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JUDGES JUDGING JUDGES:
POLARIZATION IN THE U.S. COURTS OF APPEALS

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ABSTRACT

While prior literature has documented politicization in appellate decision-making, our focus — beyond replicating and extending this pattern — is on polarization, defined as differential treatment of trial judges who share the appellate panel members' political affiliations. Using a dataset of more than 400,000 cases from 1985 to 2020, we document both phenomena. Panels with more Democratic-appointed judges are systematically more likely to reverse throughout the sample period. Polarization, however, emerges significantly only post-2000 and, while most pronounced in published cases, is present in both published and unpublished cases once reversal and publication are modeled jointly using a bivariate probit. Against a baseline reversal rate of 39 percent, moving from an all-Republican to an all-Democratic panel post-2000 increases the conditional reversal rate of Republican-nominated trial judges by approximately 9 percentage points — a 23 percent relative increase — and this effect is attenuated by roughly one-third when the trial judge is also Democratic-nominated. Polarization is strongest in ideological cases and persists among judges appointed pre-2000, indicating behavioral shifts beyond appointment effects. We find no evidence of gender-based polarization.

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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 decisions. These phenomena are particularly relevant 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 — have ignited a debate on these trends.

Using a comprehensive dataset of over 400,000 federal appellate cases from the U.S. Circuit Courts of Appeals 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 appellate behavior beginning around 2000, coinciding with the period in which polarization intensified at the Supreme Court (Bonica and Sen, 2021). In the post-2000 period, Circuit Court panels increasingly favor trial judges who share their political affiliation. This alignment effect is observed in both published and unpublished cases, indicating that it is not confined to precedential decisions, and because published opinions shape legal doctrine, polarization along this margin influences the evolution of legal doctrine. The pattern is strongest in ideologically salient disputes and is observed even among judges appointed prior to 2000, suggesting that the change reflects evolving judicial behavior rather than solely shifts in the composition of appointments.

Politicization and polarization 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

¹Kastellec (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, 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, the 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 the 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 appreciated by prior work. Beyond direct effects, the literature shows that political ideology shapes outcomes indirectly through strategic behavior in response to the ideological composition of the entire circuit (Kim, 2009), the configuration of appellate panels (Revesz, 1997), and the collegial dynamics between judges (Kastellec, 2011). Berdejo and Chen

²Examples of significant appellate cases include antitrust cases such as *United States v. Microsoft* (D.C. Circuit, 1998), *SEC v. Texas Gulf Sulphur* (Second 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.

(2017) explore how election cycles further influence decision-making.

Recent work has examined non-political identity effects in judicial decision-making. In 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 align 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 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 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 four 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 to 2020. This comprehensive dataset allows us to track pairings of trial judges and appellate panels over time, enabling fine-grained controls through Circuit \times Year fixed effects, as well as Appellate Panel and Trial Court Judge fixed effects in our most

³The 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. [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.

stringent specifications to control for unobserved, time-invariant covariates.

Second, 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 more routine) separately, as published cases present a more visible arena for polarization.

Third, because appellate panels simultaneously decide whether to reverse a case and whether to publish the opinion, we model reversal and publication as jointly determined, correlated outcomes in a bivariate probit framework. While our OLS specifications document strong politicization and growing polarization—particularly post-2000, and more strongly in published cases—publication is itself part of the same deliberative process and may reflect political factors. Modeling the two decisions jointly allows us to account explicitly for this simultaneity and to assess whether politicization and polarization operate only through selection into precedent-setting cases or more broadly across all appellate decisions. While prior literature has examined publication decisions (e.g., Revesz (1997)) and reversal decisions (e.g., Songer et al. (1994); Epstein et al. (2011)) separately, the joint determination of the two has not, to our knowledge, been examined.

Fourth, we distinguish between doctrinal disagreement and partisan identity by comparing results across ideological and non-ideological cases. If polarization reflects political identity beyond judicial philosophy, we would expect alignment effects to appear in both types of cases. If, instead, judicial philosophy is the primary driver, such effects should be concentrated in ideologically salient disputes.

We begin with OLS specifications that include a rich set of fixed effects. We find clear evidence of politicization. Reversal rates rise systematically as the number of Democratic-nominated judges on the panel increases. Turning to our main focus, we find evidence of polarization. Panels are less likely to reverse trial judges who share their political affiliation. Both effects strengthen after 2000 and appear most pronounced in published decisions, which carry precedential weight and greater visibility.

We then turn to the joint modeling of reversal and publication. Once publication is treated as endogenous, polarization effects are present in both published and unpublished cases. In the post-2000 period, the bivariate probit estimates imply a baseline conditional reversal rate among published cases of approximately 39 percent for all-Republican panels

(RRR) reviewing Republican-nominated trial judges. Increasing the number of Democratic judges on the panel generates a clear gradient: conditional reversal rates rise by roughly 1.5 percentage points for RRD panels, 4.5 percentage points for RDD panels, and 8.9 percentage points for DDD panels. When the trial judge is Democratic-nominated, the gradient is attenuated, with increases of approximately 0.4, 2.7, and 6.0 percentage points for RRD, RDD, and DDD panels, respectively. The polarization effect is also reflected in within-panel differences: under DDD panels, Republican trial judges face conditional reversal rates about 1.6 percentage points higher than Democratic trial judges, while under RRR panels the pattern reverses, with Democratic trial judges facing reversal rates approximately 1.3 percentage points higher than Republican trial judges. Comparable differences emerge among unpublished cases.

We investigate several mechanisms that could underlie these results. First, we consider whether polarization reflects divergent judicial philosophy or partisan identity. Splitting the sample into ideological and non-ideological cases, we find that polarization effects are strongest in ideological disputes, where political stakes are most salient. This concentration suggests that alignment between appellate and trial judges operates more through differences in judicial philosophy tied to case content than through generalized partisan affinity. Second, we examine whether the observed increase in polarization reflects the appointment of more polarized judges or changes in behavior among judges over time. Restricting the post-2000 sample to judges nominated prior to 2000, we continue to find a polarization effect, suggesting that changes in judicial behavior contribute to the pattern (though this does not exclude cohort effects among later appointees).

To assess whether our findings are specific to partisan political alignment, we examine whether comparable effects arise along the dimension of gender. We do not find evidence of gender-based polarization: the gender composition of appellate panels does not significantly interact with either the trial judge's gender or political affiliation, and our political polarization results are unchanged when accounting for gender.

Our findings are important for several reasons. First, because appellate panels are randomly assigned, polarization introduces a source of variability in the outcome that is independent of case merits and beyond the control of litigants: the likelihood of reversal depends not only on the quality of the trial court decision but on the partisan composition of the reviewing panel. Second, since polarization operates through both the reversal and

publication margins, it shapes not only the outcomes of individual cases but the evolution of legal doctrine: the precedents that bind future courts and litigants within a circuit reflect, in part, the partisan composition of the appellate bench at the time those decisions were rendered. Third, and perhaps most strikingly, the persistence of polarization among judges appointed before 2000 indicates that the post-2000 shift is not solely attributable to changes in the composition of the bench. Judicial behavior itself appears to have changed, suggesting that broader societal polarization has altered the norms and practice of judicial behavior more broadly.

The paper is organized as follows. Section 2 outlines the institutional background of the Circuit Courts. Section 3 describes the construction of the dataset and presents summary statistics. Section 4 first presents the baseline OLS fixed-effects results; then Section 5 develops a joint model of reversal and publication decisions using a bivariate probit framework; and finally Section 6 explores extensions and interpretation. Section 7 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 appellate 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

⁴Article 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.

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

2.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.

2.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 they are reviewed by all active judges in the specific Circuit. The panels consist of active and senior judges, and in some cases may 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 case. 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 the disagreement. Once a panel reaches a decision, it can choose 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.

2.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 Yang \(2019\)](#)), including this paper, is that judges and cases are randomly assigned to circuit court panels. Some recent studies that examine this assumption ([Chilton and Levy, 2015](#); [Levy, 2017](#); [Fischman, 2011](#)) conclude that the assignment of judges to panels, even if not truly random, deviates from randomness for technical reasons that are generally independent of political polarization.

In Section [3.C](#), below, we present balance checks which confirm that appellate panels appear to be randomly assigned to cases.⁶

3 Data

3.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 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 of 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 U.S. federal court judges with life tenure. We use this

⁶Of course, we are unable to test whether appeals themselves are non-random since our dataset 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 and Year fixed effects.

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

directory to add information about judges beyond their names, such as gender, race, age, tenure, nominating president, and date of nomination, for both Circuit and District Court judges.

The merger between the 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 jurisdiction. They 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.⁸

Altogether, the 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. 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. As shown in Figure [1], this amounts to more than 10,000 cases in a typical year.

3.B Variables 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 and equal to 0 if the panel upheld the District Court’s ruling and the original ruling remained unchanged. We

⁸The merger yields about 810,000 cases. After excluding Federal Circuit and D.C. Circuit cases, and keeping only cases for which termination was on the merits, we are left with 750,000 cases for the period 1985 to 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). 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 (such as the 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, while the remaining 25 percent come from administrative agencies, original proceedings, and other miscellaneous filings (see uscourts.gov).

⁹We 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.

also observe whether the opinion was published (=1) or unpublished (=0),¹⁰ and examine the joint outcomes implied by these two.¹¹

We measure politicization through the political affiliation of the appellate panel judges and the trial judge, which we proxy using the political party 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, based on the party of the president who nominated them. This approach is widely used in the literature (Landes and Posner, 2009; Segal and Spaeth, 2002; Sunstein et al., 2004). Figure [3] depicts the evolution of the political composition of panels over time.

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, that is, 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 appellate panel and trial court judges affect the outcome, including sex and minority status. Both 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 define ideological cases using Cohen (2025)'s extension of the classification 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

¹⁰We 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.

¹¹The 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 those affecting the case's outcome (Wasby, 2004; Merritt and Brudney, 2001).

¹²The full list of key words includes: abortion, affirmative action, campaign finance, capital punishment, commercial speech, criminal procedure, establishment clause, federalism, free exercise clause, gender dis-

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)¹³, and time period (44 percent pre-2000, 56 percent post-2000).¹⁴

Our outcome of interest, which is whether the appellate panel reverses the trial court ruling, occurs in approximately 18 percent of cases overall.¹⁵ 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 approximately 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 approximately 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 increasing markedly after 2000. Additionally, 33 percent of panels have at least one minority judge, with a modest increase in the post-2000 period.¹⁶

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

crimination, race discrimination, Second Amendment (gun control), takings clause (property rights), and voting rights.

¹³The share of published cases was higher in the 1980s, declined in the 1990s and 2000s to approximately 30 percent, and fell further to below 20 percent after 2010.

¹⁴The number of cases in our dataset increases substantially over time. In the early years (prior to 1990), the annual number of cases is fewer than 5,000, whereas after 1990 it rises to approximately 12,000 cases per year.

¹⁵Reversal rates were substantially higher in the early part of the sample, reaching as high as 38 percent in the 1980s, and declined to below 20 percent from the 1990s onward.

¹⁶We 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.

3.C Balance Checks

We investigate the balance of our sample with respect to the political affiliation of the appellate panel and the trial court judge. We confirm that in our data, there is negligible evidence of a nonrandom assignment of appellate judges to panels.

In Figure [2] 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, below, 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 \times Year and district fixed effects. Within each of the three panels of the figure, 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 show 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 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 [2] corroborates the view that cases are randomly as-

signed 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.

4 Fixed Effects Specification

In this section, we present results using an OLS specification, which permits a rich set of fixed effects, including Circuit \times Year, district, appeal type, nature of the suit, and offense type, as well as Appellate Panel and Trial Judge fixed effects in our most stringent specifications. We report effects for the full sample and separately for the pre- and post-2000 periods.¹⁷ We also split the sample by published and unpublished cases. While this yields useful insights, splitting by publication status treats the publication decision as exogenous to the reversal decision. Since the appellate panel simultaneously decides whether to reverse and whether to publish, and since both decisions may be influenced by political composition and alignment, in Section 5 we turn to a bivariate probit framework that models reversal and publication as joint, correlated outcomes. This allows us to examine the role of politicization and polarization in the joint decision without conditioning on an endogenous outcome.

4.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

¹⁷The literature documents a rise in judicial polarization beginning around the early 2000s (e.g., [Sunstein et al. \(2004\)](#); [Bartels \(2009\)](#); [Bonica and Sen \(2021\)](#)). To confirm that a similar shift appears in our data, we conduct Chow-type tests for parameter stability using the bivariate probit specification from Section 5 that jointly models reversal and publication. For candidate break years between 1995 and 2003, we estimate a fully interacted specification that allows the key political coefficients to differ before and after each candidate year and then perform Wald tests of coefficient equality across periods, focusing on the polarization variables. The Wald statistics peak in the year 2000 ($\chi^2 = 15.29$, $p = 0.004$), with proximate years also showing weaker evidence of instability, suggesting a change in the relationship around this period. In practice, results are very similar when the sample is split at proximate years in the late 1990s or early 2000s.

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 judges are female or minority, and 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 \times 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).¹⁸ 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 the Circuit over time could induce strategic behavior by trial court judges. The richness of our data allows for the inclusion of more fine-grained fixed effects, which we present 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.

4.B Results

In Table [2], we present our 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).

¹⁸For 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.

4.B.1 Politicization

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.

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 discuss the interaction effects below). Panels with an increasing number of Democratic-nominated judges are increasingly likely to reverse rulings by Republican-nominated trial judges relative to 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 3.1 and 6.3 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.

We also examine heterogeneity by publication status. Published opinions carry precedential force and greater visibility, and thus provide a natural setting in which political alignment may be more likely to manifest. Among unpublished cases pre-2000, the RRD effect is not statistically significant, while the RDD and DDD effects are 1.4 and 4.4 percentage points, respectively. Among 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.

The contrast widens post-2000. Unpublished cases follow a pattern similar to the earlier period (1.4 and 3.4 percentage points for RDD and DDD), whereas published cases show a much steeper gradient: 2.4, 6.5, and 13.3 percentage point increases for RRD, RDD, and DDD panels, respectively (all significant at the 1 percent level).

4.B.2 Polarization

We next examine *polarization*, defined as the alignment between the political affiliation of the appellate panel and the trial judge, captured by the interaction terms between panel composition and trial judge affiliation (β_4 – β_6). These test whether panels moderate their reversal behavior when reviewing co-partisan trial judges. Prior work using more limited samples finds no evidence that the political affiliation of the trial judge affects appellate outcomes (Epstein et al. 2013; Berdejó 2013). Given our broader dataset and extended time window, we revisit this question.¹⁹

We first examine reversal patterns across politically homogeneous panels (DDD and RRR), focusing on differences by the trial judge’s political affiliation. In the full sample, DDD panels reverse Republican-nominated trial judges more often than Democratic-nominated trial judges, implying a polarization effect of roughly 1.6 percentage points ($(\beta_3 + \beta_6 + \beta_7) - (\beta_3)$). By contrast, RRR panels exhibit no statistically significant differential in reversal rates by trial judge affiliation. Thus, in the full sample, polarization appears primarily among Democratic panels.

Prior to 2000, these differences are small and not statistically significant. Post-2000, polarization becomes statistically significant and more symmetric: DDD panels reverse Republican trial judges at rates about 1.2 percentage points higher than Democratic trial judges, while RRR panels reverse Democratic trial judges at rates about one percentage point higher than Republican trial judges. These post-2000 polarization effects appear concentrated in published cases, a pattern we revisit in the next section.

Another way to view these results is from the perspective of the trial judge. For Republican-nominated trial judges, moving from an all-Republican (RRR) to an all-Democratic (DDD) panel increases the reversal rate by 6.3 percentage points. For Democratic-nominated trial judges, the corresponding increase, from a DDD to an RRR panel, is somewhat smaller, at 4.1 percentage points. Thus, while Democratic panels are generally more likely to reverse, this difference is attenuated when the trial judge shares the panel’s political affiliation, consistent with the negative interaction effect.

Turning to mixed panels, Table [2] column 1, we find no significant effects of RRD or

¹⁹Berdejó (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.

RDD panels in the full sample, but DDD panels are 1.6 percentage points less likely to reverse Democratic-nominated trial judges relative to a mean reversal rate of 17.9 percent. Splitting by time 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, and a DDD panel 2.2 percentage points less likely (significant at the 5 and 1 percent levels, respectively).

In columns 4–7, we further split by publication status. The pre-2000 results remain null. Post-2000, however, the effects are concentrated in published cases. An RDD panel reduces the reversal rate of a Democratic-nominated trial judge by 3.1 percentage points; the effect increases to -4.4 percentage points for RDD panels and -7 percentage points for DDD panels. Thus, although Democratic panels are generally more likely to reverse Republican trial judges, this tendency is roughly halved when the trial judge is Democratic-nominated.

Because published opinions carry precedential force and greater visibility, alignment effects may manifest more strongly in this subset. We address the joint determination of publication and reversal directly in Section 5 using a bivariate probit framework. As we show, polarization effects remain present once publication is treated as endogenous, indicating that the patterns documented here are not solely driven by selection into precedent-setting cases.

4.B.3 Extensions and Robustness

Other effects of interest in Table [2] include a reduction in reversals for female trial court judges and an increase for minority trial court judges (column 1, -0.7 and 1.7 percentage points, respectively, both significant at the 1 percent level). These effects are similar pre- and post-2000. Panels with at least one female judge are also somewhat less likely to reverse trial court decisions. In Section 6, we investigate whether gender alignment between appellate panels and trial judges operates similarly to political alignment, and whether multiple identity channels jointly influence outcomes, and, if so, which channel appears more influential.

In columns 8–10, we assess the robustness of our post-2000 results (column 3) to more stringent specifications. Column 8 includes trial judge fixed effects, identifying β_4 – β_6 from variation in appellate panel composition for a given trial judge. Column 9 instead includes appellate panel fixed effects, asking whether trial judge affiliation matters within a given panel. Column 10 includes both sets of fixed effects. In all cases, the interaction effects remain essentially unchanged.

In sum, we find evidence of both politicization and polarization in appellate decision-making: panel political composition affects reversal rates, and panels are less likely to reverse trial judges who share their political affiliation. These effects are present in the full sample, strengthen after 2000, and are most pronounced in post-2000 published cases.

5 Endogenizing the Publication Decision

As was previously discussed, appellate panels decide not only whether to reverse a trial court decision, but also whether to publish the resulting opinion. Publication determines which cases acquire precedential force and broader visibility, and thus constitutes a distinct and substantively important margin of judicial discretion. Because reversal and publication are determined contemporaneously by the same panel in the course of resolving the appeal (see, e.g., Wasby (2004); Merritt and Brudney (2001)), they represent related components of a single deliberative process.

We therefore model the panel’s joint decision over reversal and publication using a bivariate probit specification that permits correlation in the unobserved determinants of the two outcomes. This framework allows us to estimate the effects of panel composition and partisan alignment on each margin while accounting for their interdependence.²⁰ In doing so, we assess whether the politicization and polarization patterns documented above operate only through the reversal margin or more broadly across appellate decisions. To the best of our knowledge, the role of partisan alignment in the joint publication and reversal decision has not previously been examined in this way.²¹

As in the prior specifications, we present results separately for the pre- and post-2000

²⁰As a complementary discrete-choice specification, one could treat the four possible outcomes (affirmed/unpublished, affirmed/published, reversed/unpublished, reversed/published) as mutually exclusive categories. We estimate multinomial probit and logit models that impose this structure. The results (Tables [A.1] and [A.2]) closely resemble the OLS patterns documented above: politicization is present both pre- and post-2000, while polarization appears primarily in the post-2000 period and is concentrated in published cases. However, unlike the bivariate probit framework, the multinomial models do not explicitly model reversal and publication as two jointly determined, correlated binary decisions, and therefore do not directly account for the simultaneity of the two decisions. Formal tests of the Independence of Irrelevant Alternatives (IIA) assumption yield mixed results, consistent with well-known limitations of these procedures. See, for example, Fry and Harris (1996), Fry and Harris (1998), Cheng and Long (2007), and related simulation-based evaluations showing that both the Hausman-McFadden and Small-Hsiao tests can suffer from substantial size distortions and inconsistent outcomes depending on implementation and outcome structure.

²¹Boyd (2015) employs a multinomial choice framework to examine ideological distance and publication decisions, but does not focus on partisan alignment between appellate panels and trial judges within a joint modeling framework.

periods. The specification includes the same covariates as our regression in Section 4 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 included in our OLS specification above, prove to be computationally intractable.)

Table [3] presents marginal coefficients for the two outcomes, split pre- and post-2000. Pre-2000 Democratic judges are more likely to choose to reverse and to publish cases. None of the interactions between appellate panel composition and political identity of the trial court 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 significant and negative for RDD and DDD panels and for an indicator for a Democratic trial judge, and for the latter only the DDD interaction is significant.²²

While the coefficients on reversal and publication 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 in statistical significance. Using the estimated coefficients and the correlation coefficient allows us to estimate the marginal effects of our variables of interest for the four joint outcomes: not reversed and unpublished; reversed and unpublished; not reversed and published; and reversed and published. Results are presented in Table [4]

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 [3], this politicization in reversal behavior is not confined to published

²²While the sample size is too small to present annual estimates, splitting the results across a range of years suggests that the polarization effects are relatively stable from 2000 onward with respect to the reversal and publish decision, and trending upward for the reverse and unpublished decision.

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 not reverse and 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. 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 for reversed and published and reversed 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 less 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.

The joint marginal effects therefore confirm that polarization emerges sharply in the post-2000 period and operates through both the reversal and publication margins. To gauge magnitudes, we back out the implied conditional reversal probabilities from the bivariate probit. Using the post-2000 estimates in Table [4], the baseline probability of a published reversal is 0.075 and of a published non-reversal is 0.119, implying a conditional reversal rate among published cases of 38.7 percent—close to the OLS mean of 37.4 percent.

Holding the trial judge fixed at Republican, moving from an RRR to a DDD panel increases the implied conditional reversal rate from 38.7 to 47.5 percent, an increase of 8.9 percentage points. Performing the same exercise for a Democratic trial judge yields an increase from 39.9 to 46.0 percent, or 6.0 percentage points. Thus, politicization—measured as the DDD–RRR gradient—is evident for both types of trial judges, although somewhat smaller when the trial judge is Democratic.

Turning to polarization within panel type, under DDD panels the implied reversal rate is 47.5 percent for Republican trial judges and 46.0 percent for Democratic trial judges, a difference of -1.6 percentage points. Under RRR panels, the corresponding rates are 38.7 and 39.9 percent, a difference of +1.3 percentage points, yielding an approximately symmetric polarization effect. The sign reversal across panel types reflects the interaction between panel composition and trial judge partisanship.²³

A final supplemental result is presented in Appendix Table [A.4], where we 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 regarding the simultaneity of the publication and reversal decisions, since there is no evident sequencing of these.

6 Interpretation and Extensions

The polarization that we document may reflect two related but distinct mechanisms. One possibility is increasingly divergent judicial philosophies across political parties such that alignment effects are strongest in ideologically salient disputes where legal principles and policy stakes are contested. An alternative explanation is partisan affinity—judges exhibiting greater deference toward co-partisan trial judges based on shared political identity, independent of case content. While our empirical strategy cannot fully disentangle these mechanisms, we examine patterns that are informative about their relative importance. Specifically, we test whether alignment effects are concentrated in ideological cases, whether they persist among judges appointed prior to 2000 (suggesting behavioral change rather than appointment effects), and whether comparable alignment arises along the dimension of gender. Taken together, these analyses shed light on whether the polarization we observe reflects substantive divergence in judicial outlook or generalized identity-based alignment.

²³Although these magnitudes are somewhat smaller than their OLS counterparts in Table [2] discussed above, the qualitative patterns are unchanged and the effects large in magnitude.

6.A Ideological vs. Non-Ideological Cases

To shed light on whether polarization reflects divergent judicial philosophy or political affinity, in Table [5] we split our post-2000 results by ideological and non-ideological cases (as defined in Section 3). 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 more due to partisanship rather than polarization in legal outlook. One would expect differences in judicial philosophy to express themselves more strongly in ideological cases. In both ideological and non-ideological cases, the politicization effects are present: panels with more Democratic-nominated judges are more likely to reverse trial court decisions, whether published or unpublished. However, the polarization effects — captured by the interaction between panel composition and trial judge political affiliation — are concentrated in ideological cases. For ideological cases, the interactions are negative, statistically significant, and substantively large for both published and unpublished reversals. For example, DDD panels facing a Democratic trial judge are 2.95 percentage points less likely to choose a published reversal and 1.42 percentage points less likely to choose an unpublished reversal (approximately halving the Democratic panels’ increased tendency to reverse cases, both effects significant at the 1 percent level). The corresponding effects for non-ideological cases are smaller and not statistically significant. This pattern suggests that the alignment effects we document are driven more by differences in judicial philosophy tied to case content than by generalized partisan affinity.

6.B Behavioral Change vs. Appointment Effects

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 [6], we restrict the sample to post-2000 cases decided by judges who were nominated prior to 2000, excluding panels with judges who became inactive prior to 2000 or who were nominated post-2000.²⁴ The politicization effects persist in this restricted sample: DDD panels are significantly more likely to reverse trial court decisions, both published and unpublished. The polarization effects also

²⁴While 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.

remain. DDD panels are 1.89 percentage points less likely to choose a published reversal and 1.83 percentage points less likely to choose an unpublished reversal when the trial judge is Democratic-nominated (both significant at the 1 percent level). RRR panels (reflected in the coefficient for TJ Democrat) are more likely to reverse and publish Democratic trial judges (1.46 percentage points, significant at the 1 percent level). Hence, our results suggest that, at least in part, the same judges have become more partisan over time. While this does not rule out cohort effects among later appointees, it indicates that the post-2000 shift cannot be attributed solely to changes in judicial appointments.

6.C Gender Identity

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 Figure [4] we see that the number of female judges has increased. As shown in Figure [5], these newly appointed female judges are disproportionately Democratic. Hence, it is possible that the interaction of appellate panel political composition and trial judge political affiliation could be picking up gender-based identity effects.

In Table [7], we investigate whether gender identity matters alongside political identity using the bivariate probit framework, focusing on post-2000 cases. We introduce indicators for the gender composition of the appellate panel²⁵ and interactions of both political and gender composition with the political affiliation and gender of the trial judge. The political polarization effects remain: the interactions between panel political composition and trial judge political affiliation are negative and significant, with magnitudes similar to our earlier results. In contrast, the gender composition of the panel does not significantly interact with either the trial judge’s gender or the trial judge’s political affiliation. Interactions between panel political composition and the trial judge’s gender show some effects for DDD panels on published reversals, but the gender composition interactions remain insignificant.

The absence of gender-based polarization is notable given the strong correlation between gender and political affiliation among judges. While the increasing share of female judges has been disproportionately among Democratic nominees, gender identity does not appear to matter for reversal decisions over and above political identity, reinforcing the centrality

²⁵We group together panels with two and three female judges since there are very few panels with three female judges.

of political partisanship in the patterns we document.

7 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. 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. We also provide new results on political polarization, the lack of effects based on gender-identity polarization, 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 the post-2000 period, consistent with evidence pointing to a rise in politicization around the year 2000. In our OLS results, the polarization effect is strongest among published cases post-2000 and absent among unpublished cases. However, since the publication decision is itself endogenous — made simultaneously with the reversal decision and potentially influenced by the same political factors — this descriptive pattern may not reflect the true distribution of polarization across published and unpublished cases. When we model reversal and publication as joint outcomes using a bivariate probit framework, we find that the polarization effect is present in both published and unpublished cases. Post-2000, Democratic-leaning panels are more likely to reverse trial court decisions, whether published or unpublished. However, this greater propensity to reverse is substantially attenuated when the trial judge is also Democratic-nominated. In other words, polarization partially offsets Democratic panels' baseline inclination toward reversal when reviewing co-partisan trial judges. A similar pattern holds for Republican-leaning panels, although the magnitude is smaller, reflecting their lower overall propensity to reverse. The bivariate probit results also reveal that polarization is most pronounced in ideological cases, where partisan considerations are most salient, and that it is present among judges appointed before the period of heightened polarization, pointing to behavioral shifts rather than solely appointment effects. Turning to gender identity, we do not observe effects over and above

politics.

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 phenomenon to the same extent.

Our bivariate probit results show that polarization effects are present in both published and unpublished cases post-2000, with broadly comparable magnitudes. The presence of the effect in unpublished cases suggests that polarization does not operate solely through strategic publication behavior. At the same time, the somewhat stronger patterns observed in published cases are consistent with publication serving as an additional discretionary margin at the appellate level. While we cannot disentangle these channels directly, the evidence indicates that polarization reflects behavioral changes within the appellate process rather than merely selection into precedential opinions.

There are two mechanisms that could underlie our findings: changes in the behavior of trial judges and changes in the behavior of appellate judges. If trial judges have become more polarized, their decisions may be more closely aligned with co-partisan appellate panels, resulting in lower reversal rates along partisan lines. Because trial judges cannot anticipate whether their decisions will be appealed, the composition of the appellate panel, or whether a case will ultimately be published, a trial-driven mechanism would be expected to generate similar polarization patterns in both published and unpublished cases. Alternatively, the pattern may reflect changes in the behavior of appellate judges. Appellate panels may have become increasingly reluctant to reverse co-partisan trial judges or more willing to reverse opposing-party judges. In this case, polarization could arise either broadly across all cases or selectively in published decisions if panels use precedential opinions to reinforce ideological positions. The fact that we observe polarization in both published and unpublished cases is therefore consistent with either mechanism, but it implies that a trial-judge-driven

explanation cannot be ruled out.

Our results are important for several reasons. First, the findings underscore that changes in the partisan composition of the judiciary have long-lasting behavioral consequences. Political shifts in judicial appointments translate into systematic differences in case outcomes; moreover, as evidenced by the increased polarization among judges appointed pre-2000 in the post-2000 period, appointment cycles are further amplified by shifts in the behavior of sitting judges. Second, to the extent that appellate review is intended to reduce dispersion in trial-level decision-making, the persistence of polarization at the appellate level implies limits to that attenuating role. These findings also carry broader implications for institutional design — implications that bear directly on panel assignment, publication practices, and the evolution of legal doctrine.

If, as we find, polarization effects are concentrated in ideological cases, institutional responses might focus less on panel assignment per se and more on transparency and doctrinal formation. Because polarization operates through both reversal and publication margins, and because publication determines which decisions become binding precedent, publication practices may warrant closer attention in a polarized environment. For example, greater explanation in unpublished opinions involving ideologically salient issues, or more open reliance on unpublished decisions as persuasive authority, could reduce asymmetries between published and unpublished cases. To the extent that polarization operates partly through discretionary publication choices, transparency and citation practices may influence how strongly partisan alignment shapes the evolution of doctrine.

A related implication concerns random panel assignment. Randomization is designed to safeguard fairness and prevent strategic judge selection. Yet when partisan alignment materially affects reversal rates and publication decisions, the random draw of a panel may introduce systematic variability in outcomes and precedent formation. This does not undermine the value of randomness, but it suggests that its consequences change in a polarized environment: an institution designed to promote neutrality may generate greater dispersion in outcomes when ideological differences are pronounced.

Taken together, our findings highlight how institutional design interacts with political polarization. While we do not advocate specific reforms, the interaction between polarization, publication practices, and panel assignment underscores the importance of transparency and the structure of appellate decision-making in shaping the predictability and development of

the law.

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

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

Variable	All Cases			Trial Judge		Unpublished		Published	
	All (1)	Pre-2000 (2)	Post-2000 (3)	Republican (4)	Democrat (5)	Pre-2000 (6)	Post-2000 (7)	Pre-2000 (8)	Post-2000 (9)
Panel C: Trial Judge Characteristics									
TJ Democrat	0.41 (0.49)	0.36 (0.48)	0.45 (0.50)	0.00 (0.00)	1.00 (0.00)	0.35 (0.48)	0.44 (0.50)	0.38 (0.48)	0.48 (0.50)
TJ Female	0.17 (0.37)	0.11 (0.31)	0.22 (0.41)	0.11 (0.31)	0.26 (0.44)	0.11 (0.32)	0.22 (0.41)	0.11 (0.31)	0.22 (0.41)
TJ Minority	0.14 (0.35)	0.11 (0.31)	0.17 (0.37)	0.09 (0.29)	0.21 (0.41)	0.11 (0.32)	0.17 (0.37)	0.11 (0.31)	0.17 (0.38)
TJ Chief	0.15 (0.36)	0.17 (0.38)	0.14 (0.35)	0.16 (0.37)	0.14 (0.34)	0.17 (0.37)	0.14 (0.35)	0.18 (0.38)	0.14 (0.35)
TJ Senior	0.27 (0.45)	0.23 (0.42)	0.31 (0.46)	0.26 (0.44)	0.29 (0.45)	0.24 (0.43)	0.31 (0.46)	0.22 (0.42)	0.30 (0.46)
TJ Age	63.18 (9.92)	61.13 (9.57)	64.78 (9.90)	62.75 (9.90)	63.80 (9.92)	61.21 (9.58)	64.81 (9.92)	61.00 (9.55)	64.68 (9.81)
TJ Tenure	13.77 (8.74)	11.85 (7.31)	15.27 (9.44)	13.69 (8.49)	13.88 (9.08)	11.97 (7.30)	15.20 (9.48)	11.62 (7.32)	15.55 (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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All Cases	All Cases Pre-2000	All Cases Post-2000	Unpublished Pre-2000	Unpublished Post-2000	Published Pre-2000	Published Post-2000	All Cases Post-2000 Prior-Judge Fixed Effects	All Cases Post-2000 Panel Fixed Effects	All Cases Post-2000 Prior-Panel Fixed Effects
RRD	0.010*** (0.003)	0.011*** (0.004)	0.007 (0.004)	0.005 (0.003)	0.002 (0.005)	0.016** (0.007)	0.024*** (0.009)	0.008* (0.005)		
RDD	0.031*** (0.004)	0.032*** (0.005)	0.027*** (0.005)	0.014*** (0.005)	0.014*** (0.005)	0.040*** (0.008)	0.065*** (0.010)	0.027*** (0.005)		
DDD	0.063*** (0.005)	0.065*** (0.009)	0.058*** (0.007)	0.044*** (0.007)	0.034*** (0.006)	0.056*** (0.014)	0.133*** (0.014)	0.058*** (0.007)		
RRD x TJ Dem	-0.005 (0.004)	-0.004 (0.005)	-0.006 (0.005)	-0.001 (0.004)	0.004 (0.005)	-0.006 (0.009)	-0.031** (0.012)	-0.008 (0.006)	-0.009* (0.005)	-0.011* (0.006)
RDD x TJ Dem	-0.005 (0.004)	0.003 (0.006)	-0.011** (0.005)	0.006 (0.006)	0.002 (0.005)	-0.010 (0.012)	-0.044*** (0.012)	-0.012** (0.006)	-0.011** (0.005)	-0.013** (0.006)
DDD x TJ Dem	-0.016*** (0.006)	-0.006 (0.010)	-0.022*** (0.007)	0.001 (0.010)	-0.005 (0.008)	-0.026 (0.018)	-0.070*** (0.018)	-0.020*** (0.007)	-0.022*** (0.007)	-0.021*** (0.007)
TJ Democrat	0.003 (0.003)	-0.002 (0.004)	0.010** (0.005)	-0.005 (0.004)	-0.002 (0.005)	0.007 (0.005)	0.035*** (0.010)	0.012** (0.010)		
TJ Female	-0.007*** (0.002)	-0.006** (0.003)	-0.007*** (0.002)	-0.005 (0.004)	-0.006*** (0.002)	-0.005 (0.006)	-0.010* (0.005)	-0.007*** (0.002)		
TJ Minority	0.017*** (0.002)	0.015*** (0.004)	0.018*** (0.003)	0.014*** (0.003)	0.016*** (0.002)	0.024*** (0.007)	0.032*** (0.006)	0.018*** (0.003)		
TJ Chief	-0.005** (0.002)	-0.006** (0.003)	-0.004 (0.003)	-0.002 (0.003)	-0.003 (0.003)	-0.009* (0.006)	-0.010 (0.007)	-0.004 (0.003)	-0.004 (0.003)	-0.003 (0.003)
TJ Senior	0.001 (0.002)	-0.002 (0.004)	0.003 (0.003)	-0.005 (0.004)	0.003 (0.003)	0.004 (0.007)	-0.003 (0.007)	0.005 (0.004)	0.001 (0.003)	0.003 (0.004)
TJ Tenure	0.001*** (0.000)	0.002*** (0.000)	0.000 (0.000)	0.002*** (0.000)	-0.000 (0.000)	0.001 (0.001)	0.000 (0.001)	-0.013*** (0.005)	0.000 (0.000)	-0.010** (0.005)
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** (0.002)	-0.007* (0.004)	-0.002 (0.002)	-0.003 (0.003)	0.002 (0.002)	-0.004 (0.006)	-0.007 (0.005)	-0.002 (0.002)		
Panel Minority > 0	-0.007*** (0.002)	-0.015*** (0.004)	-0.004 (0.003)	-0.008*** (0.003)	-0.003 (0.002)	-0.017*** (0.006)	0.003 (0.005)	-0.003 (0.003)		
Panel Tenure	0.000** (0.000)	0.001*** (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.002*** (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.563*** (0.199)	-0.574*** (0.195)
Observations	400,513	175,856	224,648	113,672	175,328	62,166	49,304	224,589	221,959	221,897
R-squared	0.055	0.064	0.050	0.026	0.035	0.050	0.061	0.063	0.178	0.190
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	0.172	0.171	0.171

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: Marginal Effects - Single Equations

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

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 covariates as in Table [2]. 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 4: Marginal Effects Bivariate Probit

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

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 covariates as in Table [2]. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit \times 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 5: Marginal Effects Bivariate Probit - Ideology, Post-2000

Outcome	Non ideological cases				Ideological cases			
	Unpublished		Published		Unpublished		Published	
	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed	Non Reversed	Reversed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RRD	.0048 (.0064) [.4544]	.0038 (.004) [.3421]	-.0067 (.0041) [.1078]	-.002 (.0023) [.3994]	-.005 (.0123) [.6851]	.0068* (.0039) [.0849]	-.0071 (.0081) [.3784]	.0054 (.0063) [.3933]
RDD	-.0065 (.0082) [.4252]	.0141*** (.0044) [.0013]	-.0094** (.0045) [.0353]	.0018 (.003) [.5529]	-.0168 (.0141) [.2337]	.0202*** (.0038) [0]	-.02** (.0082) [.0149]	.0166** (.0073) [.023]
DDD	-.0253** (.01) [.011]	.033*** (.0054) [0]	-.0147*** (.0049) [.0028]	.0071* (.0039) [.0687]	-.0567*** (.018) [.0017]	.0402*** (.0068) [0]	-.0268*** (.0101) [.0083]	.0432*** (.0108) [.0001]
RRD x TJ Dem	-.0043 (.0067) [.5202]	-.0012 (.0051) [.8134]	.0039 (.0045) [.393]	.0016 (.0024) [.4975]	.0215** (.0105) [.0411]	-.0058 (.0049) [.2396]	-.0032 (.0086) [.7116]	-.0126** (.0056) [.0241]
RDD x TJ Dem	.0036 (.0071) [.6135]	-.0032 (.0048) [.5002]	.0008 (.0045) [.8504]	-.0012 (.0025) [.6325]	.031*** (.0109) [.0043]	-.0081* (.0044) [.0652]	-.005 (.0077) [.5144]	-.0178*** (.0057) [.0019]
DDD x TJ Dem	.0097 (.0086) [.2582]	-.0083 (.0058) [.1496]	.0019 (.0051) [.7062]	-.0033 (.003) [.2701]	.0525*** (.0125) [0]	-.0142*** (.0053) [.0077]	-.0088 (.0095) [.3569]	-.0295*** (.0063) [0]
TJ Democrat	-.0062 (.0061) [.3142]	.0016 (.0046) [.7228]	.0023 (.004) [.5651]	.0022 (.0022) [.3094]	-.0399*** (.0095) [0]	.0061 (.0045) [.1733]	.0121 (.0076) [.1121]	.0217*** (.0053) [0]
\widehat{Prob}	.77	.095	.083	.052	.602	.08	.196	.123
	N = 152169 $\hat{\rho} = .49$ $SE(\hat{\rho}) = (.0083)$				N = 72491 $\hat{\rho} = .514$ $SE(\hat{\rho}) = (.0127)$			

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 covariates as in Table [2]. The second row reports the corresponding standard errors (in parentheses, clustered at the Circuit \times 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. All cases are post 2000.

Table 6: Marginal Effects Bivariate Probit Post-2000 - For Judges Appointed pre-2000

Outcome	Unpublished		Published	
	Non Reversed (1)	Reversed (2)	Non Reversed (3)	Reversed (4)
RRD	.0001 (.015) [.995]	.0046 (.0056) [.4059]	-.0057 (.0105) [.5881]	.001 (.0067) [.8815]
RDD	-.0194 (.0176) [.2704]	.0128** (.0063) [.0407]	-.0047 (.0114) [.6819]	.0113 (.0082) [.1662]
DDD	-.057*** (.0211) [.0068]	.0258*** (.0077) [.0008]	-.0003 (.0127) [.9784]	.0315*** (.0109) [.004]
RRD x TJ Dem	.0096 (.0109) [.3774]	-.0088 (.0055) [.1073]	.0055 (.0071) [.438]	-.0063 (.0053) [.2425]
RDD x TJ Dem	.0133 (.0115) [.2488]	-.0104** (.0052) [.0444]	.0053 (.007) [.4532]	-.0082 (.0055) [.1395]
DDD x TJ Dem	.0347** (.014) [.013]	-.0183*** (.0062) [.0031]	.0025 (.0091) [.7838]	-.0189*** (.0064) [.0032]
TJ Democrat	-.0278** (.0113) [.0142]	.0112** (.0055) [.0426]	.002 (.0075) [.7874]	.0146*** (.0056) [.0088]
\widehat{Prob}	.665	.087	.152	.096
N = 74293 $\hat{\rho} = .508$ $SE(\hat{\rho}) = (.0112)$				

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 covariates as in Table [2]. 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. All cases are post 2000.

Table 7: Marginal Effects, Bivariate Probit, including Indicators Female Judges

Outcome	Unpublished		Published	
	Non Reversed (1)	Reversed (2)	Non Reversed (3)	Reversed (4)
RRD	-.0001 (.0076) [.9889]	.0057* (.003) [.06]	-.0061 (.0049) [.2107]	.0006 (.0032) [.8597]
RDD	-.0105 (.0091) [.2488]	.017*** (.0031) [0]	-.0122** (.0052) [.0189]	.0057 (.0038) [.139]
DDD	-.0364*** (.0114) [.0014]	.0364*** (.0042) [0]	-.0169*** (.0063) [.0069]	.0169*** (.0053) [.0015]
MMF	.0096* (.0051) [.0573]	.0001 (.002) [.9669]	-.0058** (.003) [.0497]	-.0039* (.0021) [.0681]
MMF+FFF	.0026 (.0072) [.7128]	.0041* (.0024) [.0908]	-.006 (.0044) [.1744]	-.0007 (.003) [.8081]
RRD x TJ Dem	.0049 (.0062) [.4252]	-.0038 (.0037) [.307]	.0012 (.0044) [.7842]	-.0023 (.0026) [.3705]
RDD x TJ Dem	.0111* (.0065) [.0896]	-.0056 (.0035) [.1127]	-.0006 (.0044) [.8979]	-.005* (.0027) [.0698]
DDD x TJ Dem	.0221*** (.0076) [.0035]	-.0105** (.0046) [.0232]	-.0019 (.0054) [.7296]	-.0098*** (.0031) [.0018]
(FMM) x TJ Dem	-.0005 (.0045) [.9064]	-.0013 (.0023) [.5813]	.0017 (.0031) [.58]	.0001 (.0019) [.9651]
(FFM + FFF) x TJ Dem	.0039 (.0065) [.5563]	-.0031 (.0031) [.3169]	.0011 (.0045) [.8119]	-.0019 (.0027) [.4898]
(FMM) x TJ Fem	-.0056 (.0056) [.3182]	.0008 (.0028) [.762]	.0024 (.0035) [.4905]	.0023 (.0024) [.3305]
(FFM + FFF) x TJ Fem	.0046 (.0072) [.5176]	.0042 (.0038) [.2757]	-.0073 (.0047) [.1257]	-.0016 (.003) [.6054]
RRD x TJ Fem	.0099 (.0077) [.1965]	-.0009 (.0044) [.8424]	-.005 (.0051) [.328]	-.004 (.0032) [.2102]
RDD x TJ Fem	.0135* (.0078) [.0848]	-.002 (.0044) [.6489]	-.0059 (.0051) [.2425]	-.0055* (.0033) [.0901]
DDD x TJ Fem	.0186** (.0092) [.0433]	-.0023 (.0052) [.666]	-.0088 (.0059) [.1374]	-.0076** (.0038) [.0459]
TJ Democrat	-.0189*** (.0066) [.0045]	.0054 (.0036) [.1323]	.0054 (.0043) [.2135]	.0081*** (.0029) [.0046]
TJ Female	-.002 (.0079) [.8036]	-.0048 (.0042) [.253]	.0065 (.0051) [.1999]	.0003 (.0034) [.9387]
\widehat{Prob}	.715	.091	.119	.075

N = 224660
 $\hat{\rho} = .507$
 $SE(\hat{\rho}) = (.0084)$

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 Circuit, Year and appeal type fixed effects, and covariates as in Table [2]. 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. All cases are post 2000.39

Figure 1: Number of Cases Over Time

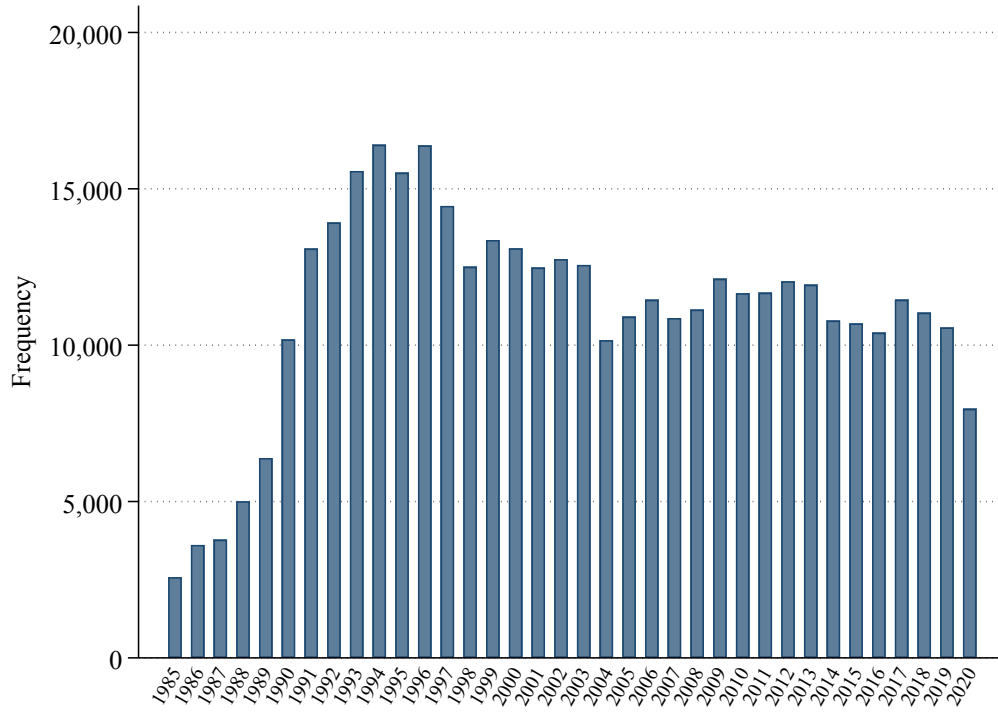
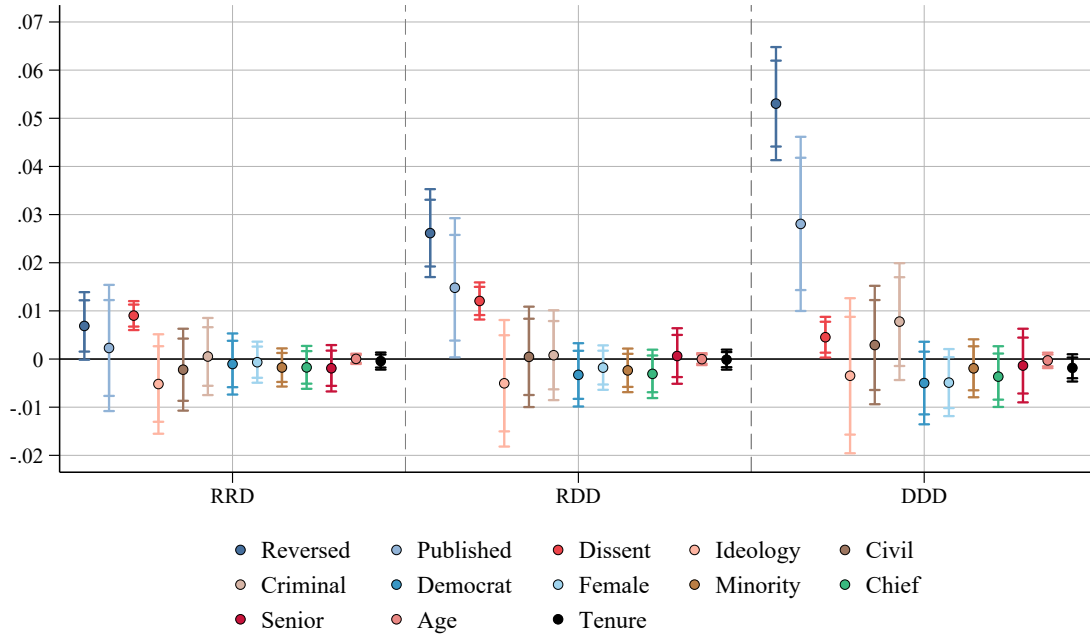


Figure 2: Balance Checks



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 3: Political Composition Over Time

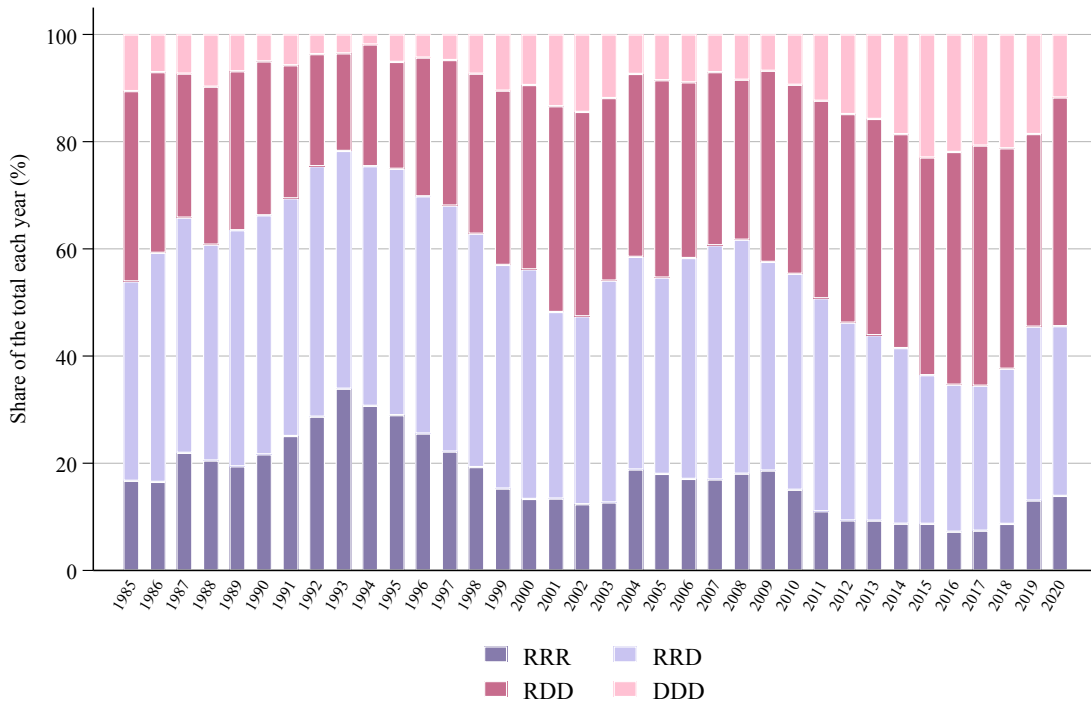


Figure 4: Gender Composition Over Time

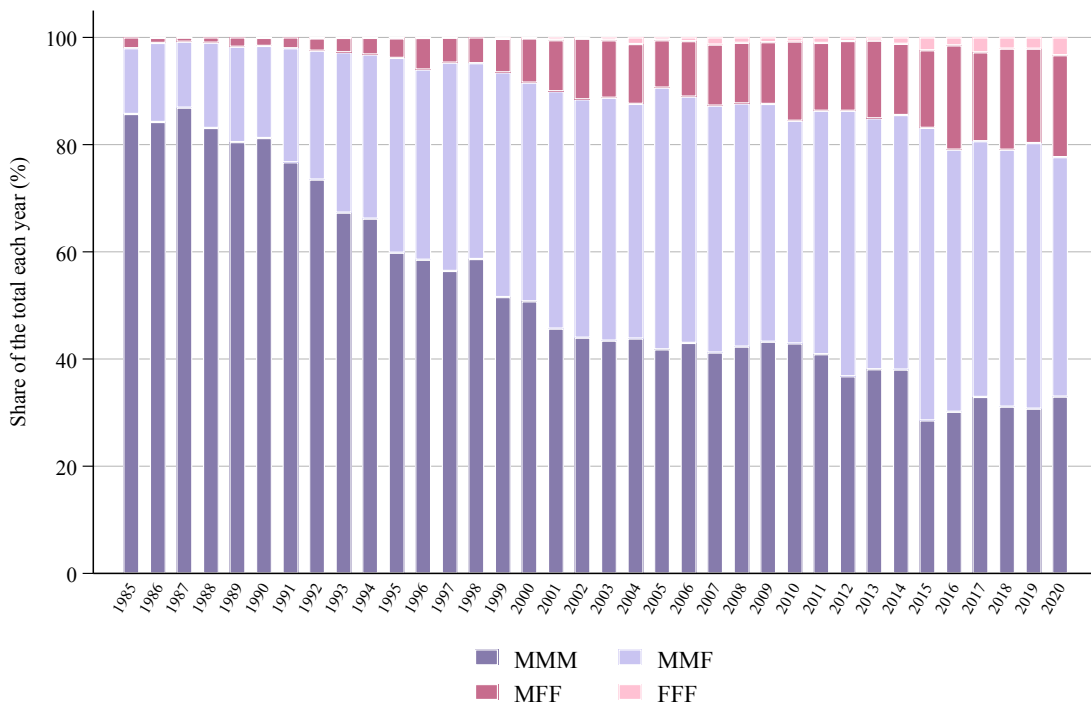


Figure 6: Panel Composition by Minority Status and by Gender

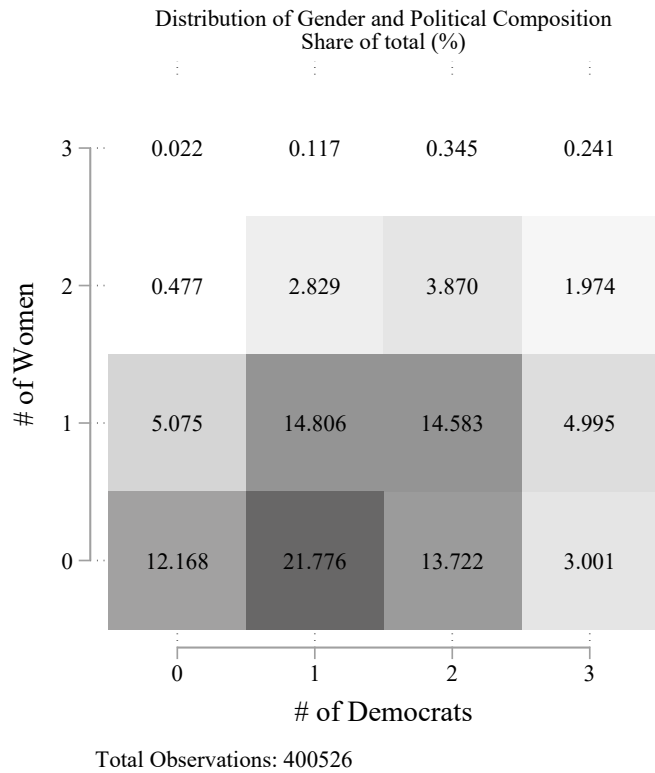


Figure 7: Share of Reversed and Published Cases Over Time

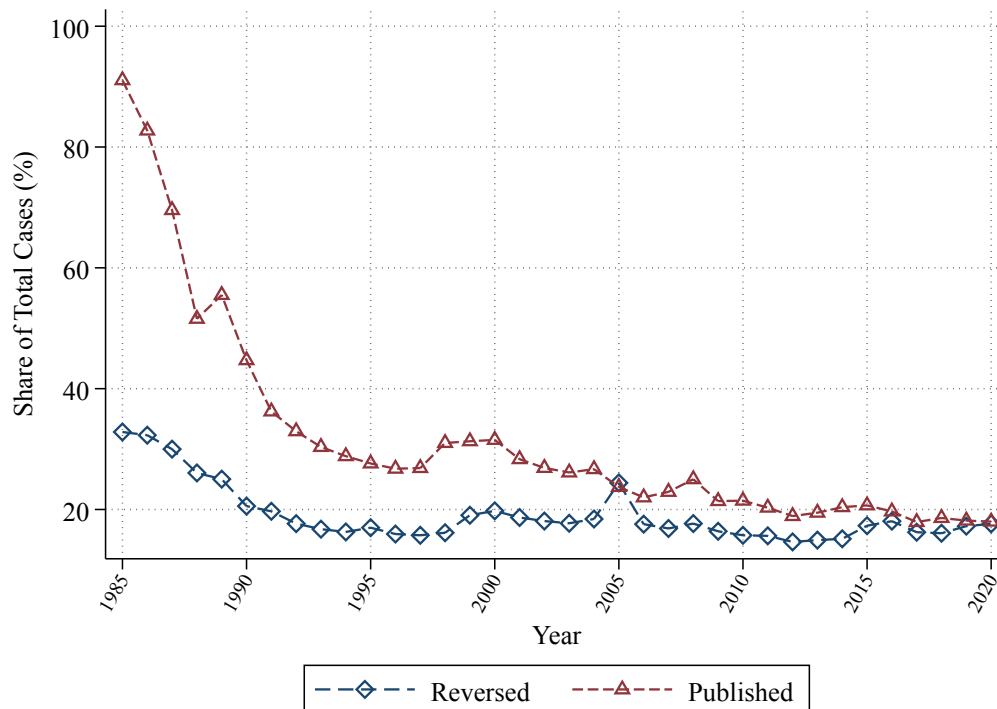
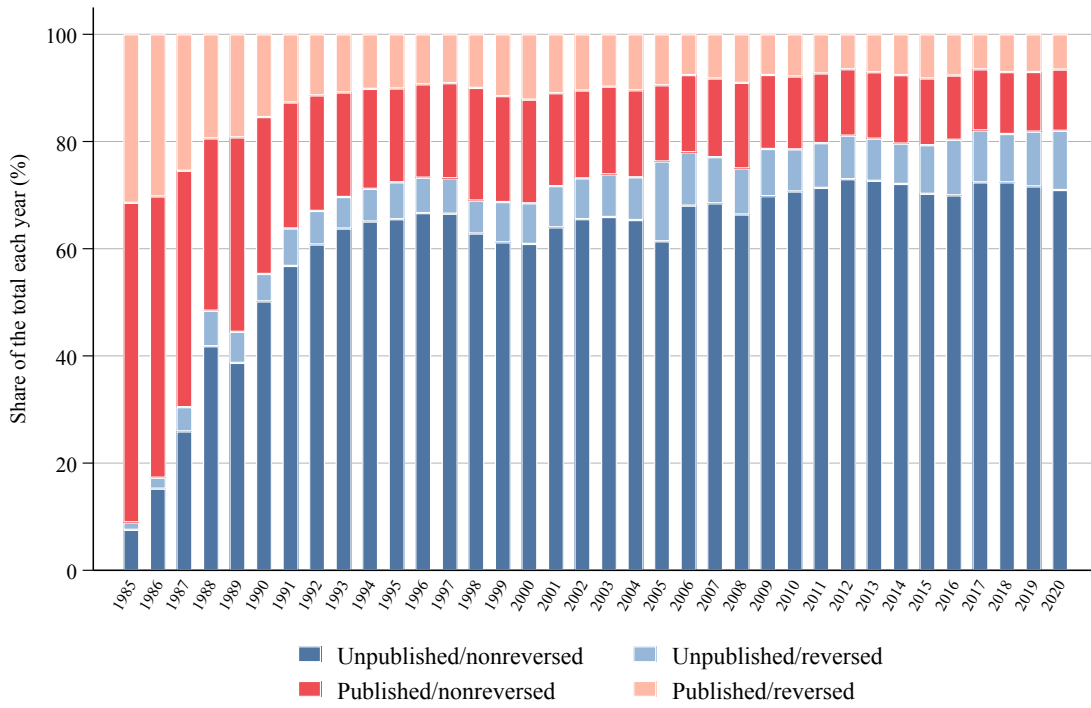


Figure 8: Composition of Reversed and Published Cases Over Time



A Supplemental Tables and Figures

Table A.1: Marginal Effects for a Multinomial Probit on Publication and Reversal

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

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

Outcome	Pre-2000				Post-2000			
	Unpublished		Published		Unpublished		Published	
	Non Reversed (1)	Reversed (2)	Non Reversed (3)	Reversed (4)	Non Reversed (5)	Reversed (6)	Non Reversed (7)	Reversed (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.4: 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 (0.005)	0.009 (0.007)	-0.005 (0.006)	0.005 (0.007)	-0.009 (0.006)	0.003 (0.007)	0.005 (0.012)	0.005 (0.012)		
RDD	0.019*** (0.005)	0.029*** (0.008)	0.001 (0.007)	0.017** (0.007)	-0.009 (0.006)	0.030*** (0.010)	0.017 (0.013)	0.018 (0.013)		
DDD	0.034*** (0.007)	0.056*** (0.013)	0.012 (0.008)	0.037*** (0.011)	-0.011* (0.007)	0.035* (0.019)	0.036** (0.016)	0.038** (0.017)		
RRD x TJ Dem	-0.002 (0.004)	-0.003 (0.005)	-0.001 (0.005)	0.000 (0.005)	0.005 (0.005)	-0.007 (0.010)	-0.012 (0.015)	-0.015 (0.015)	-0.010 (0.019)	-0.014 (0.020)
RDD x TJ Dem	0.001 (0.004)	0.009* (0.006)	-0.004 (0.005)	0.015*** (0.005)	0.004 (0.005)	-0.013 (0.013)	-0.027** (0.013)	-0.030** (0.014)	-0.020 (0.018)	-0.030 (0.020)
DDD x TJ Dem	-0.004 (0.005)	0.016* (0.009)	-0.010* (0.006)	0.025** (0.010)	0.003 (0.006)	-0.009 (0.019)	-0.040** (0.017)	-0.044** (0.018)	-0.040* (0.022)	-0.052** (0.024)
TJ Democrat	-0.001 (0.003)	-0.007 (0.004)	0.004 (0.005)	-0.011** (0.005)	-0.003 (0.005)	0.011 (0.009)	0.023** (0.011)		0.020 (0.016)	
TJ Female	-0.002 (0.002)	-0.002 (0.004)	-0.003 (0.002)	-0.001 (0.003)	-0.002 (0.002)	0.004 (0.009)	0.003 (0.006)		0.006 (0.007)	
TJ Minority	-0.004* (0.002)	-0.004 (0.004)	-0.004 (0.003)	-0.005 (0.004)	-0.005** (0.003)	-0.021** (0.009)	-0.021*** (0.006)		-0.020*** (0.007)	
TJ Chief	-0.001 (0.002)	-0.004 (0.003)	0.002 (0.002)	-0.004 (0.003)	0.004* (0.002)	-0.000 (0.007)	0.005 (0.007)	0.000 (0.009)	-0.002 (0.008)	-0.011 (0.010)
TJ Senior	0.001 (0.002)	-0.000 (0.003)	0.002 (0.003)	-0.000 (0.003)	0.002 (0.003)	0.010 (0.009)	-0.001 (0.007)	-0.000 (0.009)	-0.015* (0.008)	-0.015 (0.011)
TJ Tenure	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.001*** (0.000)	-0.000 (0.001)	0.002** (0.001)	-0.038*** (0.010)	0.003*** (0.001)	-0.023 (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*** (0.003)	-0.011** (0.005)	-0.008*** (0.003)	-0.007* (0.004)	-0.005* (0.003)	-0.020*** (0.007)	-0.020*** (0.006)	-0.020*** (0.006)		
Panel Minority > 0	-0.009*** (0.003)	-0.012** (0.005)	-0.008** (0.003)	-0.007 (0.005)	-0.006** (0.003)	-0.010 (0.008)	-0.010 (0.006)	-0.009 (0.006)		
Panel Tenure	0.002*** (0.000)	0.003*** (0.001)	0.001** (0.000)	0.003*** (0.001)	0.001** (0.000)	0.003*** (0.001)	0.001*** (0.001)	0.002*** (0.001)	0.417 (0.409)	0.355 (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.