

# Political Appointments and Outcomes in Federal District Courts\*

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## Abstract

Using an original dataset of around 70,000 civil rights cases heard by nearly 200 judges, we study the effect of presidential appointments to federal district courts. We provide the first causal estimates of whether lawsuits end differently depending on their assignment to either a Democratic or a Republican appointed judge. We show Republican appointees cause fewer settlements and more dismissals, favoring defendants by around 5 percentage points. We estimate a similarly sized effect for a sample of civil rights appeals heard in the Ninth Circuit, raising questions about the conventional wisdom that politics matters more at higher levels of the judicial hierarchy. We also find that the effect in district courts has increased over time. For cases filed during the Obama presidency, Republican appointees caused pro-defendant outcomes in 7.4% more cases than Democratic appointees. Our results suggest that district courts are an important—though neglected—subject of research for political scientists.

**Keywords:** judicial politics, federal courts, trial courts, causal inference

*Data and supporting materials necessary to reproduce the numerical results in the article are available in the Journal of Politics Dataverse (<https://dataverse.harvard.edu/dataverse/jop>) and at the website of the corresponding author (<https://ryanhubert.com/>). An online appendix with supplementary material is available at [xxx] and at the website of the corresponding author.*

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Every year, federal district courts hear at least 275,000 civil lawsuits and at least 75,000 criminal cases. This is around seven times more than the federal circuit courts and 2,300 times more than the U.S. Supreme Court. District judges are the *only* judges to preside over 90% of the litigation filed in federal courts.<sup>1</sup> Given judges' discretion over managing cases, the expense of district court litigation, and limits on appellate courts' ability to review cases, there is ample room for district court judges to exert influence over case outcomes. Thus, even though they do not have de jure power to set or modify doctrine, district judges may have de facto power to shape policy outcomes in the judiciary through their adjudication of individual cases.

It is therefore not surprising that appointments to district courts have become political battlegrounds (Scherer 2005). If judges appointed by Republican presidents resolve cases in systematically different ways than judges appointed by Democratic presidents, then in the aggregate, the battle for appointments has the potential to shape legal and policy outcomes for generations. For example, President Clinton appointed 305 district court judges during his eight years as president, filling approximately 45% of the country's active district judgeships. Twenty years after his presidency ended, 55 Clinton appointees are still active judges and another 142 still serve as semi-retired senior judges (who hear a reduced number of cases).<sup>2</sup>

In this paper, we seek to better understand the stakes of political appointments of district judges. To do this, we study whether civil rights cases in district courts are resolved differently depending on whether they are assigned to Republican appointees or to Democratic appointees. To our knowledge, we provide the first causally identified estimates of how lawsuits filed in federal district courts are affected by assignment to Republican versus Democratic appointees. Our analysis makes use of an original dataset of all civil rights cases filed over two decades in seven district courts within the Ninth Circuit (around 70,000 cases assigned to nearly 200 judges). We find systematic differences.

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<sup>1</sup>For more detailed information about caseload statistics, see <https://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables>.

<sup>2</sup>For information about federal judgeships, see <https://www.uscourts.gov/judges-judgeships>.

The effects are driven largely by differences in whether a case settles or is dismissed and not by differences in the formal judgments issued by district judges. Assignment to a Republican appointee decreases the probability that a plaintiff will obtain a settlement by 4.9 percentage points and increases the probability that a plaintiff’s case is dismissed by a similar magnitude. These effects are intuitive since settlements favor civil rights plaintiffs (a “liberal” outcome) and dismissals favor civil rights defendants (a “conservative” outcome).

We also find that the causal effect of assignment to a Republican (or Democratic) appointee has increased with time. For cases filed during the first presidency covered by our dataset—the Clinton presidency—we estimate the assignment to a Republican appointee resulted in only a 1.0 percentage point increase in pro-defendant outcomes. In contrast, for cases filed during the Obama presidency, we estimate that this effect increased to 7.4 percentage points. We find suggestive evidence that this increase is due to factors affecting the broader judiciary and not due to newer appointees being more polarized.

We also explore how the political stakes of district court appointments compare to the stakes of circuit court appointments. We estimate that, in the Ninth Circuit, a panel comprised of a majority of Republican appointees is 5.4 percentage points more likely to reach a pro-defendant outcome in a civil rights case than a panel comprised of a majority of Democratic appointees. This is approximately the same difference we estimate between Republican appointees and Democratic appointees in our sample of district courts. This finding challenges the robustness of the claim that there is less of a difference between Democratic appointees and Republican appointees in the district courts than in the circuit courts (e.g., Zorn and Bowie 2010; Epstein, Landes and Posner 2013).

Our analysis suggests that the political stakes of district court appointments are substantial. But our study has important limitations. Most notably, we do not know how our effects would generalize to other district courts, circuits or historical time periods outside our sample, or to other areas of law. Second, we are unable to fully quantify the substantive impact of our results for civil rights litigants. In particular, even though we know that Democratic appointees induce more

settlements and Republican appointees induce more dismissals, the precise details of the settlements (including dollar amounts) are private and not visible to us. Incorporating the contents of settlement agreements would be a promising—although difficult—area for future research. Finally, since we study historical data (like all prior research), our findings do not necessarily provide direct insight into current or future appointments. In fact, since we find evidence that effects are increasing over time, our estimates may understate the current or future stakes of presidential appointments.

In spite of these limitations, our analysis covers a dramatically wider scope than any existing research on this issue. We study one of the most important areas of federal law across a time period of two decades and a set of courts whose jurisdiction includes over 50 million Americans.

## **Politics and the United States District Courts**

The bulk of judicial politics research focuses on the U.S. Supreme Court and U.S. Courts of Appeals, despite the fact that the district courts resolve most cases heard by the federal judiciary. As Epstein, Landes and Posner (2013) points out, this is because many political scientists presume: “[lower] courts play a smaller role in the political life of the nation; data relating to them are more difficult to collect and process; and ideology is less likely to influence the decisions of courts that are more constrained than the Supreme Court is—and ideology is what political scientists who study the courts tend to find interesting” (p. 79).

These three presumptions require reassessment. First, since the mid-1990s, federal courts have put most or all of their court records in electronic databases that are increasingly easy to use for research. While it still requires some effort to compile this data, we provide a data collection roadmap for scholars, which we detail in the next section and online appendix A. Second, around 90% of federal civil cases are resolved by the district courts without appeal. If district judges provide the only avenue for legal relief in 90% of federal cases, it is at least debatable whether they “play a smaller role in the political life of the nation” (see also Hübert 2019).

The third presumption requires the biggest reassessment. Why *should* political scientists be so focused on empirically studying the effect of judges’ political ideologies on their decisions? As we discuss below, it is not feasible to identify causal effects of judges’ political ideologies, nor is it wise to focus specifically on district judges’ formal decisions or written opinions given the outsized importance of litigant decisions in district court cases. That said, it is still both substantively important and empirically feasible to study how politics affects outcomes in district courts.

Politics can affect outcomes in district courts through judicial appointments (Ferejohn 2002; Epstein and Segal 2005). Federal judges are appointed by presidents (subject to confirmation by the Senate), and presidents’ judicial appointments are inherently political. Presidential elections are framed at least in part around the issue of judicial appointments. Prior research has documented how outside interests, such as the Federalist Society, have tried to achieve their policy goals through appointments, which has created a judicial bench ideologically unrepresentative of the population of lawyers (Teles 2013; Bonica and Sen 2017). There is also ample scholarly research on whether and how judicial appointment methods affect real-world outcomes (e.g., Gordon and Huber 2007; Caldarone, Canes-Wrone and Clark 2009; Choi, Gulati and Posner 2010). In this paper, we shift the study of judicial decision-making away from a focus on judicial ideology and judges’ decisions, toward the stakes of judicial appointments and all case outcomes.

## **The “Effect of Political Ideology”**

Our paper fits somewhat uncomfortably with existing research on politics and decision-making in American courts. A number of studies examine whether there are differences between case outcomes based on whether Democratic appointed judges or Republican appointed judges are assigned to cases, interpreting the party of the appointing president as a “proxy” for political ideology (for an overview, see ch. 2 of Epstein 2013). Unlike most prior studies, we do not seek to empirically pinpoint the effect of judges’ political ideologies on their decision-making.

The reason for our departure from previous research is that it is infeasible to causally estimate

the effect of political ideology on judges' decisions (Ho and Quinn 2010). Doing so would require that the judges being compared (e.g., Democratic and Republican appointees) are identical in all relevant respects except their political ideologies. But judges are more appropriately viewed as compound treatments, making it difficult to untangle the specific effect of political ideology.

To illustrate the challenge, consider a concrete example. Suppose one compares outcomes in cases heard by Democratic appointees with outcomes in cases heard by Republican appointees. Even if being a Democratic or Republican appointee is correlated with judges' political ideologies, it is also correlated with other characteristics of those judges. For example, recent Democratic appointees are more likely to be women or non-white than recent Republican appointees. So, the effect of being a Republican or Democratic appointee on case outcomes is not purely due to ideology if case outcomes also differ depending on the assigned judge's gender or race.

One approach to dealing with this problem is to control for these potential confounders in an effort to "isolate" the effect of ideology (see, e.g., Ashenfelter, Eisenberg and Schwab 1995; Epstein, Landes and Posner 2013). For example, a researcher may run a regression that includes controls for whether the judge is a woman or non-white. However, there are many judge characteristics that may not be observed by the researcher and cannot be controlled for. For example, do Democratic and Republican appointees have the same legal education? Do they have the same views on textualism or originalism? The same tolerance for missed deadlines? Do they hire law clerks from the same law schools? In order to interpret the effect of being a Republican or Democratic appointee as "political ideology," one must make a selection-on-observables assumption that there are no other judge characteristics that are correlated with both case outcomes and whether they are a Republican or Democratic appointee.<sup>3</sup>

In most situations, it is not reasonable to make an assumption like this. First, a researcher must assume that all of the unobserved characteristics that she has thought of are uncorrelated with

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<sup>3</sup>A similar issue arises in studies attempting to isolate the effect of other judge characteristics (like race or gender) by controlling for partisanship (e.g., Boyd, Epstein and Martin 2010; Boyd 2013; Kestellec 2013).

either the outcome or whether the judge is a Democratic or Republican appointee. Second, and more troubling, she must assume that all of the unobserved characteristics that *she has not thought of* are also uncorrelated with either the outcome or whether the judge is a Democratic or Republican appointee. So, once a researcher controls for other judge characteristics to try to “isolate” the effect of political ideology, she is left with an estimate that is difficult to interpret, and is almost certainly not an unbiased estimate of the effect of political ideology.<sup>4</sup>

In contrast, we do not use the party of a judge’s appointing president as a proxy for their political ideology, nor do we attempt to “isolate” the effect of judges’ political ideologies. Because we are interested in the extent to which presidents’ appointments to the district courts impact outcomes in the judiciary, we do not need to know whether our effects are driven by judges’ political ideologies. We instead estimate the *direct* effect of assigning a case to a Republican appointee rather than a Democratic appointee. We do so regardless of whether this direct effect is driven by appointees’ differing political ideologies or other factors that differ between the judges that Democratic and Republican presidents appoint (such as appointees’ races, genders, ages, commitments to certain judicial principles, etc.). To further emphasize that we are not measuring the “effect of ideology,” we refer to our causal estimates as Assignment to a Republican Appointee effects (“ARA effects”).

We are not entirely unconcerned with *why* case outcomes differ depending on whether they are assigned to Republican or Democratic appointees. As we discuss in more detail below, our interpretation of ARA effects requires us to demonstrate that differences between Democratic and Republican appointees are not due solely to the timing of their appointments. Moreover, we also explore the broader institutional and historical context of our effects. That said, our conceptual objective is to direct attention away from what is inside a judge’s heads (e.g., the “effect of ideology”) and toward structural factors like changes in the set of appointed judges.

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<sup>4</sup>A judge’s “political ideology” is also a somewhat vague concept, as evidenced by the significant debate about how to measure and interpret it (see Ho and Quinn 2010).

## **Ignoring Litigants**

Our second point of departure from most previous literature on politics and decision-making in the district courts is our focus on litigants. Scholars have long recognized that the strategic behavior of litigants plays an important role in shaping outcomes in district courts. At least since the canonical work of Priest and Klein (1984), research has demonstrated that litigants are important decision-makers and behave strategically during litigation (e.g., Waldfogel 1995).

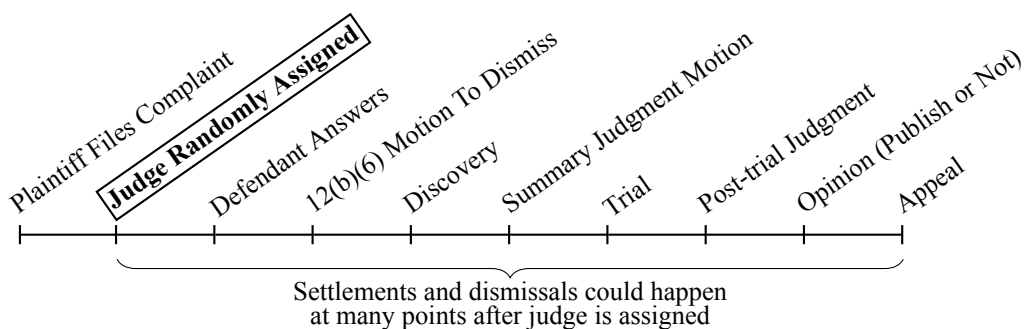
This research has largely been divorced from judicial politics research and typically does not examine whether strategic litigant behavior is specifically responsive to whether judges were appointed by Democratic or Republican presidents (except Ashenfelter, Eisenberg and Schwab 1995, discussed below). Given that judges are political actors appointed by other political actors, this is an important question in its own right. If litigants are responsive to whether judges are appointed by Democrats or Republicans (as we show), then this suggests that richer models of strategic litigation would explicitly consider how litigants' expectations about winning are shaped by the partisan breakdown of the judges appointed to the bench.

Unfortunately, many prior studies of district courts purposefully push litigant behavior to the background by focusing their analyses on subsets of cases that are formally decided by a judge. But judges' influence over cases might operate through the litigants' decisions. It would be a mistake to conclude that a well-estimated causal effect of being assigned a particular kind of judge has nothing to do with the judge simply because the effect is most visible in the litigants' subsequent decisions. In fact, a well-identified effect like this is inherently interesting since it sheds light on the mechanisms by which judges influence outcomes on cases. For example, such effects suggest that district judges may influence case outcomes in ways that are not reviewable by appellate courts.

Pushing litigants' decisions into the background is not only substantively problematic, it also creates methodological challenges. To see why, consider Figure 1, which presents a stylized depiction of the litigation process in federal courts. One of the very first things that occurs after a plaintiff files a lawsuit is random assignment of a judge to the case. This provides a unique, and



**Figure 1:** *Litigation in federal district courts is complex and has multiple stages. A judge can affect outcomes at any decision point after her random assignment to the case.*



rare, opportunity to estimate causally identified effects in this institutional setting. But in order to exploit this source of randomization and estimate causal effects, a researcher must perform analysis on a dataset that contains *all* cases that were randomly assigned to a judge.

This is not common in studies of district courts. Possibly the most well-known empirical analysis of district court cases, Carp and Rowland (1996) analyzes a subset of district court cases that end with published opinions. Perino (2006) and Keele et al. (2009) study a subset of cases that have written opinions, both published and unpublished. Randazzo (2008) and Zorn and Bowie (2010) study a subset of cases that were eventually appealed. Epstein, Landes and Posner (2013) studies both a subset of cases with motions to dismiss, and a subset of cases that were appealed. In each of these studies, the analysis was performed on datasets that dropped most settled cases, many or most dismissed cases, and a sizeable number of judgments (e.g., those which did not have an opinion or were not appealed). But actions taken by both judges and litigants earlier in litigation mean that these samples of cases do not reflect the initial randomized case assignment.<sup>5</sup>

This has seriously negative implications for causal inference. The problem is that these studies are conditioning on a post-treatment variable. There is a large (and growing) literature warning

<sup>5</sup>Research on criminal sentencing has yielded more uniform empirical effects, typically finding some differences between judges appointed by Democrats and Republicans (e.g., Schanzenbach and Tiller 2007; Fischman and Schanzenbach 2011; Cohen and Yang 2018). Somewhat tellingly, criminal sentencing cases are not subject to the problem we describe since they cannot naturally be subsetted based on outcomes.

researchers about the potential of post-treatment bias (see, e.g., Rosenbaum 1984; Acharya, Blackwell and Sen 2016; Knox, Lowe and Mummolo 2020). Once a researcher has introduced selection bias by conditioning on post-treatment variables in this way, the resulting estimates are largely uninterpretable and potentially misleading. The only way to guard against post-treatment bias in this context is to conduct analyses on the universe of cases that were assigned to a judge (or a random sample of this universe). Below, we provide evidence that we can recover as-if random assignment of cases to judges in our dataset. Then, we also demonstrate that dropping specific kinds of cases from our dataset (like settled cases) biases effects toward zero.

We are not the first to identify this problem (e.g., Hoffman, Izenman and Lidicker 2007; Kim et al. 2009). However, we can only identify one study on the effect of politics in district courts that does not make the faulty research design choice of subsetting to cases that ended in a particular way. Ashenfelter, Eisenberg and Schwab (1995, henceforth “AES”) studies a dataset of all civil rights cases from three district courts during one year (fiscal year 1981). AES finds no statistically significant differences in outcomes between cases heard by Republican appointees and cases heard by Democratic appointees. We identify at least two issues with this analysis.

First, the sample size in AES is small. The paper analyzes 2,258 cases heard by only 47 judges. The null effects in AES could be driven by low statistical power. In contrast, we analyze a much larger dataset of around 70,000 cases heard by nearly 200 judges over the course of two decades. Second, the paper makes the move that we objected to in the previous subsection, attempting to measure the “effect of ideology” with regressions that control for a host of other variables. Because the paper does not provide a simple estimate of the effect of assignment to a Republican appointee rather than a Democratic appointee, we are left with effects that are difficult to interpret causally.<sup>6</sup>

Thus, to our knowledge, we provide the first causally identified estimate of the effect of assigning district court lawsuits to Republican appointees rather than Democratic appointees. In the next

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<sup>6</sup>The paper’s estimates are actually a bit more confusing. The regression includes two variables, `REPUBLICAN PARTY` and `REPUBLICAN PRESIDENT`. The text of the paper does not identify the source for the `REPUBLICAN PARTY` variable and provides no information about what distinguishes it from the `REPUBLICAN PRESIDENT` variable.

sections, we describe our original dataset and present the results of our analysis.

## Data

Our analysis is based on an original dataset of civil rights cases filed between 1995 and 2016 in the seven U.S. District Courts in Washington, Oregon and California. Our analysis in the next sections will exploit the random assignment of cases to judges to estimate the causal effect of assignment to Republican or Democratic appointees on case outcomes. In this section, we provide a brief overview of our data collection and cleaning process. We provide more detailed information in online appendix A.

To construct the dataset, we collected every docket sheet stored in these seven courts' online record systems and used automated text processing methods to extract key pieces of information. We merge the data collected from these docket sheets with the Federal Judicial Center's Integrated Database (IDB), available at <https://www.fjc.gov/research/idb>. Because we rely both on docket sheets collected directly from the courts and the IDB, our dataset does not have the non-random missing data problems that are known to introduce bias in datasets derived from other data sources, such as Westlaw and LexisNexis (see Burbank 2004; Hoffman, Izenman and Lidicker 2007; Kim et al. 2009).<sup>7</sup> However, this also means that we are limited to courts where we received permission to access PACER. Even so, the courts in our dataset hear around 13% of civil cases filed in federal district courts and cover around 15% of the U.S. population (based on the 2010 Census).

Most case-level data is available both in the IDB and the docket sheets. However, there is some data that is available in the IDB but not in the docket sheets, or vice versa. Three variables are most important. First, the FJC does not report judge-identifying information in its publicly released version of the IDB. One of our major contributions is to include this information. Second,

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<sup>7</sup>We also note that our dataset does not suffer from problems stemming from the staggered adoption of electronic filing across the courts since we use the IDB to ensure we have the universe of cases and supplement with data from the electronic docket sheets.

the IDB does not contain detailed information about the litigants, including the number of litigants and attorneys. We extract this information from the docket sheets. Third, the docket sheets do not consistently or clearly report case outcomes. We rely on the IDB for this information.

We use PACER’s Nature of Suit codes (reported in the IDB) to identify all civil rights cases. We focus on civil rights cases for two reasons. First, after prisoner petitions, civil rights cases make up the largest share of the cases in our dataset. This reflects the fact that civil rights is an important and distinctive area of federal law. Second, limiting our analysis to civil rights cases allows for clearer interpretations about the substantive meaning of the various case outcomes. Civil rights cases typically entail plaintiffs who allege a civil rights violation and defendants who are accused of those violations. Therefore, case outcomes favoring plaintiffs can be interpreted as more “liberal,” whereas those favoring defendants can be interpreted as more “conservative.”

We took several more steps to clean the dataset before our main analysis. First, we drop cases that have no presiding district judge. Most cases without a presiding district judge are assigned directly to magistrate judges in accordance with court rules.<sup>8</sup> Second, we drop cases from court divisions and in years where there is no variation in the treatment (i.e., all cases were either assigned to Democratic appointees or to Republican appointees).<sup>9</sup>

The final step we take to clean our dataset is most consequential. To determine the judge who sits on each case, we use information from the docket sheets. Each docket sheet contains a field indicating which judge a case is assigned to. Unfortunately, this field is updated every time a case is reassigned to a new judge. This can pose a problem for our causal identification strategy, since reassignments may be judge-driven or otherwise non-random. We use automated methods

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<sup>8</sup>We cannot classify magistrate judges as Democratic or Republican appointees since they are appointed by their assigned court and not by the president.

<sup>9</sup>For this second step, we drop 2,321 observations (3% of the data) from 40 division-year blocks (12% of the blocks). These dropped blocks contain a disproportionately small number of observations, averaging 58 cases per block (versus the sample average of 221). As a robustness check, we also conduct an analysis with a more stringent requirement that there be at least two Democratic and two Republican appointees hearing cases in each court division in each year. We discuss this in online appendix A.3 and report results (which are slightly larger) in column (5) of Table C1 in online appendix C.

to identify every case where the judge listed as the assigned judge does not appear to be the same judge who was initially assigned to the case. To do this, we scan each case's docket entries to see if (1) there is any mention that the case was assigned or reassigned and (2) a different judge name (other than the listed judge or the chief judge of the court, who performs reassignments) is mentioned before or on the date that the case is reassigned. With this process, we determined that approximately 14% of civil rights cases featured a judge reassignment at some point before the case was terminated. For our main analyses, we drop these cases.<sup>10</sup>

We have no *ex ante* reason to think that any of our data cleaning decisions affect our causal identification assumption that cases are randomly assigned to judges. In particular, we mostly drop cases based on pre-treatment variables, which are determined prior to the assignment of judges and accordingly should not affect the random assignment of cases to judges. Our decision to drop reassigned cases only poses a challenge if reassignments are judge-driven in a way that is correlated with case characteristics. Since reassignments almost always occur due to a judge's retirement or appointment, this is not a substantive concern. In the next section, we provide empirical evidence that Democratic appointees and Republican appointees in our dataset hear the same kinds of cases.

After cleaning the data, we are left with a dataset of 70,680 civil rights cases across seven courts and twenty-two years. This dataset includes cases heard by 193 judges. Of these cases, half were assigned to Republican appointees and half were assigned to Democratic appointees.<sup>11</sup>

We code case outcomes using the IDB's DISP variable (how the case ended) and JUDGMENT variable (which party won). The IDB provides very detailed data, so we consolidate how cases ended into seven outcomes that are the basis of our analyses: settlements, dismissals (voluntary and involuntary<sup>12</sup>), judgments (favoring defendants, plaintiffs or other) and all other outcomes

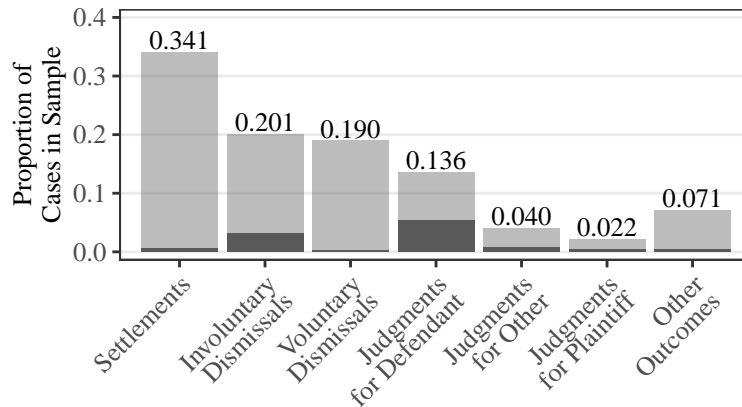
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<sup>10</sup>In column (3) of Table C1 in online appendix C, we provide an additional set of results that uses our best guess about which judge was initially assigned in these reassigned cases. The results are substantively similar.

<sup>11</sup>Since the number of Republican and Democratic appointees on these courts fluctuates over time, it is possible that the share of cases heard by Democratic or Republican appointees *in any given year* is not equal. However, in our pooled dataset across all 20 years, it is equal.

<sup>12</sup>The IDB classifies these as dismissals for other reasons. However, we opt to label them involuntary dismissals

**Figure 2:** We plot the share of our sample that ended with each outcome (light bars), as well as the share of those cases that ended up being appealed (dark bars).



(mostly transfers and remands). Settlements are largely self explanatory: the plaintiff(s) agrees with the defendant(s) to end the case in exchange for some benefit to the plaintiff (e.g., money or an injunction). Voluntary dismissals involve the plaintiff withdrawing their case, either with or without consent of the defendant, and generally mean that the plaintiff can refile at a later date (and perhaps in a different court). Involuntary dismissals and judgments both end a plaintiff’s case through judicial action. Figure 2 presents the means of these outcomes with the light gray bars.

## How Political Appointments Affect Case Outcomes

We now move to our main empirical analysis, in which we estimate the average causal effect of assigning cases to Republican appointees as opposed to Democratic appointees. As we discuss above, we refer to these as Assignment to Republican Appointee (ARA) effects.

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to distinguish them from voluntary dismissals.

## Causal Identification

A nice feature of studying adjudication in U.S. federal courts is that cases are supposed to be randomly assigned to judges. This provides an opportunity to estimate the causal effect of assigning different judges to cases. Indeed, the random assignment in principle allows us to know that the kinds of cases heard by Democratic appointees in our dataset are the same (on average) as the kinds of cases heard by Republican appointees. If they are not, then any differences we observe between Democratic and Republican appointees could be due to underlying differences in the cases the two groups of judges get assigned, and not necessarily due to the judges themselves.

In order to exploit the random assignment of judges to cases, we must be careful to account for the details of the randomization procedure. In district courts, random assignment occurs within each district court's divisions after a case is filed.<sup>13</sup> For example, General Order No. 16-05 in the Central District of California says that “when a case with a civil case number [...] is assigned to a district judge, it will be randomly assigned from a *division-specific* General Civil Assignment Deck” (emphasis added).<sup>14</sup> This means that we can only presume that judges are randomly assigned to cases *within court divisions* and *within specific intervals of time*. We accordingly make a causal identification assumption that cases are randomly assigned to Democratic or Republican appointees within each court division and year, effectively treating a division-year as a “block” in a blocked randomized experiment (Gerber and Green 2012, section 3.6.1).<sup>15</sup>

To provide evidence in favor of our causal identification assumption, we conduct a pre-treatment

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<sup>13</sup>Each district court can have multiple divisions. For example, the Central District of California is divided into three divisions (Western, Eastern and Southern) spread across four courthouses (two in Los Angeles, one in Riverside and one in Santa Ana).

<sup>14</sup>Available at <https://www.cacd.uscourts.gov/sites/default/files/general-orders/16-05.pdf>.

<sup>15</sup>Since senior judges have more control over their own caseloads, one might wonder whether the assignment of some cases to senior judges violates the overall random assignment of cases to judges. As we report in this section, we find no evidence of violations of random assignment even though 11% of our cases were heard by senior judges. However, that does not necessarily mean that causal effects will be the same for senior judges as active judges. In column (2) of Table C1 in online appendix C, we provide an additional set of results that drops senior judges. The results are substantively similar.

covariate balance test that is more aggressive than standard approaches.<sup>16</sup> The idea is straightforward: using a stacked ensemble machine learning approach described in online appendix B, we search across a very large set of model specifications in order to find the one that best predicts treatment using pre-treatment variables. In other words, we aggressively try to find a violation of random assignment.

Our balance test requires two steps. First, we generate cross-validated predicted probabilities that each case will be assigned to a Republican appointee conditioning *only* on division-year blocks, thus accounting for our block randomization. This provides us with a benchmark since we already know that judges are not randomly assigned to cases *across* the blocks. Second, we do the same thing but use division-year blocks as well as all other pre-treatment variables. If this second model provides substantially more predictive power for treatment assignment than the benchmark model, then we have evidence of a violation of our causal identification assumption. Estimation details are available in online appendix B.

**Figure 3:** *We provide evidence that assignment of judges to cases is as-if random within division-year blocks. The left panel plots receiver operating characteristic (ROC) curves and the right panel plots an empirical quantile-quantile (eQQ) plot. In each panel, we compare our ability to predict treatment using only division-year blocks (benchmark model) with our ability to predict treatment using all pre-treatment variables (saturated model).*

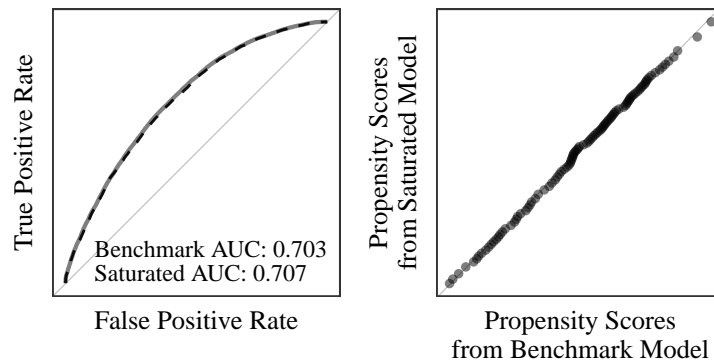


Figure 3 presents our results. In the left panel, the dashed receiver operating characteristic

<sup>16</sup>Standard balance tests have several limitations, which we discuss in online appendix B.



(ROC) curve—as well as its associated area under the curve (AUC) metric—provide an indication of the predictiveness of our benchmark model. In machine learning contexts, the goal is an ROC curve that is highly protruded from the 45 degree line, since this indicates a model is highly predictive. In this specific context, since we are testing for covariate balance, the goal is the opposite: i.e., that pre-treatment variables are *not* predictive of whether a case is assigned to a Republican or Democratic appointee. As we expect, the division-year blocks are predictive of treatment assignment since we know that randomization only occurs *within* court divisions and over time. The solid, lighter ROC curve (and associated AUC) demonstrates the predictiveness of the model that includes all pre-treatment variables including the division-year blocks. The model that includes all the pre-treatment variables is only trivially more predictive of whether cases are assigned to Republican appointees or Democratic appointees than the benchmark model. This provides evidence that, conditional on court division and year, it is reasonable to assume that judges are as-if randomly assigned to cases.

In the right panel, we present this information in a different way, using an empirical quantile-quantile (eQQ) plot. This plot compares the distribution of propensity scores for the benchmark model against the distribution of propensity scores for the saturated model that uses all pre-treatment variables.<sup>17</sup> If there are no substantial imbalances after accounting for the blocked randomization then the points should be aligned along the 45 degree line. This would indicate that the distribution of propensity scores does not shift substantially between the benchmark model and the model with all pre-treatment variables. The eQQ plot demonstrates virtually no difference between these two distributions, again evidence in favor of our causal identification assumption.

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<sup>17</sup>To generate this plot (and make it more intelligible), we bin each percentile of each distribution by taking the mean of the propensity scores in that percentile and plotting those means.

## Estimating ARA Effects

We estimate ARA effects for each outcome in our dataset.<sup>18</sup> We use a standard procedure for estimating treatment effects in block randomized designs with varying treatment assignment probabilities across blocks (see Lin 2013). Formally, for each outcome  $Y$ , we estimate the following OLS regression model:

$$Y_{idy} = \alpha + \beta_{\text{ARA}} \cdot R_{idy} + \sum_{dy} \{ \phi_{dy} \cdot X_{idy} + \gamma_{dy} \cdot R_{idy} \cdot (X_{idy} - \bar{X}_{dy}) \} + \varepsilon_{idy} \quad (1)$$

where  $i$  indexes cases,  $d$  indexes court division and  $y$  indexes case filing year. (A “block” in our study is defined by a court division and year.)

The treatment variable is  $R_{idy}$  and takes a value of 1 if case  $i$  is assigned to a Republican appointee and 0 if it is assigned to a Democratic appointee.  $X_{idy}$  is a dummy variable indicating whether case  $i$  is in division  $d$  and year  $y$  (i.e., “block”  $dy$ ), and  $\bar{X}_{dy}$  is the proportion of cases in our sample heard within division  $d$  and year  $y$ . Estimates of  $\beta_{\text{ARA}}$  give the unbiased, average causal effect of a case being assigned to a Republican appointee—the ARA effect.<sup>19</sup> Note that our estimates treat assignment to a Democratic appointee as the reference category, but it is straightforward to see that  $-\beta_{\text{ARA}}$  gives the average causal effect of assignment to a *Democratic* appointee relative to a Republican appointee. We cluster standard errors at the judge-level.<sup>20</sup>

We report the ARA effect for each outcome in Figure 4. In online appendix C, we provide additional information about our estimation procedures, and we also report numerical results and

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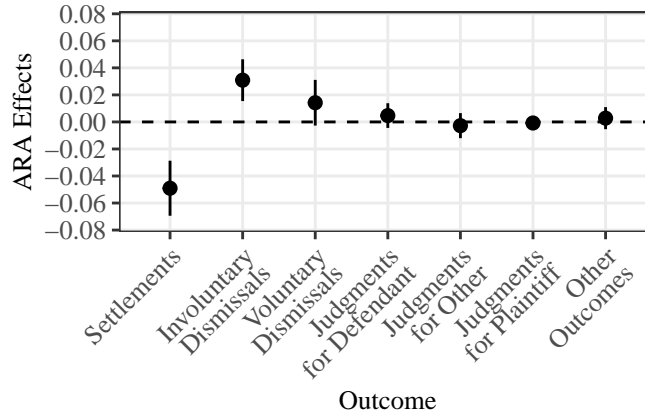
<sup>18</sup>Given the size of our dataset and the relatively few number of case outcomes, multiple comparison adjusted standard errors are not substantially different than those reported here.

<sup>19</sup>Note that the estimate for  $\beta_{\text{ARA}}$  is identical to a non-parametric weighted average of the block-specific ARA effects with weights determined by block sizes, as well as a weighted least squares regression with unit-level weights  $\frac{n_{dy} R_{idy}}{n_{dy}^1} + \frac{n_{dy}(1-R_{idy})}{n_{dy}^0}$ , where  $n_{dy}$ ,  $n_{dy}^1$  and  $n_{dy}^0$  respectively indicate the total, treated and control cases in block  $dy$  (see sections 3.6.1 and 4.5 of Gerber and Green 2012).

<sup>20</sup>To estimate our effects and clustered standard errors, we use the `estimatr` library for the R statistical programming language (Blair et al. 2015).

additional robustness checks.

**Figure 4:** For each outcome, we plot the ARA effect with a solid black dot and its corresponding 95% confidence interval based on judge-clustered standard errors.



The outcome with the largest ARA effect is settlement. On average, being assigned to a Republican appointee (instead of a Democratic appointee) causes settlements to occur 4.9 percentage points less often. Republican appointees increase the rate of voluntary or involuntary dismissals of plaintiff cases by 1.4 and 3.1 percentage points, respectively, although the former is not significant at the 5% level. We do not identify statistically significant effects for any other outcomes, including judgments.

There are a number of plausible explanations for our results that are consistent with the idea that Republican appointees disfavor civil rights claims more than Democratic appointees. It may be the case that defendants, once assigned to a Republican (Democratic) appointee, positively (negatively) update their assessment of prevailing in litigation, making them less (more) likely to provide benefits to the plaintiff through settlement. Similarly, once assigned to a Republican (Democratic) appointee, plaintiffs may negatively (positively) update their assessments and be more (less) likely to withdraw cases. Alternatively, Republican (Democratic) appointees might be less (more) likely to hold settlement conferences or exert pressure on litigants during those conferences. It is also even possible that litigants have incorrect perceptions about judges such that our effects are completely driven by litigants' stereotyping Democratic appointees as "liberal" and Republican appointees as

“conservative” rather than any true underlying difference between the judges.

Finally, our null effects for judgments are consistent with the idea that district judges are reversal averse (Choi 2012) and therefore seek to influence outcomes in ways that do not provoke a reversal by an appellate court. One way to do this is to avoid issuing controversial final judgments (subject to review by an appellate court) and instead focus on changing the litigants’ decisions (typically not subject to review). We discuss this in more detail below.

To be clear, while our data allows us to estimate unbiased causal ARA effects, it does not allow us to know for sure why case outcomes differ between Democratic or Republican appointees. But whatever the case, our analysis demonstrates that case outcomes in a non-trivial number of cases are changed solely because they were assigned to a Republican or Democratic appointee. Counterintuitively, the largest effects are not visible in the judges’ formal decisions or opinions, but rather in litigants’ behaviors in response to their assigned judge. So, it is reasonable to conclude that litigant behavior is an important part of the story. Unfortunately, traditional research designs that exclude settlements and voluntary dismissals miss this crucial part of the story.

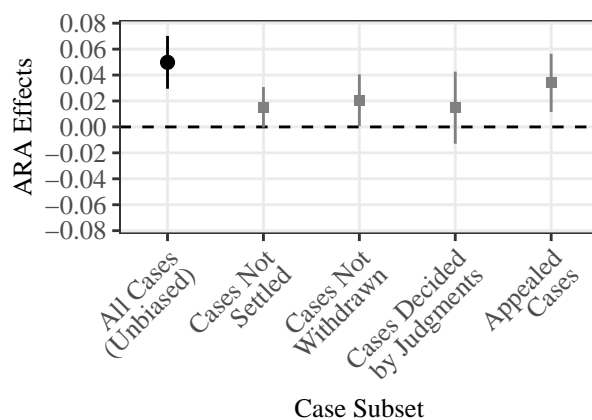
To make our remaining analyses more manageable, we consolidate the seven possible case outcomes into a single, dichotomous outcome: whether the case was resolved in a pro-defendant way or not. We code all dismissals and judgments for the defendant as pro-defendant and all other outcomes as *not* pro-defendant. Note this latter category contains cases with obviously pro-plaintiff outcomes (e.g., judgments for the plaintiff) and ones that are ambiguous (e.g., transfers to another district court). We code settlements as not pro-defendant because they do not constitute unambiguous victories for the defendant. Indeed, the common intuition about settlements is that they are plaintiff victories. In a rare study of employment discrimination settlement agreements (which are typically unobservable to researchers), plaintiffs were awarded an average of \$54,651 (Kotkin 2007). But we again note that we do not observe the contents of settlements in our own dataset.

## Why You Can't Subset Cases Based on Outcomes

Given that our largest ARA effects correspond to settlements, this raises serious questions about analyses of datasets that do not include settled cases. More broadly, we should worry that *any* dataset that subsets to specific kinds of outcomes (judgments, dismissals, appeals) will introduce selection bias into results by selecting on outcomes. We now demonstrate that this concern is warranted by dropping cases that end in certain ways and showing the extent of the resulting bias.

For this exercise, we subset to several sets of cases and re-estimate (biased) ARA effects on each of these subsets. These subsets are: (1) cases that were not settled, (2) cases that were not withdrawn either by settlement or by voluntary dismissal, (3) cases that were terminated with judgments issued by a judge, and (4) cases that were ultimately appealed to the Ninth Circuit. For comparison, we include our unbiased estimate from our full sample.

**Figure 5:** *We plot ARA effects (on pro-defendant outcomes) for our full sample and for several subsets of cases, demonstrating that dropping cases based on outcomes biases effects toward zero.*



In Figure 5, we plot the results of this exercise. On the very left, we plot the unbiased, causal ARA effect on pro-defendant outcomes using our full dataset. As we found in the previous section, there is a substantively meaningful ARA effect of 5 percentage points. However, after dropping cases ending in specific ways, this effect is reduced to anywhere between 1.5 and 3.4 percentage points. In other words, dropping cases based on outcomes introduces post-treatment bias. This

provides one possible explanation for why past research has yielded mixed results about the effect of Republican versus Democratic appointees being assigned to cases, much of it demonstrating no effects. For example, Zorn and Bowie (2010) finds no effect of partisanship on a subset of district court cases that were appealed to the Supreme Court. Examining Figure 5, there is reason to worry that these effects could be biased downward due to the post-treatment bias introduced when subsetting to cases that were appealed.

## **Interpreting ARA Effects**

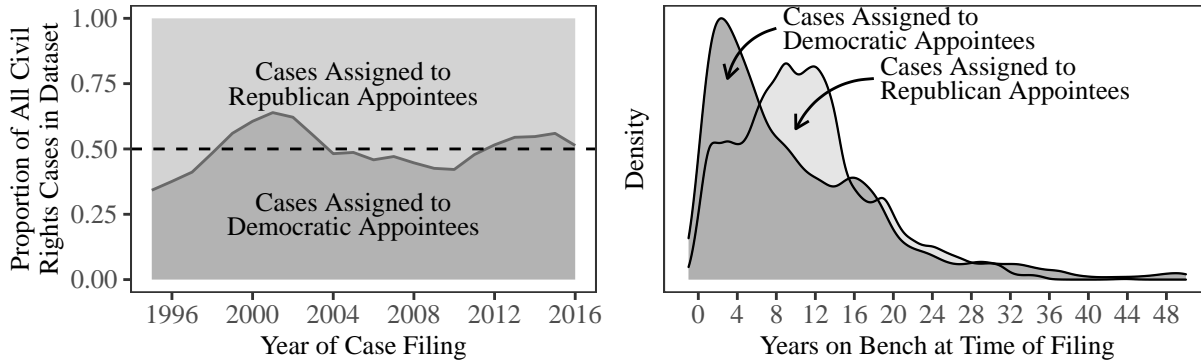
Since we find that assignment to Republican appointees (instead of Democratic appointees) causes case outcomes to be more favorable to defendants, this is suggestive evidence that it matters whether a Democratic president or a Republican president makes appointments to the district courts. If it does matter, then presidents' appointments to the federal district courts change the probability that any given case gets assigned to a Republican appointee and thus alters the average outcome of civil rights cases.<sup>21</sup> The left panel of Figure 6 demonstrates how presidents' appointments over two decades changed the probability that civil rights cases in our dataset were assigned to a Democratic or Republican appointee.

However, we also consider the possibility that our ARA effects do not accurately reflect the stakes of district judge appointments. This is possible because our ARA effects do not measure the effect of political appointments on case outcomes per se. To illustrate, consider an extreme hypothetical world in which all Democratic appointees in our sample had been appointed before 2005 while all Republican appointees in our sample had been appointed in 2005 and after. In this hypothetical example, it would be possible that our ARA effects are simply an artifact of time trends, rather than genuine differences between Democratic and Republican appointees.

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<sup>21</sup>The ARA effects do not appear to be driven by outlier appointees. Figure C1 in online appendix C shows the effect of assigning cases to each judge, relative to the judges of the other party she served with in the same division-year blocks. The effects are smoothly distributed.

**Figure 6:** *Left panel: The appointment of district judges over time changes the probability that a given case will be assigned to a Democratic or Republican appointee. Right panel: Since presidents serve four-year terms, this creates an imbalance in presiding judges' years on the bench between the set of cases heard by Democratic and Republican appointees.*



Practically speaking, we need to worry that there are time-varying confounding variables that make Democratic appointees incomparable to Republican appointees. Indeed, because our dataset spans two Democratic administrations (and follows 12 years of Republican presidents), the Republican appointees assigned to cases in our dataset have more experience on the bench than Democratic appointees. The right panel of Figure 6 demonstrates this.

In order to address concerns that our sample of Democratic and Republican appointees are not comparable, we conduct two robustness checks. First, we re-run our analyses controlling for judges' years on the bench. In a rough way, this should account for differences between Democratic and Republican appointees that are due to being appointed at different times. We report these results in column (6) of Table C1 in online appendix C. The results are nearly the same.

Second, we perform our analysis on subsets of cases heard by appointees of adjacent presidents (of opposing parties). For example, for one analysis, we subset to cases heard by Carter and Reagan appointees and estimate the ARA effect. Since adjacent presidencies occur closer in time, this minimizes the chance (as much as possible) that differences between Republican appointees and Democratic appointees could be due to different eras in which these appointments occurred. We present the results of this exercise in Figure C2 in online appendix C. A weighted average of these

adjacent president effects is 7 percentage points, which is close to and statistically indistinguishable from our main ARA effect on pro-defendant outcomes.

These two robustness checks provide convincing evidence that the main ARA effects are driven by genuine differences between Democratic and Republican appointees and not historical quirks relating to the timing of their appointments. In the next section, we put the ARA effects into broader context, comparing them to appellate courts and exploring how they have changed over time.

## **Putting ARA Effects in Broader Context**

We now conduct two additional analyses to put our findings in a broader context. For the first analysis, we demonstrate that our ARA effects are similar in magnitude to effects we estimate in an original dataset of civil rights appeals to the Ninth Circuit. This provides reason to revisit the conventional wisdom that politics matters less in district courts than in appellate courts (Zorn and Bowie 2010; Epstein, Landes and Posner 2013). The second analysis looks at the heterogeneity of district court ARA effects over time. We find that ARA effects have been increasing over time. Although our analysis of these changes over time is limited by the size of our dataset, the evidence suggests that increasing effects are due to factors affecting the broader judiciary. We do not find convincing evidence that the increase is due to increased polarization between newer appointees.

## **Comparison with the Appellate Court**

It is generally accepted that politics plays a smaller roll in district courts than in the “higher, more politically important courts—such as state high courts and the US courts of appeals” (Bonica and Sen 2017, p. 561). The reasons offered for this are numerous. For example, district court judges may be constrained by the supervision of the appellate courts, they might preside over more legally “easy” cases that leave less room for disagreement among judges, and the power of district court judges may be limited by their inability to establish precedent (Epstein, Landes and Posner 2013,



ch. 5).

But there are strong reasons to doubt the prevailing wisdom. First, the supervision over district courts by appellate courts is limited. There is a strong informational rationale for appellate courts' purposeful deference to district courts (Hübert 2019), and the vast majority of appealed decisions are affirmed. Most district court cases cannot even be appealed in the first place. For example, neither settlements nor voluntary dismissals are reviewable by an appellate court, except under extraordinary circumstances. Many involuntary dismissals, such as those dismissed "without prejudice," are also not appealable.<sup>22</sup> To see how this manifests in our own dataset, consider the appeals rates across our seven outcomes. Looking back to Figure 2, we plot the the share of cases that were appealed by outcome (the dark portion of the bars).

The limits on appellate review also raise questions about the idea that district courts "filter out" legally clear-cut cases that judges uniformly agree on, leaving appellate courts with a set of legally harder cases. Our analysis above demonstrated that the largest difference between Republican and Democratic appointees in our sample relates to whether cases settle. Settlements are rarely appealable. Thus, it is possible that district courts actually prevent appellate courts from reviewing many of the legally hard cases that engender disagreement between judges.

Finally, the importance of circuit courts' authority to establish precedent may be overstated. Circuit courts have starkly reduced the percentage of cases in which they create formal precedent from almost 100% to less than 10% (Copus 2020). As a result, the locus of their legal and political power has arguably shifted away from the creation of explicit law and toward informal precedent (i.e., litigant expectations) set by the aggregate adjudication of individual cases.<sup>23</sup> Adjudication is a task that district court judges do more of. And though there is no formal rule that district courts must follow one another, it is not uncommon for district judges to show deference to previous decisions

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<sup>22</sup>If a case is dismissed "without prejudice," then a plaintiff may refile the case in district court, which forecloses the possibility of direct appeal to a circuit court.

<sup>23</sup>Expectations informed by the aggregate adjudication of individual cases might be increasing with the rise of litigation analytics services such as Lex Machina.

of other district judges, especially those within their own district (Mead 2011).

We provide empirical evidence against the notion that district courts are less politically important than circuit courts. More specifically, we extend our analysis to civil rights cases heard by the Ninth Circuit and estimate that the effect of assigning a case to a majority-Republican panel rather than a majority-Democratic panel is approximately equivalent to the ARA effect we report in our analysis above. To conduct this analysis, we constructed an original dataset of all civil rights appeals heard by three-judge panels in the Ninth Circuit during the period 1996 to 2012. Of the 7,862 appeals, 64% were resolved with a pro-defendant outcome—i.e., affirming a district court decision that favors the defendant or reversing a district court decision that favors the plaintiff.

While researchers frequently invoke the assumption that cases are as-if randomly assigned to panels of judges at the appellate level, we are unable to find support for that assumption in our Ninth Circuit dataset. We note that scholars have recently called attention to the possibility that *judges* are not as-if randomly assigned to panels (Chilton and Levy 2015). As we discuss in more detail in online appendix D, we find evidence that *cases* are not even randomly assigned to panels in our dataset. We use the same machine learning technique we describe above and find that our ability to predict case assignment to panels with a majority of Republican appointees using pre-treatment variables is significantly higher than a baseline predictive model that uses only circuit division-year blocks. To correct for potential selection bias, we estimate an augmented inverse probability weighted (AIPW) average treatment effect (Glynn and Quinn 2010). We bootstrap standard errors at the panel level.

Our AIPW estimate reveals that panels with a majority of Republican appointees are 5.4 percentage points more likely to reach a pro-defendant outcome in a civil rights appeal than panels with a majority of Democratic appointees. The estimate is statistically significant.<sup>24</sup> This is ap-

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<sup>24</sup>The bootstrapped panel clustered standard error is 0.010. An inverse probability weighted (IPW) estimate is 0.060 with a standard error of 0.015. The unadjusted (biased) estimate is 0.078 with a standard error of 0.015. Note that our preferred estimate is the AIPW estimate since it corrects for bias and is doubly robust to model misspecification (Glynn and Quinn 2010). We provide more estimation details in online appendix D.

proximately the same difference that we found between Republican and Democratic appointees in our sample of district court cases (5 percentage points). However, note that there is not complete overlap in the time periods of our Ninth Circuit and district court datasets. We truncate our district court dataset to the period 1996–2012 and re-estimate our ARA effect for pro-defendant outcomes. The effect is slightly smaller (4.5 percentage points) but substantively similar (see column (4) of Table C1 in the online appendix).

While there are approximately four times as many authorized district court judgeships as circuit court judgeships, district court judges resolve approximately seven times as many cases. The results here cast doubt on the conventional wisdom that appointments to the circuit court are more politically important than those in the district court.

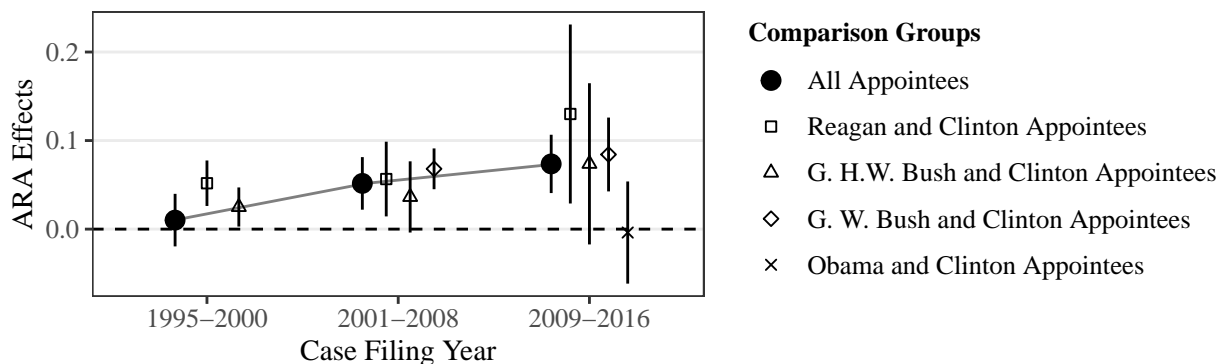
Our analysis is obviously not the last word on the question of the relative role of politics in the district versus circuit courts. The comparison we make here only covers civil rights cases from one circuit and seven districts over a decade and a half. There are also important questions to be answered about the reliability of estimates from circuit courts, given the evidence we provide that cases do not appear to be randomly assigned to panels. A promising avenue for future research would be to extend this analysis to other courts, time periods and areas of law.

However, our findings provide grounds to revisit claims about the relative importance of circuit and district courts. Our comparison between district and circuit courts improves upon past research since we do not analyze unrepresentative subsets of cases. Some prior research that compares cases across levels of the judicial hierarchy (such as Zorn and Bowie 2010; Epstein, Landes and Posner 2013) drops subsets of cases in ways that would bias findings. For example, Zorn (2010) only examines district court cases that were ultimately appealed. As our analysis above demonstrates, subsetting to appealed cases will yield biased estimates.

## Increasing ARA Effects

ARA effects have been increasing with time. To see this, we divide our data into three time periods, each of which corresponds to a different presidency: Clinton, G. W. Bush and Obama. The solid black circles in Figure 7 show how ARA effects have changed over these three time periods. When restricting our analysis to cases filed between 1995 and 2000, we estimate that assignment to a Republican appointee increased pro-defendant outcomes by only 1 percentage point. When looking at cases filed between 2009 and 2016, we estimate an ARA effect of 7.4 percentage points, a nearly eight-fold increase from the 1995–2000 time period. From 2001 to 2008, we estimate an ARA effect of 5.2 percentage points. And while the differences between the 2001–2008 ARA effect and the other two are not statistically significant, they are consistent with a story of increasing effects over time.

**Figure 7:** To examine whether ARA effects have changed over time, we subset our dataset to cases filed during each of the three presidencies that occurred in our sample time frame. We then estimate ARA effects on these subsamples, as well as president-specific effects (using Clinton appointees as the reference category).



There is a substantial literature exploring the increased politicization of the lower-court appointment process (for a review of the literature, see Scherer 2017). Whether due to the growing role of interest groups, the general polarization of political ideology or shifting beliefs in the importance of controlling the courts, the fights over appointments have undoubtedly become more contentious. For example, the U.S. Senate refused to confirm less than 10% of Carter’s nominations. By the

time that Obama was president, this figure had climbed to 38% (Scherer 2017).

We now explore whether polarization between the newest appointees is responsible for the increasing ARA effects. Investigating this issue requires us to slice our data thinly, but the evidence suggests that the increase in ARA effects is instead driven by factors that affect the judiciary as a whole, and not just new appointees.

We mostly draw on Figure 7 for our analysis. In addition to showing ARA effects during each time period (the solid circles), it displays the effect of assigning a case to each president's appointees rather than a Clinton appointee during each time period. We use Clinton appointees as a reference group, as his appointees are well represented across the entire dataset and have significant overlap with appointees of other presidents. We conduct separate sub-analyses for each president's appointees. We conduct separate analyses for each president because some of our division-year blocks do not include cases assigned to all presidents' appointees. For each analysis, we include only division-years where both presidents had appointees who were assigned cases.<sup>25</sup>

Consider first the effects within each of the three time periods. Though the large standard errors prevent us from making firm conclusions, there is little evidence that more modern appointees are more polarized. In both the first and last time period, it is the appointees of Reagan—the most historically distant president—that generate the highest point estimates. We are also unable to detect any difference between Obama's appointees and Clinton's appointees. The absence of larger effects among more recent appointees provides the first reason to doubt that effects are driven by polarization among new appointees.

Consider next how effects by appointing president have changed between the 1995–2000 and 2009–2016 time periods. Again, the evidence is only suggestive, but it points toward factors affecting the broader judiciary rather than only new appointees. For each president's appointees, the estimated effects—relative to Clinton appointees—are largest during the later period.

Finally, we note that DIME scores, a measure of political ideology based on campaign contribu-

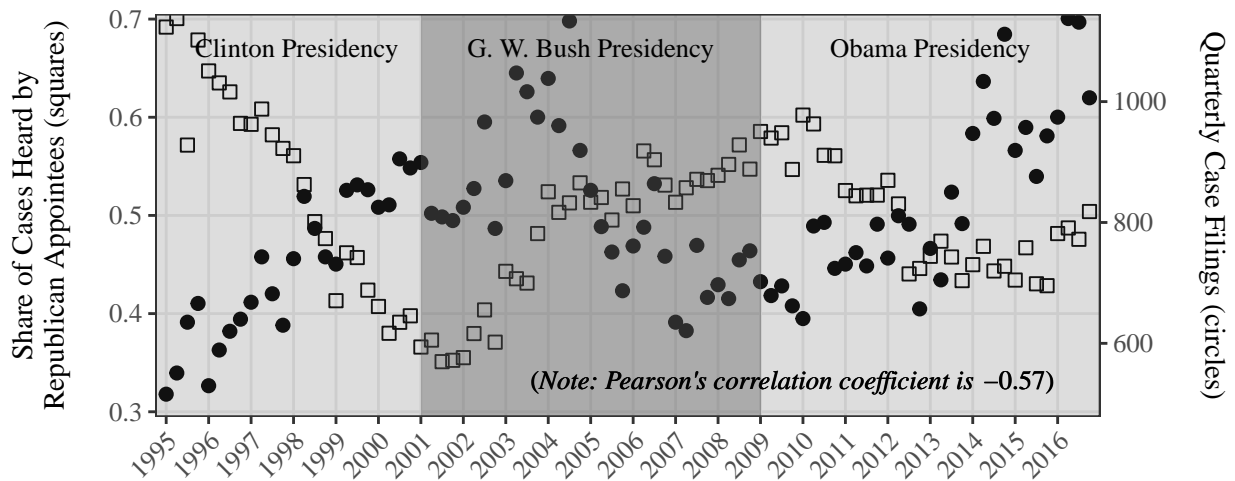
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<sup>25</sup>We exclude appointees of presidents prior to Reagan due to a low number of observations.

tions (Bonica and Sen 2017), have *not* polarized for modern appointees. We calculate the average DIME scores for the president’s appointees in our dataset (weighted by the share of cases heard by each judge in our sample). For Carter, Clinton and Obama, these estimates are  $-1.01$ ,  $-0.82$  and  $-0.98$ , respectively. For Reagan, G. H.W. Bush and G. W. Bush, these estimates are  $0.49$ ,  $0.27$  and  $0.44$ , respectively. By this measure, there is no clear trend of increasing polarization among new appointees, providing another reason to doubt that they are responsible for increasing ARA effects in our sample.

Collectively, the evidence does not support a story of polarization among only new appointees. But more research is required. First, as we have tried to stress, our investigation of heterogeneity over time is limited by our sample size. Second, there is still much to understand about how the stakes of appointments have changed. Most importantly, we have not addressed the nominations that were ultimately rejected by the Senate. Perhaps the stakes of the battle over appointments have increased more than we can detect, as those appointees who were not confirmed might have—if confirmed—generated larger ARA effects.

**Figure 8:** For each quarter of every year in our dataset, we plot the share of cases heard by Republican appointees (hollow squares) as well the total number of cases filed (solid circles).



Before concluding, we emphasize that our analysis in this paper does not fully capture *all* the possible ways that presidential appointments affect civil rights outcomes. Our data gives us new

(and causal) insight into the set of civil rights cases that were filed in court. However, presidential appointments might also affect which cases get filed in the first place. For example, new judicial appointments might alter people’s assessments of their chances of winning, changing their calculations about whether to file cases. Or companies might change their business practices in anticipation of new appointees’ jurisprudence. In fact, our dataset reveals a striking negative correlation between the share of cases heard by Republican appointees and the number of civil rights cases filed (Pearson’s correlation coefficient of  $-0.57$ , see Figure 8). Fully exploring the possible explanations for this correlation is beyond the scope of this paper, but it is an interesting and promising avenue for future research. It suggests that, in addition to the direct effects of appointments on civil rights cases that we document here, there could also be indirect effects.

## Conclusion

Using an original dataset of around 70,000 civil rights cases heard by almost 200 district judges, our analysis demonstrates that it matters whether Republican appointees or Democratic appointees are assigned to hear cases. We find that around 5 percent of cases in our sample (approximately 3,500 cases) were resolved differently *solely* due to whether their assigned judge was a Democratic or a Republican appointee. We also show that (1) this effect is similar to the effect we estimate in a sample of Ninth Circuit cases, and (2) this effect has grown over time.

For civil rights, our results suggest that the stakes of district court appointments—as well as the elections that determine which party will have the power to appoint judges—are substantial. Modern presidents typically appoint around 150 district judges during their first terms, significantly altering the chance that any given civil rights case is assigned to a Republican or Democratic appointee. And a president’s appointees will have a lasting impact: district judges hear an average of 900 civil rights cases during their tenure. Thus, if we make the admittedly strong assumption that the causal estimates in this paper generalize across jurisdictions and time, then Donald Trump’s

victory over Hillary Clinton in 2016 could result in anywhere between 6,750 and 9,990 more pro-defendant outcomes in federal civil rights cases, simply through his ability to appoint district judges to the bench during his presidency.<sup>26</sup>

Of course, there is still much to learn about the effects of appointments. What are ARA effects in other courts and in other areas of law? What is the substantive impact of these ARA effects on litigants? Do appointments also change the types and numbers of cases that are filed in the courts? We hope to have set the stage for these and other inquiries into the role of politics in the judicial appointment process and policy outcomes.

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<sup>26</sup>Additional details about this calculation in online appendix F.



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