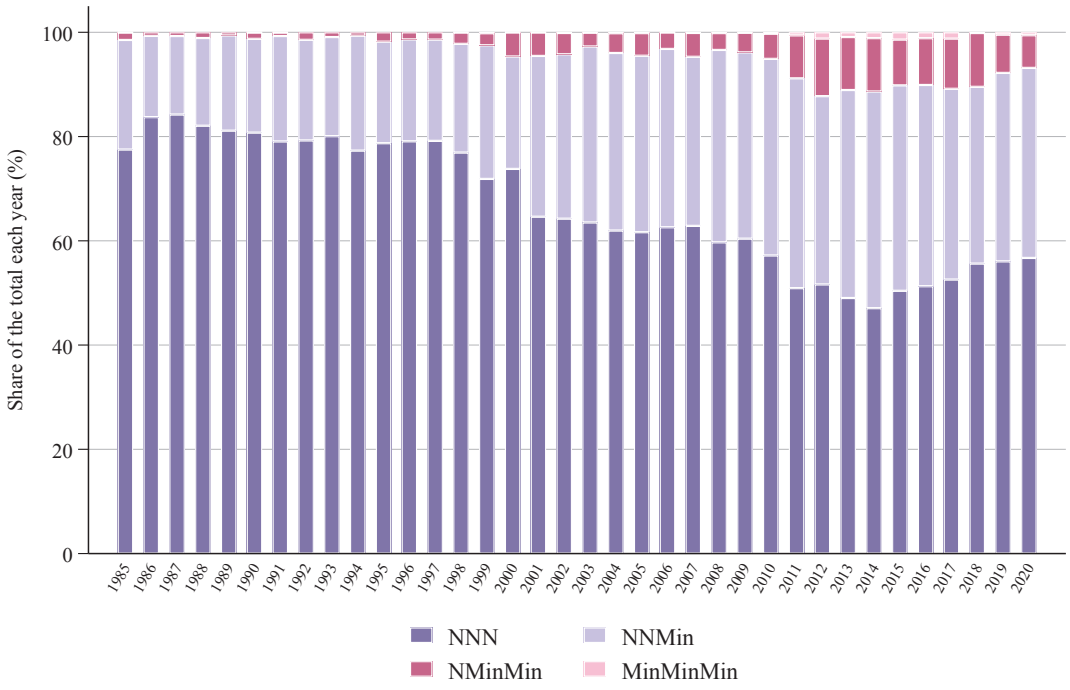
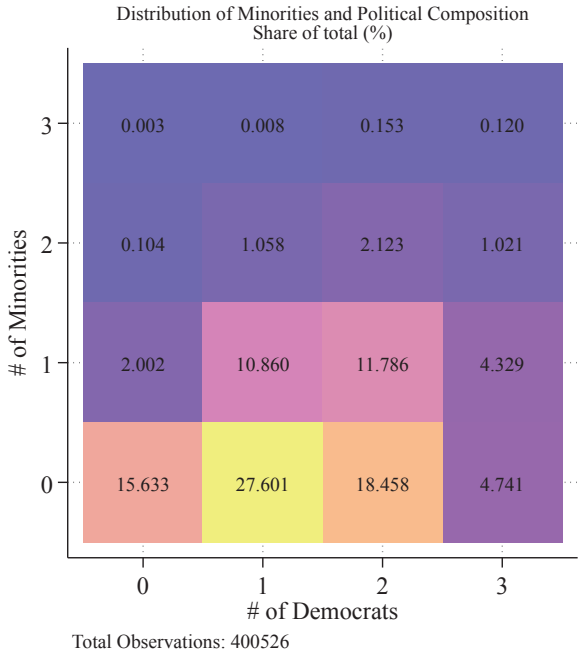
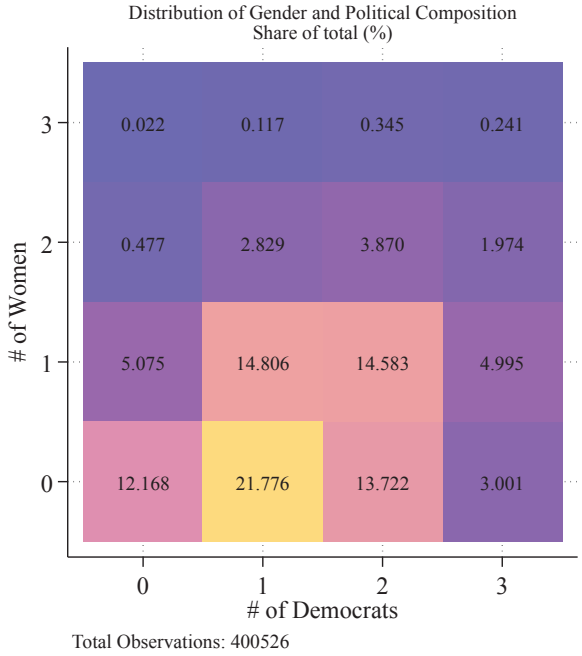


Figure 6: Panel Composition by Minority Status and by Gender



(a) Panel Composition by Minority Status



(b) Panel Composition by Gender

Figure 7: Share of Reversed and Published Cases Over Time

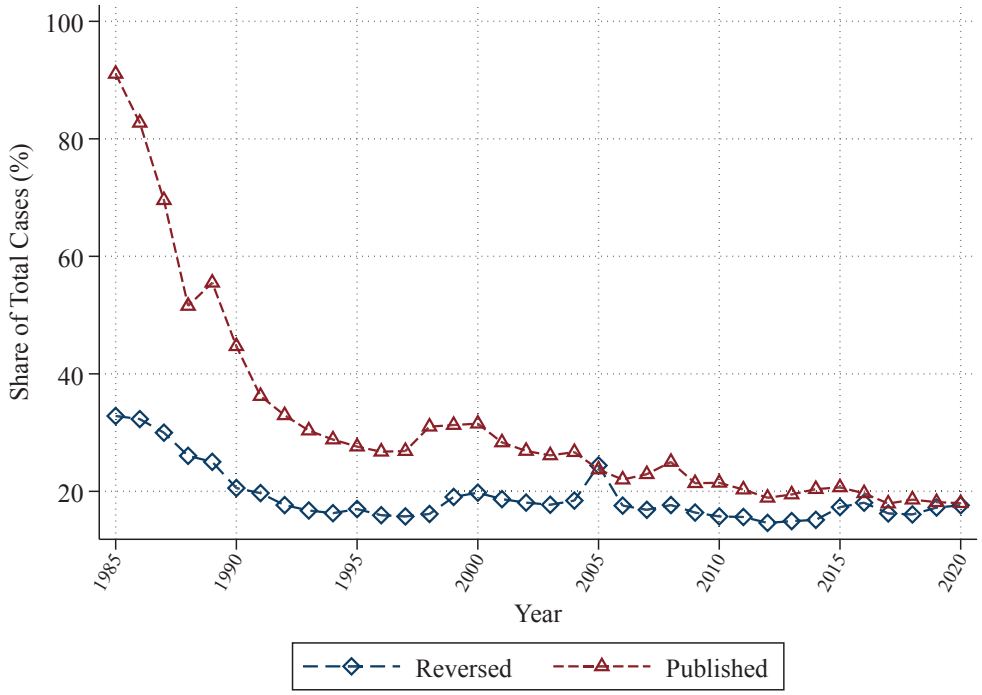


Figure 8: Composition of Reversed and Published Cases Over Time

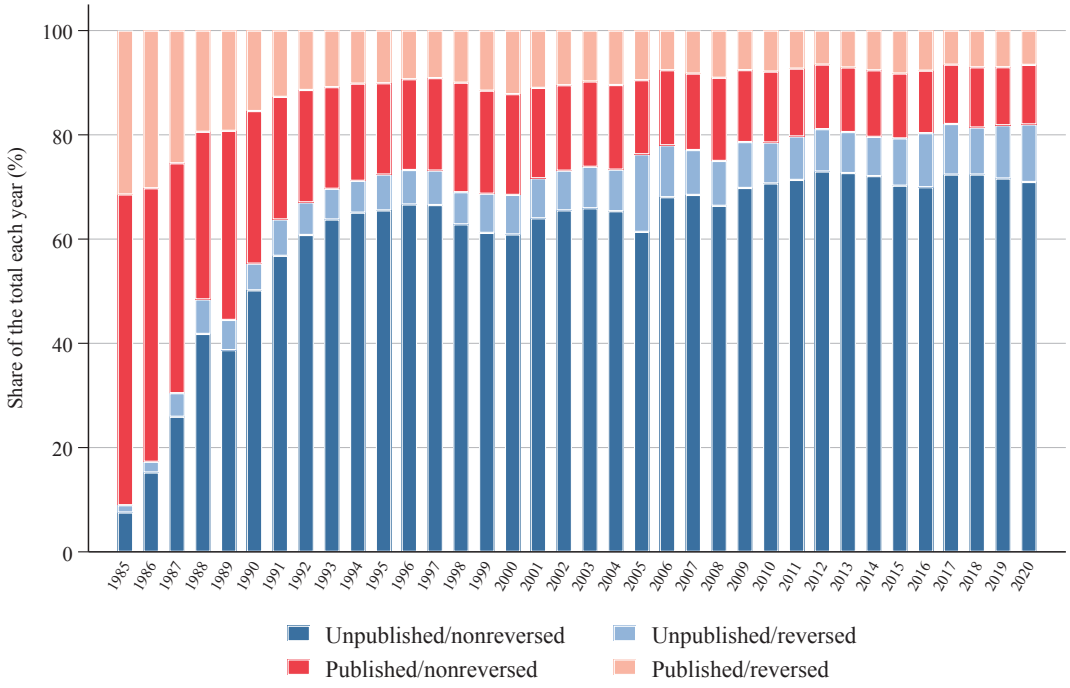
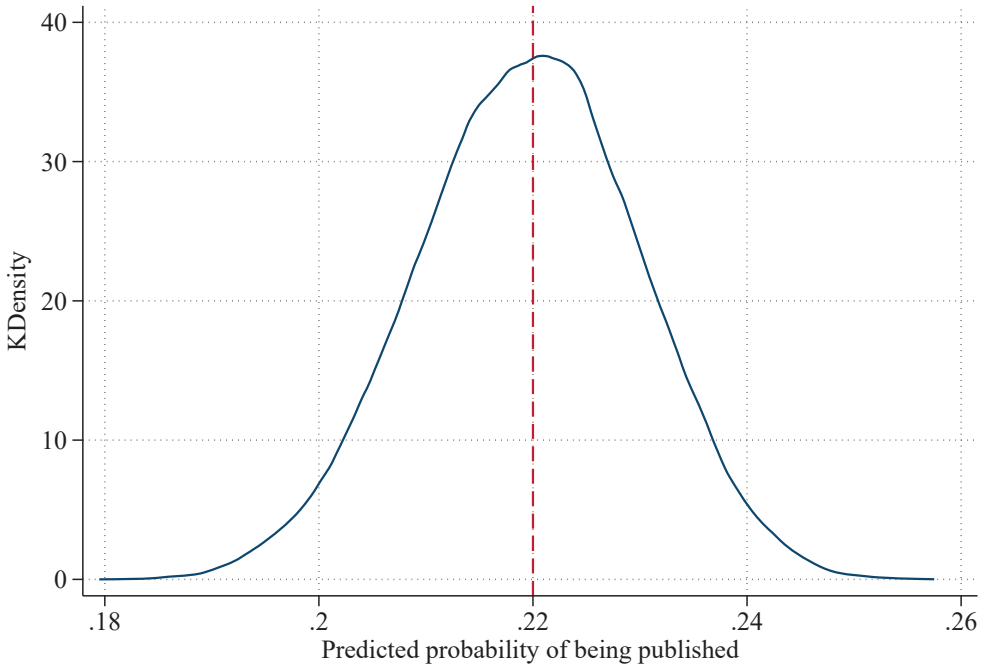


Figure 9: Predicted Probability of Publication



Notes: Dashed line indicates the average of published cases after 2000 (.22). Std Dev is .414. N = 224660.

A Supplemental Tables and Figures

Table A.1: The Effect of Gender Identity on Reversal

VARIABLES	(1) All Cases	(2) Unpublished Before	(3) Unpublished After	(4) Published Before	(5) Published After	(6) Prior-Judge Fixed Effects	(7) Panel Fixed Effects	(8) Prior-Panel Fixed Effects
MMF	-0.001	0.000	0.002	0.005	0.001	0.001		
	(0.002)	(0.002)	(0.003)	(0.006)	(0.006)	(0.006)		
MFF+FFF	0.007**	0.002	0.014***	-0.017	-0.000	-0.000		
	(0.003)	(0.007)	(0.004)	(0.017)	(0.008)	(0.008)		
(FMM) x TJ Fem	0.005	0.011	-0.002	0.002	0.013	0.015	0.017	0.017
	(0.003)	(0.007)	(0.004)	(0.014)	(0.011)	(0.011)	(0.014)	(0.015)
(FFM + FFF) x TJ Fem	0.000	0.005	-0.002	-0.050	0.010	0.007	0.020	0.012
	(0.005)	(0.015)	(0.006)	(0.031)	(0.014)	(0.015)	(0.019)	(0.020)
TJ Democrat	-0.002	-0.004**	-0.000	0.001	0.000		0.003	
	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)		(0.007)	
TJ Female	-0.009***	-0.009**	-0.005	-0.004	-0.018**		-0.022**	
	(0.002)	(0.004)	(0.003)	(0.007)	(0.008)		(0.011)	
TJ Minority	0.017***	0.014***	0.016***	0.024***	0.033***		0.038***	
	(0.002)	(0.003)	(0.002)	(0.007)	(0.006)		(0.008)	
TJ Chief	-0.005**	-0.002	-0.004	-0.009*	-0.009	-0.005	-0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)
TJ Senior	0.001	-0.005	0.003	0.004	-0.003	0.002	-0.004	0.006
	(0.002)	(0.004)	(0.003)	(0.007)	(0.007)	(0.010)	(0.008)	(0.011)
TJ Tenure	0.001***	0.001***	-0.000	0.001	0.000	-0.013	-0.001	-0.029**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.010)	(0.001)	(0.013)
TJ Tenure^2	0.000**	-0.000	0.000***	0.000	0.000*	0.000	0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel Minority > 0	-0.001	-0.003	0.000	-0.011*	0.013**	0.014***		
	(0.002)	(0.002)	(0.002)	(0.006)	(0.005)	(0.005)		
Panel Tenure	0.001**	0.000	-0.000**	0.002***	-0.001	-0.001	-0.039	-0.144
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.280)	(0.279)
Observations	400,513	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.053	0.025	0.034	0.049	0.058	0.090	0.279	0.312
Prior Judge FE	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: Standard errors clustered at the Circuit×Year level.

Table A.2: The Effect of Minority Identity on Reversal

VARIABLES	(1) All Cases	(2) Unpublished Before	(3) Unpublished After	(4) Published Before	(5) Published After	(6) Prior-Judge Fixed Effects	(7) Panel Fixed Effects	(8) Prior-Panel Fixed Effects
NNMin	-0.002	-0.005*	-0.001	-0.008	0.011*	0.012**		
	(0.002)	(0.003)	(0.002)	(0.006)	(0.006)	(0.006)		
NMinMin+MinMinMin	0.009*	0.005	0.005	-0.003	0.036***	0.040***		
	(0.005)	(0.014)	(0.004)	(0.021)	(0.014)	(0.014)		
(NNMin) x TJ Min	-0.001	0.005	0.001	-0.019	0.008	0.002	0.002	-0.005
	(0.004)	(0.007)	(0.004)	(0.015)	(0.013)	(0.014)	(0.018)	(0.018)
(NMinMin + MinMinMin) x TJ Min	-0.012	-0.010	-0.001	-0.009	-0.066**	-0.074**	-0.103***	-0.111***
	(0.008)	(0.023)	(0.008)	(0.046)	(0.028)	(0.029)	(0.035)	(0.038)
TJ Democrat	-0.002	-0.004**	-0.000	0.001	0.000		0.003	
	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)		(0.007)	
TJ Female	-0.007***	-0.005	-0.006***	-0.005	-0.010**		-0.011*	
	(0.002)	(0.004)	(0.002)	(0.006)	(0.005)		(0.007)	
TJ Minority	0.018***	0.013***	0.016***	0.028***	0.034***		0.042***	
	(0.003)	(0.004)	(0.003)	(0.008)	(0.008)		(0.010)	
TJ Chief	-0.005**	-0.002	-0.003	-0.009*	-0.009	-0.005	-0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)
TJ Senior	0.001	-0.005	0.003	0.004	-0.003	0.002	-0.004	0.006
	(0.002)	(0.004)	(0.003)	(0.007)	(0.007)	(0.010)	(0.008)	(0.011)
TJ Tenure	0.001***	0.001***	-0.000	0.001	0.000	-0.013	-0.001	-0.029**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.010)	(0.001)	(0.013)
TJ Tenure^2	0.000**	-0.000	0.000***	0.000	0.000*	0.000	0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel Tenure	0.000**	0.000	-0.001**	0.002***	-0.001	-0.001	-0.041	-0.147
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.280)	(0.278)
Panel Female > 0	0.001	0.002	0.005**	0.003	0.004	0.004		
	(0.002)	(0.003)	(0.002)	(0.006)	(0.005)	(0.005)		
Observations	400,513	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.053	0.025	0.034	0.049	0.058	0.090	0.279	0.312
Prior Judge FE	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: Standard errors clustered at the Circuit×Year level.

Table A.3: The Effect of Gender and Minority Identity on Reversal

VARIABLES	(1) All Cases	(2) Unpublished Before	(3) Unpublished After	(4) Published Before	(5) Published After	(6) Prior-Judge Fixed Effects	(7) Panel Fixed Effects	(8) Prior-Panel Fixed Effects
MMF	-0.014	0.137***	0.016	-0.324	-0.255	-0.197		
	(0.063)	(0.020)	(0.049)	(0.229)	(0.269)	(0.263)		
MFF+FFF	-0.012	0.137***	0.025	-0.353	-0.264	-0.206		
	(0.064)	(0.022)	(0.049)	(0.229)	(0.269)	(0.263)		
NNMin	-0.008***	-0.009***	-0.004*	-0.016**	0.001	0.003		
	(0.002)	(0.003)	(0.002)	(0.007)	(0.006)	(0.006)		
NMinMin+MinMinMin	-0.002	-0.006	0.001	-0.018	0.015	0.020		
	(0.005)	(0.014)	(0.004)	(0.020)	(0.013)	(0.013)		
RRD	0.010***	0.005	0.001	0.016**	0.025***	0.025***		
	(0.003)	(0.003)	(0.005)	(0.007)	(0.009)	(0.009)		
RDD	0.031***	0.015***	0.013**	0.041***	0.067***	0.066***		
	(0.004)	(0.005)	(0.005)	(0.008)	(0.010)	(0.010)		
DDD	0.063***	0.044***	0.033***	0.058***	0.134***	0.137***		
	(0.005)	(0.007)	(0.006)	(0.014)	(0.014)	(0.015)		
RRD x TJ Dem	-0.005	-0.001	0.004	-0.006	-0.031***	-0.035***	-0.033**	-0.038**
	(0.004)	(0.004)	(0.005)	(0.009)	(0.012)	(0.012)	(0.014)	(0.016)
RDD x TJ Dem	-0.005	0.006	0.002	-0.009	-0.045***	-0.048***	-0.044***	-0.047***
	(0.004)	(0.006)	(0.005)	(0.012)	(0.012)	(0.013)	(0.015)	(0.018)
DDD x TJ Dem	-0.016***	0.001	-0.005	-0.025	-0.070***	-0.074***	-0.071***	-0.071***
	(0.006)	(0.010)	(0.008)	(0.018)	(0.018)	(0.019)	(0.023)	(0.025)
(FMM) x TJ Fem	0.005	0.010	-0.002	0.003	0.016	0.017	0.019	0.019
	(0.003)	(0.007)	(0.004)	(0.014)	(0.011)	(0.011)	(0.014)	(0.015)
(FFM + FFF) x TJ Fem	0.001	0.005	-0.002	-0.049	0.014	0.010	0.023	0.015
	(0.005)	(0.015)	(0.006)	(0.031)	(0.014)	(0.015)	(0.019)	(0.020)
(NNMin) x TJ Min	0.001	0.005	0.002	-0.016	0.013	0.007	0.006	-0.002
	(0.004)	(0.007)	(0.004)	(0.015)	(0.013)	(0.014)	(0.018)	(0.018)
(NMinMin + MinMinMin) x TJ Min	-0.009	-0.007	-0.001	-0.005	-0.056**	-0.063**	-0.097***	-0.106***
	(0.008)	(0.023)	(0.008)	(0.047)	(0.027)	(0.028)	(0.035)	(0.038)
Observations	400,513	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.055	0.026	0.035	0.050	0.061	0.093	0.279	0.312
Prior Judge FE	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: Standard errors clustered at the Circuit×Year level. All specifications include the usual FE and covariates. Dummy on female presence is excluded.

Table A.4: Differentiating the Effect of Democratic and Female Trial Court Judges

VARIABLES	(1) All Cases	(2) Unpublished Pre-2000	(3) Unpublished Post-2000	(4) Published Pre-2000	(5) Published Post-2000	(6) Prior-Judge Fixed Effects	(7) Panel Fixed Effects	(8) Prior-Panel Fixed Effects
MMF	-0.006***	-0.004	0.002	-0.007	-0.011	-0.012*		
	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)		
MFF+FFF	-0.002	-0.003	0.012***	-0.009	-0.021**	-0.024**		
	(0.004)	(0.008)	(0.004)	(0.021)	(0.010)	(0.011)		
RRD	0.011***	0.005	0.001	0.017**	0.026***	0.028***		
	(0.003)	(0.003)	(0.005)	(0.007)	(0.009)	(0.009)		
RDD	0.031***	0.013***	0.013**	0.043***	0.070***	0.071***		
	(0.004)	(0.005)	(0.005)	(0.009)	(0.010)	(0.011)		
DDD	0.064***	0.041***	0.033***	0.057***	0.137***	0.143***		
	(0.005)	(0.007)	(0.006)	(0.014)	(0.014)	(0.015)		
RRD x TJ Dem	-0.004	-0.001	0.004	-0.005	-0.030**	-0.035***	-0.032**	-0.038**
	(0.004)	(0.004)	(0.005)	(0.009)	(0.013)	(0.013)	(0.016)	(0.017)
RDD x TJ Dem	-0.004	0.005	0.003	-0.009	-0.043***	-0.047***	-0.043***	-0.049**
	(0.004)	(0.006)	(0.005)	(0.012)	(0.013)	(0.014)	(0.017)	(0.020)
DDD x TJ Dem	-0.015**	-0.002	-0.002	-0.027	-0.072***	-0.075***	-0.076***	-0.079***
	(0.006)	(0.009)	(0.008)	(0.019)	(0.019)	(0.020)	(0.025)	(0.027)
(FMM) x TJ Dem	0.002	0.001	-0.003	0.015	0.006	0.008	0.003	0.005
	(0.003)	(0.004)	(0.003)	(0.010)	(0.009)	(0.010)	(0.012)	(0.012)
(FFM + FFF) x TJ Dem	-0.004	-0.004	-0.007*	-0.057**	0.008	0.014	0.022	0.029
	(0.004)	(0.008)	(0.004)	(0.027)	(0.014)	(0.015)	(0.018)	(0.018)
(FMM) x TJ Fem	0.006	0.008	-0.001	0.001	0.016	0.018	0.020	0.021
	(0.004)	(0.007)	(0.004)	(0.014)	(0.011)	(0.011)	(0.015)	(0.015)
(FFM + FFF) x TJ Fem	0.003	0.003	0.000	-0.038	0.015	0.012	0.021	0.014
	(0.005)	(0.015)	(0.006)	(0.031)	(0.014)	(0.015)	(0.019)	(0.020)
RRD x TJ Fem	-0.005	0.002	0.001	-0.014	-0.008	-0.011	-0.011	-0.015
	(0.005)	(0.006)	(0.006)	(0.017)	(0.014)	(0.014)	(0.016)	(0.017)
RDD x TJ Fem	-0.007	0.014**	-0.000	-0.020	-0.020	-0.024	-0.024	-0.027
	(0.005)	(0.007)	(0.007)	(0.017)	(0.015)	(0.016)	(0.018)	(0.020)
DDD x TJ Fem	-0.006	0.029**	-0.004	0.011	-0.007	-0.022	-0.005	-0.014
	(0.007)	(0.014)	(0.008)	(0.026)	(0.020)	(0.022)	(0.025)	(0.027)
TJ Democrat	0.002	-0.005	-0.000	0.005	0.031***		0.033**	
	(0.003)	(0.004)	(0.005)	(0.008)	(0.010)		(0.013)	
Observations	400,513	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.055	0.026	0.035	0.050	0.061	0.093	0.279	0.312
Prior Judge FE	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: Standard errors clustered at the Circuit×Year level. All specifications include usual FE and covariates. Dummy on minority presence is excluded.

Table A.5: Differentiating the Effect of Democratic and Minority Trial Court Judges

VARIABLES	(1) All Cases	(2) Unpublished Pre-2000	(3) Unpublished Post-2000	(4) Published Pre-2000	(5) Published Post-2000	(6) Prior-Judge Fixed Effects	(7) Panel Fixed Effects	(8) Prior-Panel Fixed Effects
NNMin	-0.006**	-0.006*	-0.005*	-0.010	0.014*	0.018**		
	(0.003)	(0.003)	(0.003)	(0.008)	(0.007)	(0.008)		
NMinMin+MinMinMin	-0.002	-0.003	-0.004	-0.019	0.033**	0.043**		
	(0.005)	(0.015)	(0.005)	(0.025)	(0.016)	(0.017)		
RRD	0.009***	0.004	0.001	0.016**	0.021**	0.021**		
	(0.003)	(0.003)	(0.005)	(0.007)	(0.009)	(0.009)		
RDD	0.029***	0.013***	0.013**	0.040***	0.061***	0.059***		
	(0.004)	(0.005)	(0.005)	(0.008)	(0.010)	(0.011)		
DDD	0.061***	0.042***	0.033***	0.054***	0.127***	0.129***		
	(0.005)	(0.007)	(0.007)	(0.014)	(0.014)	(0.015)		
RRD x TJ Dem	-0.005	-0.001	0.002	-0.003	-0.024**	-0.028**	-0.027*	-0.032**
	(0.004)	(0.004)	(0.006)	(0.009)	(0.012)	(0.013)	(0.015)	(0.016)
RDD x TJ Dem	-0.005	0.005	-0.001	-0.005	-0.033***	-0.035***	-0.034**	-0.037**
	(0.004)	(0.006)	(0.005)	(0.012)	(0.012)	(0.013)	(0.016)	(0.018)
DDD x TJ Dem	-0.016***	0.000	-0.008	-0.024	-0.055***	-0.058***	-0.058**	-0.057**
	(0.006)	(0.010)	(0.008)	(0.018)	(0.018)	(0.018)	(0.023)	(0.025)
(NNMin) x TJ Dem	-0.005*	-0.008*	0.003	-0.015	-0.031***	-0.035***	-0.023**	-0.026**
	(0.003)	(0.004)	(0.003)	(0.010)	(0.009)	(0.009)	(0.012)	(0.012)
(NMinMin + MinMinMin) x TJ Dem	0.001	-0.008	0.010	0.006	-0.039*	-0.049**	-0.041	-0.045*
	(0.006)	(0.014)	(0.007)	(0.037)	(0.021)	(0.023)	(0.026)	(0.027)
(NNMin) x TJ Min	0.000	0.005	0.000	-0.011	0.019	0.013	0.009	0.003
	(0.004)	(0.007)	(0.004)	(0.016)	(0.014)	(0.014)	(0.018)	(0.018)
(NMinMin + MinMinMin) x TJ Min	-0.012	-0.009	-0.004	-0.006	-0.049*	-0.057**	-0.093***	-0.100***
	(0.008)	(0.024)	(0.008)	(0.048)	(0.028)	(0.028)	(0.035)	(0.037)
RRD x TJ Min	0.006	0.005	0.013	-0.014	0.003	0.005	-0.001	-0.002
	(0.006)	(0.007)	(0.008)	(0.015)	(0.016)	(0.017)	(0.022)	(0.022)
RDD x TJ Min	0.009	0.020**	0.011	-0.019	-0.008	-0.002	0.001	0.001
	(0.006)	(0.008)	(0.007)	(0.015)	(0.017)	(0.018)	(0.022)	(0.024)
DDD x TJ Min	0.014*	0.021	0.015	0.011	-0.006	-0.002	0.008	-0.003
	(0.008)	(0.014)	(0.009)	(0.029)	(0.021)	(0.022)	(0.028)	(0.032)
TJ Democrat	0.005	-0.003	-0.001	0.008	0.039***		0.039***	
	(0.003)	(0.004)	(0.005)	(0.008)	(0.011)		(0.013)	
Observations	400,513	113,672	175,328	62,166	49,304	49,220	45,088	44,969
R-squared	0.055	0.026	0.035	0.050	0.061	0.094	0.280	0.312
Prior Judge FE	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.179	0.0975	0.115	0.355	0.374	0.374	0.369	0.369

Notes: Standard errors clustered at the Circuit×Year level. All specifications include the usual FE and covariates. Dummy on minority presence is excluded.

Table A.6: Marginal Effects for a Multinomial Probit on Publication and Reversal

	Pre-2000				Post-2000			
	Unpublished		Published		Unpublished		Published	
Outcome	Non-Reversed	Reversed	Non-Reversed	Reversed	Non-Reversed	Reversed	Non-Reversed	Reversed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RRD								
	-.0092	.004*	-.0019	.0071	.0052	.0007	-.0097**	.0038
	(.0107)	(.0024)	(.0067)	(.005)	(.0081)	(.004)	(.0048)	(.0038)
	[.3921]	[.0922]	[.7778]	[.1597]	[.5236]	[.8641]	[.0438]	[.3171]
RDD								
	-.0342**	.0065**	.0017	.0261***	-.0035	.0098**	-.0182***	.012***
	(.0135)	(.003)	(.008)	(.0069)	(.0098)	(.0041)	(.0051)	(.0044)
	[.0111]	[.0325]	[.8373]	[.0001]	[.7175]	[.0165]	[.0004]	[.0068]
DDD								
	-.0683***	.0215***	.0086	.0382***	-.0262**	.0246***	-.0282***	.0298***
	(.0211)	(.0055)	(.0129)	(.012)	(.0122)	(.0053)	(.0058)	(.0069)
	[.0012]	[.0001]	[.505]	[.0014]	[.0324]	[0]	[0]	[0]
RRD x TJ Dem								
	.0072	-.0007	-.0011	-.0054	.0001	.0036	.005	-.0086**
	(.0076)	(.0033)	(.0058)	(.0044)	(.0065)	(.0048)	(.0044)	(.0035)
	[.3452]	[.8333]	[.8523]	[.2194]	[.9878]	[.4558]	[.2621]	[.0134]
RDD x TJ Dem								
	-.0081	.0028	.0095	-.0041	.0054	.0034	.0051	-.0138***
	(.0086)	(.0043)	(.0065)	(.0051)	(.0069)	(.0044)	(.0045)	(.0034)
	[.3432]	[.5232]	[.1449]	[.4171]	[.4378]	[.4472]	[.2519]	[0]
DDD x TJ Dem								
	-.0057	-.001	.0149	-.0082	.0152*	-.0011	.0063	-.0205***
	(.0124)	(.0059)	(.011)	(.0072)	(.0083)	(.0058)	(.0058)	(.0036)
	[.6445]	[.8659]	[.1727]	[.2564]	[.0662]	[.8555]	[.2722]	[0]
TJ Democrat								
	.0117*	.0002	-.012**	.0002	-.0134**	-.0042	.0009	.0168***
	(.0064)	(.0032)	(.0052)	(.0041)	(.0063)	(.0042)	(.0042)	(.0032)
	[.0668]	[.961]	[.0209]	[.9694]	[.0336]	[.3135]	[.8317]	[0]
$\widehat{Prob}$	.609	.067	.202	.122	.716	.09	.116	.078
	N = 175866				N = 224660			

Notes: The first row under each outcome represents the marginal effect at means estimated using the `margins` Stata command, after running a multinomial probit regression including fixed effects at the Circuit and Year levels, and all usual covariates. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit × Year level), and the third row provides the associated p-values (in square brackets). The penultimate row shows the conditional probability of each outcome fixed at the means of all the covariates.

Table A.7: Marginal Effects Multinomial Logit

	Pre-2000				Post-2000			
	Unpublished		Published		Unpublished		Published	
Outcome	Non-Reversed	Reversed	Non-Reversed	Reversed	Non-Reversed	Reversed	Non-Reversed	Reversed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RRD								
	-.01	.0037	.0003	.0061	.0049	.0004	-.0081*	.0028
	(.0112)	(.0023)	(.0069)	(.0051)	(.0077)	(.0038)	(.0046)	(.0034)
	[.3714]	[.1093]	[.9626]	[.231]	[.5265]	[.9243]	[.0768]	[.406]
RDD								
	-.0367***	.0062**	.0063	.0242***	-.0025	.0089**	-.0161***	.0097**
	(.0141)	(.0029)	(.0084)	(.0069)	(.0095)	(.0039)	(.0051)	(.004)
	[.0093]	[.0334]	[.4498]	[.0005]	[.7878]	[.0217]	[.0017]	[.015]
DDD								
	-.0696***	.0211***	.0122	.0363***	-.0224*	.0229***	-.0263***	.0259***
	(.0227)	(.0053)	(.0137)	(.0124)	(.012)	(.0051)	(.0058)	(.0065)
	[.0022]	[.0001]	[.3747]	[.0035]	[.0608]	[0]	[0]	[.0001]
RRD x TJ Dem								
	.0064	-.0008	-.0017	-.0039	-.0009	.0037	.0042	-.007**
	(.0078)	(.0031)	(.0056)	(.0042)	(.0062)	(.0045)	(.004)	(.003)
	[.4134]	[.8097]	[.7652]	[.3463]	[.8856]	[.4156]	[.2899]	[.0198]
RDD x TJ Dem								
	-.0078	.0029	.0068	-.0019	.0041	.0029	.0047	-.0116***
	(.0088)	(.0041)	(.0065)	(.0048)	(.0067)	(.0042)	(.0042)	(.0029)
	[.3796]	[.4792]	[.2968]	[.6921]	[.5444]	[.4921]	[.2641]	[.0001]
DDD x TJ Dem								
	-.0084	-.0013	.0152	-.0055	.0115	-.0007	.0063	-.0171***
	(.0129)	(.0053)	(.0109)	(.0069)	(.0078)	(.0053)	(.0056)	(.0031)
	[.5133]	[.8127]	[.1642]	[.4273]	[.1419]	[.9013]	[.2629]	[0]
TJ Democrat								
	.0138**	-.0006	-.0103**	-.0029	-.0096	-.0043	.0009	.013***
	(.0067)	(.0031)	(.0051)	(.0039)	(.0061)	(.0039)	(.0038)	(.0028)
	[.0383]	[.8382]	[.0426]	[.4556]	[.1136]	[.2756]	[.816]	[0]
$\widehat{Prob}$	.629	.063	.193	.115	.736	.082	.113	.069
	N = 175866				N = 224656			

Notes: The first row under each outcome represents the marginal effect at means estimated using the `margins` Stata command, after running a multinomial logit regression including fixed effects at the Circuit and Year level, and all usual covariates. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit×Year level), and the third row provides the associated p-values (in square brackets). The penultimate row shows the conditional probability of each outcome fixed at the means of all the covariates.

Table A.8: Independence of Irrelevant Alternatives tests

	Pre-2000				Post-2000			
	Unpublished		Published		Unpublished		Published	
Outcome	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Hausman Test								
χ^2		1051.60	4.07	-2.0e+04		-2.8e+05	2374.42	-9511.90
p-value		0.000	1.000	–		–	0.000	–
Panel B. Small-Hsiao Test								
χ^2		344189.2	4146.307	6226.659		103.427	469.956	2605.107
p-value		0	0	0		0	0	0

Notes: Panel A reports chi-squared statistics and p-values from Hausman tests comparing restricted and full multinomial logit models, with outcome 1 as the base. When the chi-squared statistic is negative, the p-value is not reported. Panel B shows analogous statistics from the Small-Hsiao test, which splits the sample and compares likelihoods of a restricted and amalgamated model.

Table A.9: Published as the Dependent Variable

VARIABLES	(1) All Cases	(2) All Cases Pre-2000	(3) All Cases Post-2000	(4) Non-reversed Pre-2000	(5) Non-reversed Post-2000	(6) Reversed Pre-2000	(7) Reversed Post-2000	(8) Prior-Judge Fixed Effects	(9) Panel Fixed Effects	(10) Prior-Panel Fixed Effects
RRD	0.005	0.009	-0.005	0.005	-0.009	0.003	0.005	0.005		
	(0.005)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.012)	(0.012)		
RDD	0.019***	0.029***	0.001	0.017**	-0.009	0.030***	0.017	0.018		
	(0.005)	(0.008)	(0.007)	(0.007)	(0.006)	(0.010)	(0.013)	(0.013)		
DDD	0.034***	0.056***	0.012	0.037***	-0.011*	0.035*	0.036**	0.038**		
	(0.007)	(0.013)	(0.008)	(0.011)	(0.007)	(0.019)	(0.016)	(0.017)		
RRD x TJ Dem	-0.002	-0.003	-0.001	0.000	0.005	-0.007	-0.012	-0.015	-0.010	-0.014
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.015)	(0.015)	(0.019)	(0.020)
RDD x TJ Dem	0.001	0.009*	-0.004	0.015***	0.004	-0.013	-0.027**	-0.030**	-0.020	-0.030
	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.013)	(0.013)	(0.014)	(0.018)	(0.020)
DDD x TJ Dem	-0.004	0.016*	-0.010*	0.025**	0.003	-0.009	-0.040**	-0.044**	-0.040*	-0.052**
	(0.005)	(0.009)	(0.006)	(0.010)	(0.006)	(0.019)	(0.017)	(0.018)	(0.022)	(0.024)
TJ Democrat	-0.001	-0.007	0.004	-0.011**	-0.003	0.011	0.023**		0.020	
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.009)	(0.011)		(0.016)	
TJ Female	-0.002	-0.002	-0.003	-0.001	-0.002	0.004	0.003		0.006	
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.009)	(0.006)		(0.007)	
TJ Minority	-0.004*	-0.004	-0.004	-0.005	-0.005**	-0.021**	-0.021***		-0.020***	
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.009)	(0.006)		(0.007)	
TJ Chief	-0.001	-0.004	0.002	-0.004	0.004*	-0.000	0.005	0.000	-0.002	-0.011
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.007)	(0.007)	(0.009)	(0.008)	(0.010)
TJ Senior	0.001	-0.000	0.002	-0.000	0.002	0.010	-0.001	-0.000	-0.015*	-0.015
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.009)	(0.007)	(0.009)	(0.008)	(0.011)
TJ Tenure	0.002***	0.002***	0.002***	0.002***	0.001***	-0.000	0.002**	-0.038***	0.003***	-0.023
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.010)	(0.001)	(0.014)
TJ Tenure^2	-0.000***	-0.000**	-0.000***	-0.000**	-0.000***	-0.000	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel Female > 0	-0.010***	-0.011**	-0.008***	-0.007*	-0.005*	-0.020***	-0.020***	-0.020***		
	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)	(0.007)	(0.006)	(0.006)		
Panel Minority > 0	-0.009***	-0.012**	-0.008**	-0.007	-0.006**	-0.010	-0.010	-0.009		
	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.008)	(0.006)	(0.006)		
Panel Tenure	0.002***	0.003***	0.001**	0.003***	0.001**	0.003***	0.001***	0.002***	0.417	0.355
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.409)	(0.406)
Observations	400,513	175,856	224,648	142,716	186,025	33,132	38,610	38,524	33,697	33,553
R-squared	0.362	0.407	0.292	0.421	0.304	0.320	0.257	0.287	0.527	0.552
Prior Judge FE	No	No	No	No	No	No	No	Yes	No	Yes
Panel ID FE	No	No	No	No	No	No	No	No	Yes	Yes
Mean Dep Var.	0.278	0.354	0.220	0.281	0.166	0.666	0.477	0.477	0.466	0.466

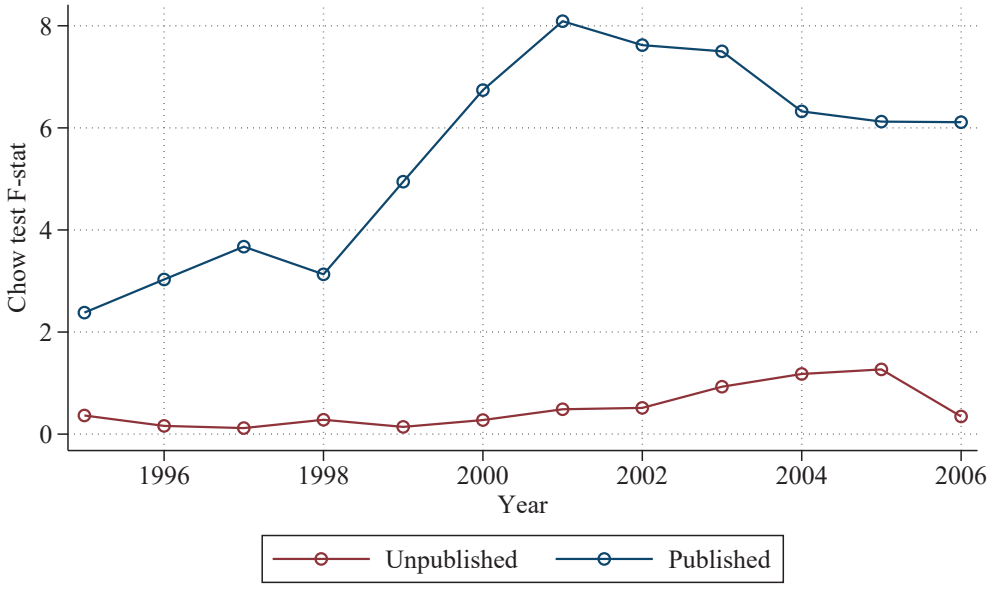
Notes: The table presents results from regressions with *Published* as the dependent variable, with specifications mirroring Table [2] and as specified in the column headers and notes. Standard errors are in parentheses and are clustered by Circuit×Year. All regressions also include Circuit×Year, District, Appeal Type, Nature of the suit (for Civil cases), and Offense Type (for Criminal cases) fixed effects. Coefficient estimates and standard errors are listed with stars indicating statistical significance (* p<0.1, ** p<0.05, *** p<0.01). The coefficients of interest are those on the political composition of the appellate panel (RDD, RRD, and DDD) and their interaction with the political affiliation of the trial court judge.

B Structural Break and Rolling Window Analysis

In the results presented in the body of the paper, we split our results in the year 2001. This is based on the literature and context, namely the view that there was a significant shift in the contentious nature of politics beginning with the George W. Bush presidency.

To add a dimension of quantitative evidence to this, we turn to a Chow-style test for a structural break, successively splitting the sample at every year from 1995 to 2015, interacting the specification to allow that parameter to change before and after that year, and then performing an F-test on the joint significance of the structural break indicator and interactions. We perform this test separately for published and unpublished cases, with the expectation that the former results will show a more pronounced shift in the early 2000s. The results are presented in Figure [B.1].

Figure B.1: Chow Test

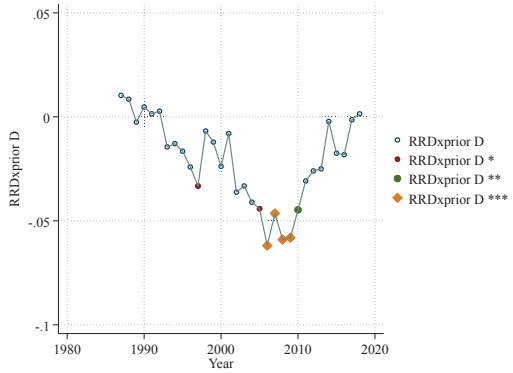


Notes: The figure presents the F-statistics of a Chow test on a joint test of significant differences for our main coefficients of interest (RRD × After, RDD × After, DDD × After, After × D (Democratic Trial Judge), RRD × After × D, RDD × After × D, DDD × After × D) before and after the date on the x-axis. The blue line depicts coefficients for published cases, and the red line for unpublished cases. The former reaches a maximum in the year 2001, while the former is relatively flat and smaller in magnitude.

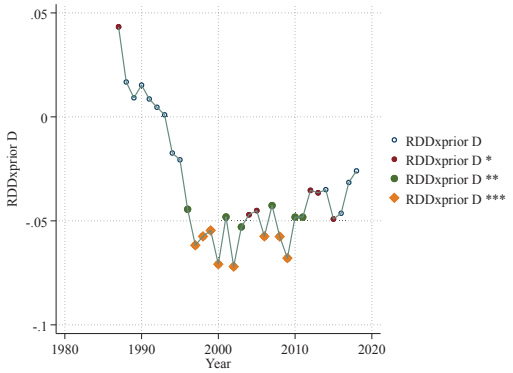
In Figure [B.1]. we see that the F-stat on the structural break peaks among published cases in 2001. For unpublished cases, the F-stats on a break are much lower, and do not show the same peak. For consistency of presentation, we will present results pre- and post-2000 for both published and unpublished cases.

In Figure [B.2] , we present estimates of one of our key coefficients of interest (DDD × Prior Judge = D) in a rolling five-year window from 1987 to 2018. We find the first significant effect in 1998 (hence the window spanning 1996 to 2000), with effects systematically significant post-2000, again corroborating the pre- vs. post-2000 split.

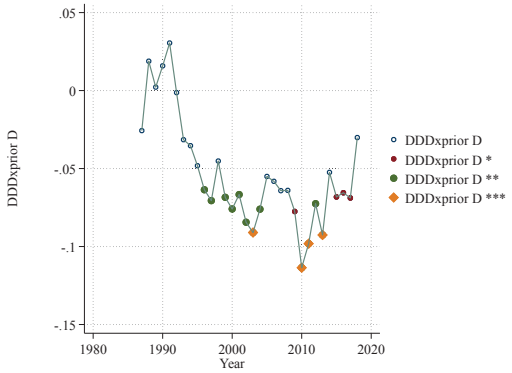
Figure B.2: Point estimates for the main coefficient of interest across rolling 5-year windows.



(a) RRD × Prior D



(b) RDD × Prior D



(c) DDD × Prior D

Notes: The figure presents the estimates for our main coefficient of interest for a 5-year window centered on the date on the x-axis. The symbols reflect statistical significance: hollow blue circle = not significant, red circle = 10%, green circle = 5%, diamond = 1%.

C Lasso Variable Selection and Estimation Procedure

A key concern with the main specification in this analysis is the potential presence of covariates related to the variables of interest, which could lead to omitted variable bias. In particular, we consider an additional set of covariates capturing identity-based characteristics of judges and panel composition—specifically gender and minority status—that may be correlated with political affiliation. To address this, we implement a double-selection Lasso approach following Belloni et al. (2014). Assume that the underlying baseline regression model is given by:

$$E[y \mid \boldsymbol{d}, \boldsymbol{x}] = \beta_0 + \boldsymbol{d}\alpha' + \boldsymbol{x}\beta' \tag{2}$$

Where y denotes the reversal outcome of the case. The vector $\mathbf{d}$ includes the variables of interest, fixed effects, and controls that are *not* subject to Lasso selection, being indexed by $j = 1, ..., J$. In our setting, this consists of six dummy variables capturing the political composition of the panel and its interaction with the prior judge's political affiliation. It also includes the case-level covariates from our main specification—namely, the political affiliation of the trial court judge, their seniority status (chief justice or senior justice), and measures of experience such as tenure and tenure squared, both for individual judges and the panel average. Fixed effects, as described in the main specification, are also included in this set.[23] Importantly, our parameters of interest are contained within the vector α.

In the component $\boldsymbol{x}$ of our equation, we include a comprehensive set of variables —the Lasso pickable ones—, incorporating panel composition along two additional dimensions as well as interactions between panel composition and judge characteristics. These potential controls are indexed by $p = 1, ..., P$. Beyond political composition, panel composition is also measured along gender composition and minority composition. These variables are interacted with three judge-level indicators: whether the previous judge is female, minority, or a Democrat. These interactions, along with judge-level covariates (e.g., female, minority) and background characteristics of panel composition, form our set of potentially eligible controls. The following table provides a detailed summary of all included interaction terms.

[23]For computational purposes, we demean the variables and run the double-selection Lasso estimations on their transformed versions, accounting for fixed effects.

Notably, it excludes the interaction of political composition with itself, as they are part of vector **d**.

Our double-selection lasso estimation algorithm then goes through the following steps:

1. We perform a linear Lasso with y on the left hand side and the set of P potential controls $\boldsymbol{x}$ on the right hand side. This corresponds to find a vector of coefficient estimates β minimizing the following:

$$\frac{1}{2N}(y - \boldsymbol{x}\beta')'(y - \boldsymbol{x}\beta') + \lambda \sum_{p=1}^{P} \|\beta_p\| \tag{3}$$

   For a given value of λ. We pick λ^* using the heteroskedastic plugin estimator developed in Binder and Malzman (2009) and Belloni et al. (2012), which is the default and recommended one in most statistical packages when looking for causal inference purposes. Let's define the set of selected variables in this step as $\tilde{\boldsymbol{x}}_y = \left\{ \boldsymbol{x}_p \in \boldsymbol{x} : \hat{\beta}_p^{lasso} \neq 0 \right\}$.

2. For each j fixed-covariate of interest included in $\boldsymbol{d}$, perform a similar linear lasso with d_j in the left-hand side and the entire vector $\boldsymbol{x}$ on the right hand side. Each of these lassos select another specific λ_j^* through the plugin estimator. We get a total of J separate vectors of selected covariates given by $\tilde{\boldsymbol{x}}_1, \tilde{\boldsymbol{x}}_2, ..., \tilde{\boldsymbol{x}}_J$.

3. Define $\hat{\boldsymbol{x}}$ as the distinct variables in the union of selected covariates in $\tilde{\boldsymbol{x}}_1, \tilde{\boldsymbol{x}}_2, ..., \tilde{\boldsymbol{x}}_J$ and $\tilde{\boldsymbol{x}}_y$.

4. Finally, fit a linear regression of y on $\hat{\boldsymbol{x}}$ and $\boldsymbol{d}$ and report estimates.

Table C.1: Sets of Interactions

Panel Composition Type	**Interacted with...**
Political (RRD, RDD, DDD)	Previous Female
	Previous Minority
Minority (NNMin, NMinMin + MinMinMin)	Previous Female
	Previous Minority
	Previous Democrat
Gender (FMM, FFM + FFF)	Previous Female
	Previous Minority
	Previous Democrat

The algorithm is run for each subsample—picking subsample-specific Lasso controls—and uses demeaned variables to account for fixed effects for computational efficiency reasons.