Political Appointments, Careers, and Performance in the Public Sector: Evidence from U.S. Federal Judges*

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Abstract

This paper studies the dynamic effects of political appointments on the careers and performance of civil servants. We focus on U.S. federal judges, who are nominated by the President on the recommendation of their home-state senators. Leveraging a novel individual-level dataset linking judges and senators over more than two centuries (1789–2019), we employ difference-in-differences and event-study designs to compare judges whose recommending senators have left office to those whose have not. Following a recommender's exit, judicial performance declines: judges author fewer and lower-quality opinions, accumulate a larger backlog of civil cases, and see more of their decisions reversed on appeal. The main channel is an erosion of career prospects: once their recommenders leave, district court judges become markedly less likely to be promoted to higher courts. These findings show that political appointments shape the performance of civil servants well beyond the selection stage, with consequences that persist and compound throughout their careers.

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1. INTRODUCTION

Political appointments have historically been the dominant method of staffing the public sector. For most of modern history, appointments to government positions were determined by personal or partisan ties rather than competitive examinations (Finer, 1997; Weber, 1978). Bureaucratic merit systems — formal rules that govern the careers of officials and select them based on competence — emerged only gradually over the past two centuries, often as the result of civil service reforms in the late nineteenth and early twentieth centuries (Aneja and Xu, 2024; Evans and Rauch, 2000; Moreira and Pérez, 2024). Even today, many branches of government around the world continue to rely on discretionary appointments, especially for high-level positions (Lim and Snyder Jr, 2021).

Understanding how political discretion shapes the behavior of public employees is central to evaluating the performance of the state. Political appointments can foster alignment between bureaucrats and elected officials (Besley and Ghatak, 2005; Spenkuch et al., 2023), but create ties that can affect incentives once political conditions change. When advancement or recognition depends on political sponsors, the departure of such sponsors from office may alter the motivation and effort of their appointees, with consequences for bureaucratic performance and, more broadly, for the development of state capacity (Besley and Persson, 2011; Xu, 2018). The empirical study of these mechanisms presents several challenges. First, the output of public employees is often difficult to measure (Besley et al., 2022). Second, detailed information on the political sponsors and careers of public sector workers is rarely available. Third, few settings allow researchers to observe the dynamic consequences of political appointments over long time horizons.

In this paper, we study a setting in which political discretion remains a defining feature of public employment: the U.S. federal judiciary. Since 1789, federal district court judges have been nominated by the President based on recommendations from their home-state senators who share the President's party affiliation (Domnarski, 2009; Rutkus, 2016). This institutional arrangement creates persistent political ties between judges and their recommending senators, providing an ideal context to study how these ties — and their dissolution — affect performance and career dynamics within a bureaucracy.

Theoretically, the effect of losing political sponsors on judicial performance is ambiguous. Lifetime tenure and judicial independence imply that, in principle, judges should be largely unaffected by changes in the political environment. Yet, if recommending senators are instrumental not only for the initial appointments, but also for the career prospects of district court judges, their departure may weaken career incentives and reduce effort. Alternatively, in the aftermath of their recommenders' exit, judges may respond by working harder to compensate for the loss of political support. Whether and how the exit of recommending senators affects judicial performance is therefore an empirical question.

To answer this question, we assemble a novel individual-level dataset linking federal judges and U.S. senators from 1789 to 2019, which allows us to identify the political sponsors and career trajectories of all judges. We then combine it with information on both the quantity and the quality of the output of judges over our sample period. Leveraging the staggered and plausibly exogenous timing of the exits of senators from Congress, we estimate within-judge effects of changes in political ties by means of a stacked-by-event design (Cengiz et al., 2019; Deshpande and Li, 2019).

Our findings indicate that the performance of district court judges declines significantly following the departure of their recommending senators. We first document negative effects on judicial opinions: Judges reduce the number and the quality of their opinions, as measured by length, number of citations included in the opinion, and number of citations received by the opinion. This reflects on the pending caseload that judges face and on the quality of their decisions: Once their recommenders leave Congress, judges accumulate a larger backlog of civil cases and are more likely to have their decisions reversed by appellate courts. Event-study estimates show that these effects emerge only after the senator's exit, supporting the parallel-trend assumption underlying our identification strategy.

These negative effects are widespread. Heterogeneity analyses show that they hold across judges with varying levels of quality and tenure, as well as across judges appointed by Republican and Democratic senators and judges with one and two sponsors at the time of appointment. Furthermore, the decline in performance occurs regardless of whether the senator's departure from office is due to their own choices — such as retirement,

resignation, or pursuing another office — or to a non-voluntary event — such as death or electoral defeat. The findings are also robust to a range of sensitivity checks, including an alternative approach to identifying political recommenders.

To confirm that the observed decline in judicial productivity is driven by the exit of recommenders, rather than broader partisan dynamics, we further show that the treatment effects remain even when a judge's recommender is replaced by a co-partisan. This suggests that ties to specific senators — rather than shared party affiliation — are responsible for the observed changes in judicial output, so that the incentives to work hard may only survive as long as recommenders keep their seats in Congress.

Next, we investigate the mechanisms linking the exit of recommending senators to the observed decline in judicial performance. We test four potential channels. We begin with career incentives: Since senators play an important role in the nomination of higher-level judges (Domnarski, 2009), we hypothesize that the loss of a recommending senator may decrease the opportunities for career advancement of a district judge. Given that district court judges hold lifetime appointments, established economic theories of career incentives (Gibbons and Murphy, 1992; Holmström, 1999; Rosen, 1986) imply that the departure of a key political sponsor may reduce the incentives to exert high effort.

Three complementary pieces of evidence point to the role of career incentives in explaining the observed drop in judicial performance. First, we find a substantial negative effect of recommenders' exit on the career prospects of judges. Judges experience a sharp decline in the probability of being promoted to an upper-level court following the departure of their recommenders, effectively closing off opportunities for advancement within the federal judiciary. In line with the rules governing federal judicial nominations, this effect is concentrated in years when judges share partisan affiliation with the sitting president — and are thus best positioned to benefit from their recommenders' endorsement.

Second, we show that — while their recommenders are still in office — the performance of district court judges is a strong predictor of their promotion probability. This reinforces our interpretation that, once recommenders leave and promotion becomes unattainable, judges may lose the incentives to keep up with their performance standards. Third, we demonstrate that the drop in performance following the exit of recommenders

only materializes after 1891 and 1925, the years of the "Evarts Act" and of the "Judges' Bill", two reforms that dramatically changed the career prospects of district court judges. The Evarts Act of 1891 created the federal courts of appeals, opening up the possibility for district court judges of being elevated to the appellate bench. The Judges' Bill of 1925, on the other hand, increased the prerogatives of appellate courts, creating stronger incentives for district judges to seek elevation and for political actors to view experienced district judges as more suitable candidates to the appellate bench.

As a second potential mechanism, we test whether the departure of recommenders affects judges' decisions about their own career termination. To this end, we re-estimate our main specification using as outcome an indicator for whether a judge takes senior status — a form of pre-retirement that significantly reduces the caseload while also changing the type of cases that a judge is assigned to. We find no significant effects on this outcome, suggesting that the observed decline in judicial output is unlikely to be determined by a judge's own decision to progressively retire from the judiciary.

The third channel we investigate is the potential role of recommenders in influencing the type of cases that their appointees are allocated. While judicial protocols entail the random assignment of cases on a rotation basis within each jurisdiction, it may still be the case that influential senators have the soft power to help their appointees secure or avoid certain cases. Our difference-in-differences estimates do not support this possibility: The exits of recommenders do not have significant effects on the types of cases that their appointees receive, nor on the identity of the plaintiffs and defendants involved.

Finally, we test whether the exit of senatorial sponsors affects the degree of recognition for judges' work among their colleagues. To this end, we decompose the citations received by a judge's opinions by the time at which these opinions were written. This exercise reveals that the observed drop in citations is determined not only by the opinions written after the exit of recommenders, but also by those written when the recommenders were still in office. This suggests that, even holding the quality of a judge's work constant, their work becomes less likely to be recognized once senatorial sponsors leave, which may constitute an additional reason for judges to reduce their effort.

This paper makes several contributions. First, it provides new evidence on how pa-

tronage shapes the performance of public-sector employees. Existing work has largely focused on the selection effects of political appointments, asking whether patronage places less qualified individuals in office or affects the ideological composition of public organizations (e.g., Colonnelli et al., 2020; Gallo and Lewis, 2012; Spenkuch et al., 2023; Xu, 2018; Voth and Xu, 2022). Here, we show that political ties continue to matter long after appointment. Leveraging the departure of recommending senators, we document how the performance of appointed officials depends not only on who helps them obtain office, but also on whether those political sponsors remain in positions of influence.

The paper also contributes to the literature on career incentives in the public sector, which has consistently shown the importance of motivating public employees by providing meaningful opportunities for career progression (Bertrand et al., 2020; Deserranno et al., 2025; Finan et al., 2017; Karachiwalla and Park, 2017; Kim, 2022; Nieddu and Pandolfi, 2022). A central challenge in this literature is that promotions are often jointly determined with performance, making it difficult to isolate the incentive effects of career advancement opportunities. Thanks to our unique setting, we can show that the departure of a political sponsor substantially reduces the career prospects of judges and is followed by a deterioration in performance. These findings suggest that career concerns remain an important source of motivation even in occupations characterized by strong employment protections, lifetime tenure, and a high degree of formal independence.

Our results further speak to work on the political economy of the judiciary. Much of the existing literature examines how political considerations affect judicial decisions, focusing on ideology, partisanship, or electoral incentives (Berdejó and Yuchtman, 2013; Besley and Payne, 2013; Canes-Wrone et al., 2014; Gordon and Huber, 2007, 2004; Lim et al., 2015; Cohen and Yang, 2019; Sunstein et al., 2007; Schanzenbach and Tiller, 2008). We highlight a different channel: political connections affect not only what judges decide, but also how effectively they perform their judicial duties.

This distinction is important. For citizens and firms, the quality of a judicial system depends not only on the correctness of legal decisions, but also on the speed with which disputes are resolved. Delays in adjudication generate substantial economic and social costs, increasing uncertainty, discouraging investment, and limiting access to jus-

tice (Acemoglu and Johnson, 2005; Kondylis and Stein, 2023; La Porta et al., 1997, 2004; Ponticelli and Alencar, 2016). By showing that the loss of political sponsors leads judges to produce fewer opinions and accumulate larger case backlogs, we identify a previously overlooked determinant of judicial efficiency.

Finally, our findings contribute to the broader debate on the merits of alternative appointment procedures for high-level public officials (Huber and Ting, 2021), challenging the view that lifetime appointments insulate public officials from political influence. While federal judges are protected from electoral pressures and enjoy substantial independence once appointed, our results demonstrate how their performance nevertheless responds to changes in their political environment. This suggests that the influence of politics on public institutions extends beyond selection and into the day-to-day functioning of the state itself.

The remainder of the paper is organized as follows. Section 2 provides background on the U.S. federal court system, with particular emphasis on the role of home-state senators in the nomination process for district court judges. Section 3 describes the data sources and key features of our dataset on federal judges and U.S. senators, along with the procedure used to match them. Section 4 outlines the empirical strategy. Section 5 presents the main results on judicial performance, and Section 6 examines the potential mechanisms that could explain these results. Section 7 concludes.

2. Institutional Background

2.1. US Federal Judiciary. The U.S. federal judiciary has three tiers: 94 district courts, 13 courts of appeals (also known as circuit courts), and the U.S. Supreme Court.

District courts are the principal trial courts of the federal system and have jurisdiction over most federal civil and criminal cases. District judges are responsible for managing litigation from filing through final judgment, including supervising discovery, resolving procedural disputes, conducting hearings, presiding over jury and bench trials, sentencing criminal defendants, and issuing written decisions on contested legal questions (Administrative Office of the United States Courts, 2025a, 2026).

Within a district court, cases are generally assigned to judges through random assignment systems established by local rules. Although procedures vary somewhat across districts, the prevailing principle is that newly filed cases are distributed randomly among eligible judges to equalize workloads and reduce opportunities for litigants to select a preferred judge. Exceptions may be made for related cases, recusals, multidistrict litigation, or specialized court divisions. Once assigned, a district judge ordinarily retains responsibility for the case through its resolution. Given the volume of filings, district judges manage substantial caseloads.

In fiscal year 2025, the federal judiciary reported 382,692 new district court filings and 677 authorized district judgeships (Administrative Office of the United States Courts, 2025b), implying an average of roughly 565 new filings per judgeship, although actual workloads differ considerably across districts and case types. The work of judges on cases may terminate with the production of judicial opinions: written decisions that explain the court's reasoning, apply legal rules to the facts of a dispute, and provide a record for appellate review. Routine matters are resolved through short orders, while more articulated cases — about one out of ten — require the publication of one or more opinions to justify the court's conclusions and potentially guide future litigation.

2.2. Nomination of Federal Judges. Unlike state-level judges, who are in part elected by voters, all federal judges are appointed for life by the President of the United States. However, while the president formally makes these nominations, the process — particularly at the district court level — is mostly shaped by other political actors. In practice, candidates for district court judgeships are traditionally recommended by home-state senators who share the president's party affiliation. If no such senators exist, the president often consults with other high-ranking state officials from the same party, such as House representatives (Rutkus, 2016), but may also hear the opinions of home-state senators from the opposing party.

After vetting the proposed candidate(s), the president submits a nomination to the Senate Judiciary Committee, which then conducts a confirmation hearing that includes a public question-and-answer session with the nominee. At the confirmation hearing,

appointees are usually introduced by their recommenders, who summarize their qualifications and — most of the times — provide information as to the origins of their acquaintance. Following the hearing, the committee refers the candidate to the full Senate with a favorable, unfavorable, or no recommendation. In the vast majority of cases, the nominees are reported favorably and in a relatively swift manner.[1] The full Senate then votes on the nomination, typically by unanimous consent. Aside from the Senate, the only other institution with a formal role in the process is the American Bar Association (ABA), which provides a non-binding evaluation of the nominee — not qualified, qualified, or well qualified — before the Judiciary Committee takes action.[2]

The practice of deferring to home-state senators in the nomination of district court judges has become a deeply entrenched norm, applied consistently across administrations of both parties. As a result, district court judges are often associated more closely with their senatorial recommenders than with the nominating president. As U.S. Attorney General Robert F. Kennedy once put it, "Basically it's senatorial appointment with the advice and consent of the president" (O'Brien, 1986, p. 40).

This practice has drawn criticism for favoring politically connected candidates over more qualified ones. As one U.S. senator openly acknowledged, it serves as an "important source of political patronage" for members of the Senate (Tydings, 1977). Factors influencing senatorial recommendations often include personal ties such as friendship, acquaintance, or family relationships (Domnarski, 2009). Moreover, political alignment plays a substantial role in selection: Most district judges were politically active prior to their appointment (Carp et al., 2019).

While home-state senators are generally thought to influence only district court nominations, anecdotal and documentary evidence demonstrate their active role in circuit court appointments (Domnarski, 2009). Court of appeals nominees are introduced by home-state senators to the Judiciary Committee, and appointees themselves often de-

[1] However, longer confirmation times — and occasional rejections — have become more common in recent decades (Binder and Maltzman, 2009).

[2] Until roughly 1990, the Committee employed a four-tier ranking that added "exceptionally well qualified" as a top category above "well qualified"; this rating was subsequently discontinued, leaving the present three-tier scale.

scribe senators as key sponsors for a seat on the appellate bench.[3] As reflected in hearings' transcripts, home-state senators are particularly likely to endorse the promotion of judges that they had initially recommended for the district-court level.[4] In their endorsements, several senators take particular care to describe how the effort on the district bench has been a key determinant of a judge's advancement within the federal judiciary.[5]

3. DATA

To study the impact of senators' tenure on the performance and careers of federal judges, we assemble a novel dataset that combines information on U.S. federal judges and senators covering the period from 1789 to 2019.

3.1. US Federal Judges Data. Data on judicial careers come from the Biographical Directory of Article III Federal Judges, compiled by the Federal Judicial Center (FJC), the research and education agency of the judicial branch of the U.S. government. The directory includes biographical information on judges appointed since 1789 to the U.S. district courts, courts of appeals, Supreme Court, and Court of International Trade, as well as now-defunct courts such as the U.S. circuit courts, Court of Claims, Customs Court, and Court of Customs and Patent Appeals. The FJC data provide complete career histories for federal judges, including the exact dates of each appointment.

Our main source of data on judges' performance is CourtListener, a legal research platform operated by the non-profit Free Law Project. CourtListener aggregates case law from a wide range of sources, including court websites, law libraries, and academic

[3] In the questionnaires submitted to the Senate Judiciary Committee, home-state senators are the most mentioned category of politicians when candidates for courts of appeals are asked about details on their selection process.

[4] In several cases, senators reveal that they had been monitoring the performance of their appointees throughout their district-court tenure. As an illustration, in recommending the promotion of Judge Levin H. Campbell to the Court of Appeals for the First Circuit in 1972, Senator Brooke said that he "followed Judge Campbell's decisions closely and [...] found them to be consistently, scholarly, inevitably logical and imminently fair" (United States Senate Committee on the Judiciary, 1972, p. 6).

[5] In the 154 promotion hearings that we could retrieve, home-state senators mention the effort that the appointee put as a district-court judge in 48% of cases. For instance, in endorsing their promotion to the Court of Appeals for the Sixth Circuit, Senator Metzenbaum described judges Krupansky and Contie Jr. as "the workaholics of the federal bench" (United States Senate Committee on the Judiciary, 1982, p. 76).

archives, and contains more than nine million legal opinions from federal, state, and specialty courts. For the purposes of this study, we examine about 520,000 opinions authored by the district court judges between 1789 and 2019 and 554,000 authored by appellate judges between 2000 and 2019.[6]

Judicial opinions — written statements explaining the court's ruling — constitute a central component of a judge's work and are typically authored individually at the district court level. While not constituting binding precedents, judicial opinions are commonly used as the basis for future decisions and, as such, cited as *persuasive authority*. Opinions also typically include several citations of prior opinions that have provided useful guidance over the case's subject-matter. Because new cases are randomly assigned to judges on a rolling basis, variation in the number and the quality of opinions across judges reflect differences in working standards rather than differences in caseload composition. Prior studies have indeed relied on opinions as a primary measure of judicial performance (Ash et al., 2024; Ash and MacLeod, 2024, 2015; Posner, 2008).

For each judge-year observation, we compute four performance outcomes using district court judicial opinions. The first captures the *quantity* of output: the total number of judicial opinions authored by a judge in a given year. While not all case resolutions require a written opinion, this metric serves as a close proxy for the number of *substantive* rulings issued and cases closed.

Next, we compute three outcomes to proxy for the *quality* of judicial output: the average number of words per opinion (excluding citations), the average number of citations included, and the number of citations received by judge i at year t, normalized by the number of opinions authored by that judge until that year. While citations do not directly measure the correctness of a decision, they serve as informative indicators. More citations received imply that the opinion proved useful to future courts, whereas more citations included suggest that a judge made a greater effort to ground their reasoning in precedent.

One additional measure we derive from judicial opinions is the share of decisions

[6]This information is assembled from the March 2026 CourtListener bulk data release.

made by a judge on a given year that are reversed by appellate courts.[7] To the extent that courts of appeals are capable of correcting errors made by district courts, this constitutes a direct measure of the accuracy of a judge's decisions — arguably the most important aspect of a judge's overall performance. A reversal is observed only for decisions that are appealed and only when the appellate ruling can be linked back to the district judge whose decision was under review. The structural information needed for this linkage — a district docket number and a named district judge in the appellate opinion — is very sparse before the 21st century, so we restrict analyses of this outcome to the years from 2000 onward.[8] Together, these constraints imply that we code a reversal outcome for roughly 18% of the appellate decisions in our 2000–2019 window (about 101,000 of 554,000).

The last outcome we analyze measures the degree to which judges have accumulated a backlog of cases to solve. We draw this information from Civil Justice Reform Act (CJRA) Reports, available at the judge level semiannually starting in 1998. Mandated by the Civil Justice Reform Act of 1990, these reports are compiled by the Administrative Office of the U.S. Courts and provide standardized statistics for each U.S. district judge. Among the various metrics reported, we focus on the number of civil cases pending for more than three years — a key indicator of judicial backlog and protracted litigation delays.[9] This outcome constitutes a crucial measure of efficiency in the context of the U.S. Federal Judiciary. In fact, with about 584,000 pending cases and 65,836 pending more than three years as of 2023, backlogs are among the most pressing problems of the U.S. Federal Judiciary (CJRA, 2023).

[7] Unlike the four outcomes described in the previous paragraph, which use opinions authored by district court judges, this measure is built from opinions written by appellate judges, which we link back to the district judge whose decision is under review.

[8] Before 2000, the information needed to link an appellate decision to the reviewing district judge and code its disposition is available for less than 3% of appellate decisions; from 2000 onward this rises to about 18%.

[9] Our baseline measure excludes eight judges who served as transferee judges for large multidistrict litigation (MDL) proceedings. Because the Judicial Panel on Multidistrict Litigation consolidates mass-tort and product-liability cases before a single judge, these judges' counts of cases pending over three years can reach the tens of thousands — over 120,000 in one instance — reflecting the structure of MDL assignment rather than their own pace of work. We show that our results are robust to including them, as well as to alternative transformations of the outcome, in Section 5.3.

3.2. US Senators Data. Data on U.S. senators are compiled from three sources: the Biographical Directory of the United States Congress, the website Voteview.com, and the Roster of Members of the United States Congress (McKibbin et al., 1984). By combining these sources, we construct complete political biographies of all senators who served between 1789 and 2019.

3.3. Matching of the Datasets. We focus on federal judges who were appointed to a U.S. district court.[10] We follow each judge's career in the district courts until their promotion, retirement, resignation, or death — whichever comes first. In doing so, we also track whether and when the senator(s) who recommended their nomination left Congress.

To identify the recommending senator(s) for each judge, we merge this panel with the data on senators' careers. We define as recommenders the senator or pair of senators who: (i) represented the state of appointment at the U.S. Senate at the time of the nomination, and (ii) belonged to the same party as the nominating president.[11] Because our treatment of interest is the departure of a judge's recommenders from Congress, we exclude judges appointed in states where no senator shared the president's party at the time of the nomination.

The final sample includes 1,885 judges, appointed between 1789 and 2019.[12] Table A.1 provides summary statistics. The average judge authors approximately 8 opinions per year. These opinions are, on average, 3,450 words in length, include 14 citations, and receive 0.35 citations per year. Judges have, on average, 9.5 civil cases pending for more than three years and 19% of their opinions reversed on appeal. About half of the judges have two recommending senators. Six percent of judges in the sample are promoted to an appellate court, with an average time to promotion of seven years. Figure A.1 shows the distribution of promotions by year. Roughly half of the judges in the sample are appointed by a Democratic president, and half by a Republican.

[10] We exclude: (i) judges appointed in years when their state did not yet have Senate representation, and (ii) judges serving in the district courts of Washington, D.C., and Puerto Rico.

[11] This procedure reflects the institutional practice of senatorial recommendation, described in Section 2.

[12] As discussed in Section 3, judicial opinion data are available from 1789 onward, backlog data from 1998, and reversal rates from 2000 onwards.

4. EMPIRICAL STRATEGY

The identification strategy leverages the staggered exit of recommending senators from Congress. Two key features allow us to use these events for identification. First, judges have no influence over the timing or the duration of their recommenders' tenure in the Senate. Second, approximately half of all exits occur for plausibly unanticipated reasons — such as death in office, loss in a general election, or loss in a primary — which form the focus of our main specification.[13] Hence, the timing of treatment is plausibly exogenous. The empirical strategy compares the evolution of judicial outcomes between judges whose recommenders leave Congress at different times.

Given the presence of staggered treatment assignment, we adopt a "stacked-by-event" design (Aneja and Xu, 2024; Cengiz et al., 2019; Deshpande and Li, 2019). This estimator accounts for the pitfalls of two-way fixed effects estimators in the presence of staggered adoption (Borusyak et al., 2024; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). In this context, the key concern is including already-treated judges in the control group. In the presence of heterogeneous treatment effects across judges experiencing the exit of senators at different points in time, this can lead to biased estimates.

The "stacked" design treats each senator exit wave as a separate sub-experiment. For each wave, we construct a difference-in-differences estimate comparing judges that lose their recommenders during that wave to those who do not. We then stack these wave-specific estimates to obtain a pooled effect of senators' exits across all events. This design makes the composition of comparison groups explicit. Specifically, the control group for each event consists of judges who have not yet experienced a senator's exit by the end of the event window ("not-yet-treated" judges).

Let j index the senator exit wave, and k denote the number of years relative to the exit, where $k = 0$ corresponds to the year of the exit and negative values denote years before the exit. We restrict the pooled sample to a 12-year symmetric window around

[13] These exits account for 49% of all exits between 1789 and 2019. Other exits occur when a senator chooses not to seek re-election, seeks or accepts another office, or resigns. Including these additional cases yields similar results (see Section 5.3).

each event.[14] For judge i, senator exit wave j, and year k relative to the exit, we estimate:

$$y_{ijk} = \beta(Treated_{ij} \times Post_{jk}) + \theta_{ij} + \tau_{jk} + \epsilon_{ijk} \tag{1}$$

where $Treated_{ij} = 1$ if judge i's recommender exits Congress in wave j, and 0 otherwise. The outcome of interest is denoted by y_{ijk}. The variable $Post_{jk}$ is defined as $Post_{jk} = \mathbb{1}\{k \geq 0\}$ for each wave j. The term τ_{jk} represents wave-specific year fixed effects, which control for common temporal shocks across judges in the same wave. Because the same judge may appear as both treated and control in different exit waves, we include judge fixed effects θ_{ij} separately for each event wave.[15]

The main parameter of interest is β, which captures the effect of losing the recommending senator relative to control judges who have not yet experienced an exit in wave j. We estimate Poisson Pseudo-Maximum Likelihood (PPML) regressions, except for specifications using the reversal rate as the outcome, which we estimate via OLS. Standard errors are clustered by judge, to account for serial correlation and for the repeated appearance of judges across waves, either as treated or control units.

A causal interpretation of $\hat{\beta}$ requires that, in the absence of treatment, outcomes for treated and control judges would have followed parallel trends. To assess this assumption and explore the dynamic evolution of treatment effects, we also estimate:

$$y_{ijk} = \sum_{l=-5}^{+6} \beta_l (Treated_{ij} \times \mathbb{1}\{k = l\}) + \theta_{ij} + \tau_{jk} + \epsilon_{ijk} \tag{2}$$

with the year immediately preceding the senator's exit ($k = -1$) omitted as the reference category. In this specification, the coefficients β_l capture the difference in outcomes for treated judges l years relative to the exit, compared to the pre-exit year, and relative to the evolution of outcomes for control judges who have not yet been treated.

As discussed in Section 2, judges may be recommended by one or two senators. Our main interest is assessing the impact of losing all political ties, so we focus on the exit

[14] Six years corresponds to the length of a full Senate term. Results are robust to alternative window lengths (not reported for brevity but available upon request).

[15] Control judges who experience a senator's exit within the event window of wave j are excluded from that wave's estimation.

of the last active recommender. Thus, for judges with two recommenders, we define treatment as the exit of the last remaining recommender.[16]

5. Main Results

5.1. Effect of Senator's Exit on Judge's Performance. Table 1 reports estimates of β from Equation (1), which capture the causal effect of the exit of the last recommending senator from Congress on judicial performance. Columns (1) through (4) present results for the four measures of performance based on judicial opinions; column (5) displays the coefficient for the share of decisions reversed on appeal; finally, column (6) shows the effect on the number of civil cases pending more than three years.

Column (1) reveals a substantial decline in judicial output following the departure of the judge's last active recommender: this event leads to a 13% reduction in the number of opinions authored.[17]

As noted above, the number of opinions that a judge authors on a given year is mechanically tied to the number of cases they close during that period. Thus, the negative and significant coefficient in column (1) may reflect one of two dynamics: judges may take more time per case in order to produce higher-quality opinions, or they may simply slow down, closing fewer cases without any improvement in quality. The estimates in columns (2) through (4) support the latter interpretation. Following the exit of the recommending senator, judges author opinions that are, on average, shorter by 14% (column 2), include 14% fewer citations of prior work (column 3), and receive 16% fewer citations from other opinions (column 4).

The results in column (5) reinforce this interpretation. In line with their declining quality, the opinions of district court judges become 5 percentage points more likely to be reversed by appellate courts after the exit of the last recommender. Given a baseline reversal rate of 16%, this amounts to a 31% increase in the likelihood of reversal.

[16] In Section 5.2, we also explore the effects of each of these exits separately.

[17] Since the coefficients in columns from (1) to (4) and in column (6) are estimated using Poisson regressions, the magnitude of the effects can be calculated as follows: $\%\Delta y = 100 \times (e^{\beta} - 1)$.

Table 1: Effect of Senator's Exit on Judge's Performance

VARIABLES	Judicial Opinions					
	Total Authored (1)	N. of Words (2)	Citations Included (3)	Citations Received (4)	Share Reversed (5)	Cases Pending (6)
Treated x Post	-0.14**	-0.15***	-0.15***	-0.17***	0.05**	0.80**
	(0.06)	(0.04)	(0.03)	(0.05)	(0.02)	(0.32)
Observations	41,194	41,194	41,022	39,561	7,560	9,282
Mean of DV	7.58	3,144.76	12.60	0.33	0.156	5.132
Judge x Event FE	Y	Y	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y	Y	Y
Estimator	PPML	PPML	PPML	PPML	OLS	PPML
Sample period	1789–2019	1789–2019	1789–2019	1789–2019	2000–2019	1998–2019

Notes: The unit of observation is judge × year × senator exit wave. The dependent variables are: the number of opinions authored by judge i in each year (column 1); the average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the average number of citations included in the opinions authored by judge i in each year (column 3); the average number of citations received in year t by opinions authored by judge i, normalized by the number of opinions authored by judge i up to t (column 4); the share of judge's i decisions reversed on appeal (column 5); and the number of civil cases assigned to judge i that have been pending for more than three years as of year t (column 6). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge × event and year × event fixed effects. The mean of each dependent variable is calculated for judges that lost their recommender in wave j, for the years prior to the recommenders' exit. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Finally, the results in column (6) show how this decrease in performance translates into a larger backlog of cases. After the departure of their recommender, judges accumulate a significantly higher number of civil cases. Given that judges have, on average, 5.13 pending cases while their recommender remains in office, the estimated coefficient — corresponding to a 123% effect — implies an increase of more than 6 additional pending cases. Taken together, these results show that the exit of recommending senators leads to a decline in both the quantity and the quality of judicial output.

Figure 1 displays estimates of the β_l parameters from Equation (2), for $l \in [-5, +6]$, using the year prior to the senator's exit ($l = -1$) as the omitted category. The figure presents six event-study panels, one for each of the outcomes considered in Table 1. For all outcomes, the estimates show no evidence of pre-trends. As for the evolution of the treatment effect, the estimates reveal similar but not identical patterns across the six de-

pendent variables. The measures of judicial productivity — i.e., the number of opinions authored and the number of pending civil cases — show an immediate change in the expected directions. The other four outcomes change in a somewhat slower manner, with the treatment effects achieving statistical significance in the third year since the loss of the last recommender. Finally, for all outcomes, the effects tend to compound over time, implying that the loss of recommenders causes a permanent erosion of judges' performance, not just a temporary shock that is reabsorbed over time.

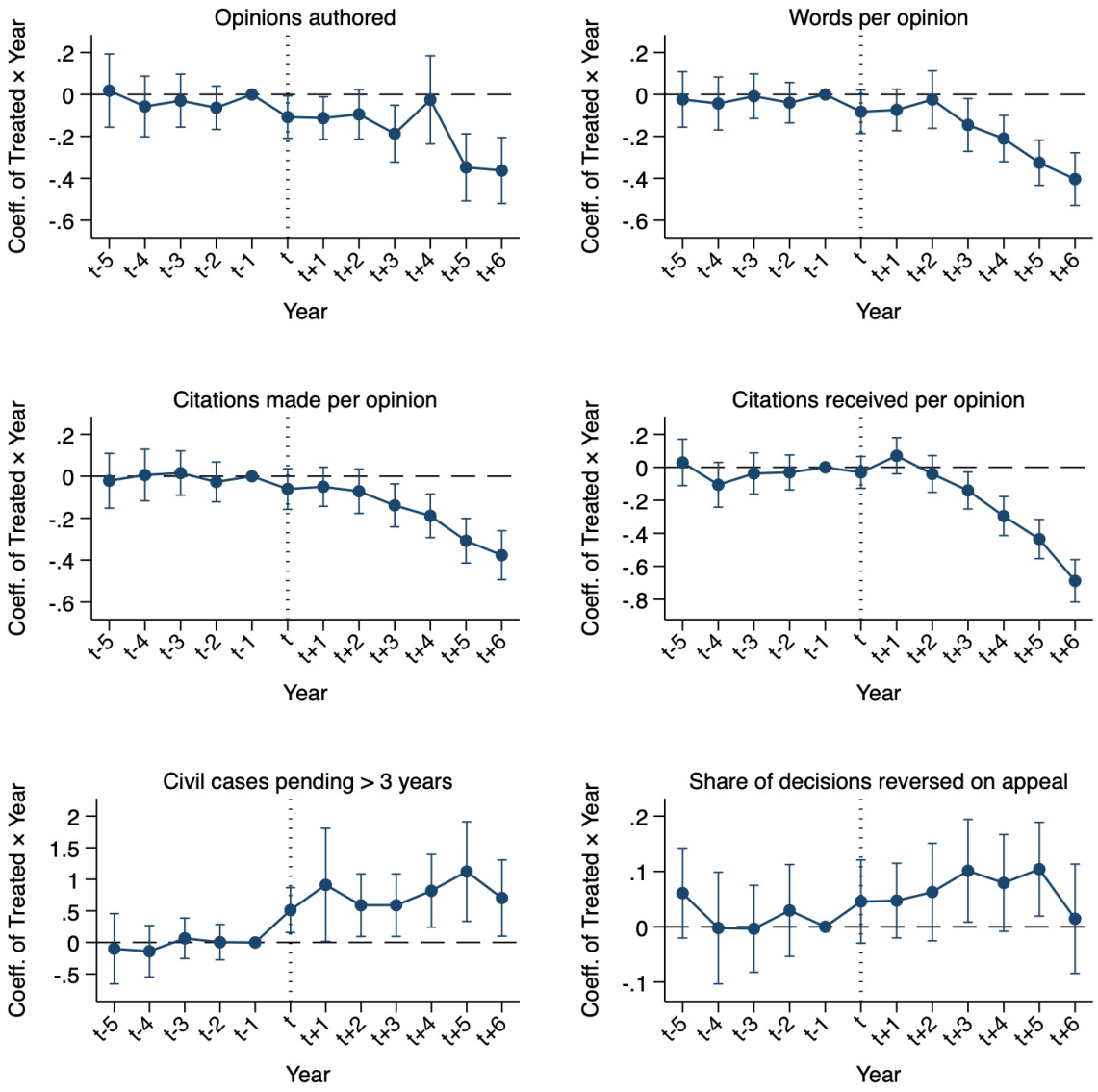
5.2. Heterogeneity Analyses. The results presented above show that the performance of federal district court judges significantly declines following the departure of all recommending senators. But are these effects driven specifically by the exit of the unique recommender (for judges with only one) or by the last active recommender (for judges with two)? Figures A.2 and A.3 address this question.

Across all specifications, performance declines in both cases, with slightly more pronounced effects when the departing senator is the judge's unique recommender. This resonates with the fact that judges appointed by a single recommender work in more politically divided states. In such states, the single senator representing each party is likely to wield a stronger influence on the sitting President than in contexts where the President can consult with two senators of his own party. Hence, losing senatorial support can be more consequential in the former than in the latter scenario.

A further question is whether the effects documented above are homogeneous across judges with different characteristics. To examine this, we introduce interaction terms into Equation (1) and assess heterogeneity in the results from Table 1 along four key dimensions. First, we investigate the role of partisanship. Since senators' involvement in the nomination process can vary widely across cases (Domnarski, 2009), it is possible that Democratic and Republican judges respond differently to the loss of their recommending senators. However, as shown in Figure A.4, the decline in judicial productivity appears across judges appointed by both Democratic and Republican presidents, with somewhat larger effects on opinions authored and share of reversals for Democratic appointees.

Second, we examine whether professional competence moderates the impact of losing

Figure 1: Effect of Senator's Exit on Judge's Performance
Dynamic Effects



Notes: The figure reports estimates from Equation (2), an event-study specification that augments Equation (1) by allowing the estimated difference between treated and control judges to vary by year relative to the one before the senator's exit. The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (middle left); the average number of citations received in each year by the opinions authored by judge i up to that year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left); and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All specifications include judge $\times$ event and year $\times$ event fixed effects and are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

political sponsors. To assess this, we link each judge to the rating they received from the American Bar Association (ABA) at the time of their appointment and divide the sample into two groups: those rated Not Qualified or Qualified (low-qualified), and those rated Well Qualified or Very Well Qualified (high-qualified). As shown in Figure A.5, the effects of recommender exit are present for both groups, suggesting that professional qualifications do not shield judges from the consequences of losing political support. However, it is interesting to note how the increase in case reversals is more pronounced for low-qualified judges. Speculatively, this may reflect the fact that appellate courts may become less reluctant to reverse the decisions of poorly qualified judges once these judges have lost their senatorial sponsors.

Third, we examine whether the effects vary by judicial tenure at the start of the 12-year observation window. Judges are divided into three groups based on tenure quartiles: newly appointed judges (1 year), early-career judges (2-3 years), and longer-serving judges (4+ years).[18] Overall, Figure A.6 shows that most of the effects of recommender exit are consistently present across all tenure groups, suggesting that political sponsorship influences judicial performance throughout the course of a judge's career. Nonetheless, it is worth noting how the effects tend to be slightly larger in magnitude for judges in the middle category (2-3 years of tenure at the start of the event window). Hence, losing senatorial support seems less consequential for judges who — at the time of the shock — have either not yet settled a performance standard or have long established their performance record. Because these estimates compare treated and control judges within the same experience bins, they also help address the concern that the main results reflect the two groups being observed at different career stages: the decline appears within every tenure group, not only where treated and control judges happen to differ in experience.

Fourth, we examine whether the impact differs depending on the reason for the senator's departure. We distinguish between non-voluntary exits (the focus of our preferred specification) — due to death in office or electoral defeat — and voluntary exits, such as retirement or resignation. Figure A.7 presents the results. It is remarkable to see how the

[18]These groups are constructed by dividing judges into quartiles based on their tenure at the beginning of the 12-year window, with 1 year of tenure corresponding to the bottom two quartiles.

effects are very similar across the two categories for all six outcomes. This, besides confirming how pervasive the baseline effects are, reassures us that the effect is not specific to judges who cannot anticipate the exit of their recommender. This finding lends support to the identification strategy underlying our analyses and conforms with the absence of pre-trends in Figure 1.

Finally, we check for heterogeneous effects based on the party affiliation of the senator who replaces the recommender. The results are in Figure A.8. For all outcomes, the effects are either similar or larger in magnitude for judges whose recommender is replaced by a co-partisan, showing that it is the departure of the senator who recommended the judge — rather than changes in partisan alignment — that drives the observed decline in productivity. This is in line with the selection procedure described in Section 2, which is based on personal rather than partisan ties.

Taken together, these heterogeneity analyses show that the consequences of senatorial exits are broad, affecting judges regardless of their number of recommenders, their partisanship, professional qualifications, tenure, the reason why their recommenders leave Congress, and the partisan affiliation of their senatorial replacements.

5.3. Robustness Checks. We conduct additional analyses to assess the robustness of the results presented in Table 1. First, we test whether the findings hold under a more fine-grained method of identifying recommenders. Instead of assuming that each judge was recommended by all home-state senators who shared the president's party affiliation at the time of nomination, we rely on explicit endorsements made during the judge's confirmation hearing and on the nominees' answers to the written questionnaires submitted to the Senate Judiciary Committee.[19] Because not all hearing transcripts and written questionnaires can be retrieved, we apply this stricter definition to about two thirds of the judges in our sample, while retaining the original measure for the remainder. Re-estimating Equation (1) using this alternative definition yields results that are virtually unchanged, as shown in Table A.2.

Second, we examine the sensitivity of our inference to an alternative clustering strat-

[19] Written questionnaires typically contain an item asking each appointee about the details of the selection process that lead to their nomination.

egy. While our baseline specification clusters standard errors at the judge level, we re-estimate the model clustering at the level of the recommending senator (or senator pair) — that is, the level that determines the (dynamic) evolution of treatment status in our context. As reported in Table A.3, this alternative approach does not affect the statistical significance of our estimates.

Third, we check the robustness of the results to different transformations of the outcome variables. As detailed in Section 4, all four outcomes derived from district court opinions are discrete counts. Our approach in Table 1 has been to keep these outcomes in levels and use Poisson regression to estimate Equation (1). In the Appendix, we show that the sign and significance of the estimates are unaffected when applying a log transformation (Table A.4) or an inverse hyperbolic sine transformation (Table A.5) and estimating Equation (1) via OLS.

In a similar vein, we test the robustness of the results in column (6) of Table 1 to different definitions and transformations of the number of pending cases. Namely, in Table A.6 we show that the findings are largely unchanged when we do not exclude cases in Multi-District Litigation (MDL; column 2) or we examine the effects on the log number of pending cases augmented by 1 via an OLS regression (column 3).[20] All three estimates remain positive, although the one in column (3) falls slightly short of statistical significance at conventional levels (p-value $\simeq 0.135$).

6. MECHANISMS

In this section, we investigate the channels linking the exit of recommending senators to the decline in judicial productivity documented in Section 5. We consider four potential mechanisms, which are *not* to be considered as mutually exclusive.

6.1. Effect of Senator's Exit on Judge's Promotion Probability We begin by studying whether the effects are driven by an erosion of career prospects following the exit of the last recommender. Congressional minutes and anecdotal evidence (Domnarski, 2009)

[20] Column 1 reproduces the effects using the baseline measure, which excludes cases in MDL.

document that senators play an active role in promoting district judges to appellate courts, implying that the chances of career advancement of a judge may depend heavily on whether their senatorial sponsors remain in office. If this is the case, the departure of recommending senators can reduce the incentives for continued effort by weakening judges' promotion prospects. We perform several empirical tests to systematically inquire into this potential channel. First, we directly test for the impact of recommenders' exit on the career prospects of judges, estimating Equation (1) using as the dependent variable an indicator for whether judge i is promoted to an appellate court in a given year.

The coefficients reported in Table 2 show that the exits of recommending senators significantly reduce the likelihood that a judge is promoted to a court of appeals. Given the rarity of promotions, our estimates imply that advancement to the appellate bench becomes nearly unattainable without the continued support of a senatorial sponsor. Notably, as shown in column (2), the effect is concentrated in years when the judge shares partisanship with the sitting president — periods in which the judge would be best positioned to benefit from their recommender's backing.

To complement these results, Figure 2 presents event-study estimates from Equation (2), where we also allow the dynamic effects of recommenders' exit to vary by the party of the incumbent president. The findings from the event study mirror the estimates in Table 2, indicating that the negative effects are concentrated in years when judges and presidents are politically aligned (blue estimates), while no meaningful effects are produced if the exit takes place during a presidency of the opposite party (red estimates). In all cases, we find no evidence of anticipatory effects before the senator's departure, consistent with the parallel trends assumption. The timing of the effect aligns with what we found for performance: the drop starts at the time of the exit, becomes significantly different from zero by the third year, and compounds over the following years.

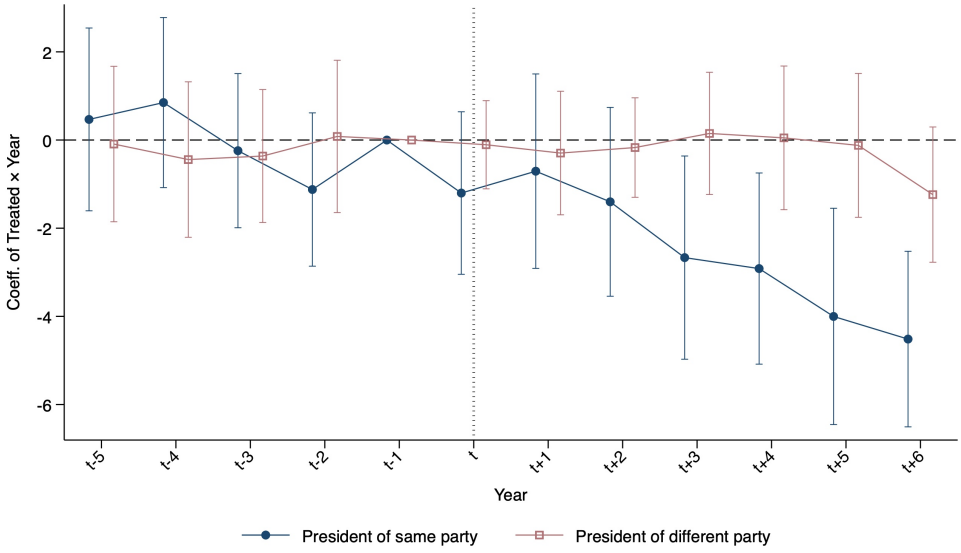
Figure A.9 shows that, similar to what we found for performance, the effect of senators' exit on judges' promotion do not depend on the number of recommenders at the time of appointment. The effects are similar for judges who remain without senatorial sponsors following the exit of their unique recommender (left panel) and judges who started with two recommenders and lose their last recommender (right panel). Again in

Table 2: Effect of Senator's Exit on Judge's Promotion

	Promoted (×100)	
	(1)	(2)
Treated × Post	-1.17***	-0.29
	(0.42)	(0.54)
Treated × Post × Same-party president		-1.68**
		(0.79)
Observations	42,389	42,389
Mean of dependent variable,		
Treated = 1 & Post = 0	0.51	0.51
Judge × Event FEs	Y	Y
Time × Event FEs	Y	Y

Notes: The unit of observation is judge × year × senator exit wave. The dependent variable is an indicator equal to 1 if judge i is promoted to an upper-level court in a given year (multiplied by 100). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge × event and year × event fixed effects. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 2: Effect of Senator's Exit on Judge's Promotion, Dynamic Effects



Notes: The figure reports estimates from Equation (2), interacted with an indicator for whether the incumbent president is of the same party as judge i (blue dots) or of a different party (red squares). The dependent variable is an indicator equal to 1 if judge i is promoted to an upper-level court in a given year (multiplied by 100). All regressions include judge × event and year × event fixed effects. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

line with what we found for performance, the negative effect on promotions applies to judges with different tenure spells at the beginning of our estimation window, but it is slightly more pronounced for judges with intermediate tenure (Figure A.10).

The evidence presented thus far is in line with the possibility that the drop in performance documented in Section 5 is — at least partially — motivated by an erosion of judges' career prospects following the exit of their recommenders. However, for this channel to be plausible, performance in office should be among the elements that senators take into account when deciding who to sponsor for promotion to appellate courts. We have already presented some anecdotal evidence in this regard in Section 2, documenting how senators often report about their appointees' judicial productivity during congressional hearings vetting nominees to U.S. courts of appeals.

Table A.7 complements these anecdotes with some correlational estimates of the effects of each of the six performance metrics used in this paper on a judge's probability of promotion. The measures that are significantly correlated with promotion are those summarizing the quality of a judge's output rather than the quantity. Conditional on having at least one recommender still in Congress, the probability that a judge is promoted at year t is positively correlated with the number of citations included in their opinions (column 3) and the number of citations received by their opinions (column 4), while it is negatively correlated with the share of their decisions reversed on appeal (column 6).

To further prove the link between the drop in performance and the drop in promotion probability following the exit of recommenders, we leverage two key reforms of the U.S. Federal Judiciary. These reforms — the Evarts Act of 1891 and the Judges' Bill of 1925 — dramatically changed the career prospects of district court judges, as shown descriptively by the time series plot in Figure A.1.

First, the Judiciary Act of 1891 — known as the "Evarts Act" from the name of its proponent — created the U.S. federal courts of appeals, opening up the possibility for district judges to be elevated to an appellate court. If career prospects are driving the observed effects of recommender's exit on judicial performance, these effects should materialize only after 1891, as promotion to an appellate court was not a potential career incentive prior to the Evarts Act. Figure A.11 confirms this intuition, showing how the decline in

the quantity and quality of judicial opinions is specific to the post-1891 period.[21]

Our second historical heterogeneity leverages the fact that promotions to appellate courts started to become much more desirable at a specific point during our sample period. The Judiciary Act of 1925 — commonly referred to as the "Judges' Bill" — substantially expanded the role of the federal courts of appeals within the judicial hierarchy. By granting appellate courts greater discretion over their dockets and shifting a large share of final judicial review away from the Supreme Court, the Act increased both the importance and the workload of circuit courts (Sternberg, 2008).

As a result, promotions from district courts to courts of appeals became more consequential. This should have strengthened the link between district court performance and opportunities for career advancement within the federal judiciary, which is key for the causal chain that we are considering. Figure A.12 presents strong evidence in this regard, showing how the effects of recommenders' exit on judicial opinions vary before and after 1925.[22] For all four outcomes, the negative effects of recommenders' exit are specific to years after 1925, when the Judges' Bill significantly raised the likelihood that strong performance in district courts could be rewarded by elevation to the appellate bench.

6.2. Effect of Senator's Exit on Judge's Senior Status In this subsection, we consider another potential channel through which the exit of senatorial recommenders may induce a drop in the performance of district court judges. Namely, we examine the effects of senators' exit on judges' own career choices.

Here, the most important outcome to consider is whether a judge decides to take senior status. Federal judges who meet age and tenure requirements may assume senior status, a form of partial retirement that allows them to remain on the bench while substantially reducing their workload (Carp et al., 2019). Senior judges continue to receive their full salary and may still hear cases, but are exempt from maintaining a full caseload and enjoy greater flexibility in selecting assignments. Consequently, transitions to se-

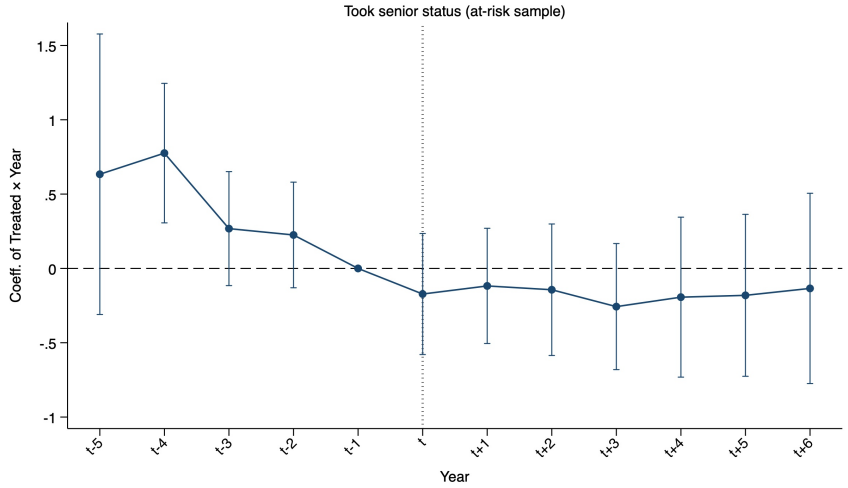
[21] We cannot conduct this heterogeneity analysis on pending cases and reversals because these outcomes are not available for the years prior to 1891.

[22] We cannot conduct this heterogeneity analysis on pending cases and reversals because these outcomes are not available for the years prior to 1925.

nior status are typically accompanied by a decline in the number of cases handled and in overall judicial output, which would be consistent with at least some of the effects documented in Section 5. However, as shown in the event study of Figure 3, we find no evidence that the exit of the last active recommender from Congress induces district court judges to take senior status.

Consistent with this, Table A.8 shows that the baseline effects of recommender's exit on judicial performance are largely preserved when excluding observations referred to judges who took senior status. Partial exceptions in this regard are for the number of opinions produced (column 1) and the share of decisions reversed on appeal, for which the effects slightly decrease and become marginally insignificant at conventional levels.

Figure 3: Effect of Senator's Exit on Judge's Senior Status, Dynamic Effects



Notes: The figure reports estimates from Equation (2). The dependent variable is an indicator equal to 1 if judge i took senior status in a given year (multiplied by 100). All regressions include judge $\times$ event and year $\times$ event fixed effects. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

6.3. Effect of Senator's Exit on the Allocation of Cases The third potential channel we investigate is the role of senatorial recommenders in affecting judges' caseload. Here, it is important to note that judicial rules should prevent any external actors — including recommending senators — from influencing the number and types of cases that judges are assigned to. Federal district courts generally assign newly filed cases among eligible judges according to local case-assignment rules that rely on randomization or rotation procedures. The purpose of these systems is to prevent judge-shopping and ensure that

cases are distributed impartially across judges within a district (Bird, 1974).

Because case assignment is governed by court rules rather than political actors, recommending senators should have little scope to influence the types of cases allocated to their appointees. However, formal assignment procedures do not necessarily preclude more subtle forms of political influence. If recommending senators help their appointees secure more desirable case portfolios or shield them from particularly demanding assignments, the departure of those senators could affect judicial productivity through changes in caseload composition rather than changes in effort.

To rule out this possibility, we proceed in three steps. First, we source from CourtListener all the available information on judges' caseload, which we divide between dockets and opinion clusters. A docket corresponds to a case before the court, whereas clusters of opinions are the written decisions issued within that case. Since not all cases terminate with the production of opinions, information on dockets and opinion clusters serve complementary but distinct purposes in the context of this paper. Namely, dockets give us information on the overall caseload of each judge and on the topic of the case. However, comprehensive docket-level coverage begins around the year 2000, when the Case Management/Electronic Case Filing platform — whose records are made publicly accessible through PACER (Public Access to Court Electronic Records) — was rolled out across federal district courts; pre-2000 dockets exist in the source but are sparse, so we restrict to filings from 2000 onward. In contrast, opinion clusters extend back further in time — the cluster panel includes filings as early as 1789 — but concentrate on the subset of cases that produced the baseline estimates of Table 1, i.e., those that terminated with the publication of one or more opinions. We exploit the opinion-cluster panel for systematic party-identity coding, and extract plaintiff and defendant information from the case-name string.

Next, we use large language models (LLMs) to extract information on: the topic(s) of dockets and clusters, the identity of the plaintiff(s) in each cluster, and the identity of the defendant(s) in each cluster. This procedure identifies eight categories of case types, four categories of plaintiffs, and four categories of defendants. We use these outcomes as dependent variables in Equation (1). We begin with the most basic feature of a judge's

caseload — the total number of cases assigned in a given year — and then turn to its composition: the share of cases of each type, and the identity of the plaintiffs and defendants. Case-type composition is measured both at the docket level (all filings, from 2000 onward) and at the opinion-cluster level (the subset of cases that produce an opinion).

Figure 4 reports the results. The exit of recommenders has no effect on the total number of cases a judge is assigned (top row of the upper panel). The volume of a judge's caseload does not change when a sponsor leaves — a first-order check on the allocation channel — and this also rules out that the larger backlog documented in Section 5 reflects a higher inflow of cases rather than slower resolution, reinforcing the random-assignment assumption that underlies our opinion-based measures. Composition is similarly unaffected: the exit of recommenders does not shift the share of a judge's caseload accounted for by any case type, whether measured across all filings (docket level, upper panel) or across cases that produce an opinion (opinion-cluster level, lower panel). The one exception is a positive coefficient on tort and product-liability filings at the docket level; across the roughly two dozen balance coefficients we estimate, however, one or two reaching significance is no more than chance would predict, and the corresponding cluster-level coefficient is null.

Figure 5 extends the analysis to the identity of the litigants. The departure of recommenders does not change the likelihood that a judge's cases involve individual, corporate, or government plaintiffs or defendants. Taken together, these results reject any meaningful role of senators in shaping the volume, composition, or parties of their appointees' caseloads, and reinforce that the effects of senators' exit on performance are not a byproduct of a change in the cases that judges handle.

6.4. Effect of Senator's Exit on Judge's Reputation The last potential mechanism we test concerns the visibility and recognition that judges enjoy among their colleagues. Recommending senators may help keep judges connected to professional and political networks within the federal judiciary, increasing the likelihood that their work is noticed and cited by other judges. In Table 1 and Figure 1, we have already documented that a judge's opinions become significantly less likely to be cited by their colleagues once the judge's

last recommender leaves Congress. However, this overall effect may subsume two different dynamics: (*i*) *Quality effects*, whereby judges write worse opinions after their recommenders leave Congress, which naturally receive fewer citations and (*ii*) *Recognition effects*, whereby other judges become less likely to cite a judge's opinions after her recommender leaves, regardless of the quality of such opinions.

To disentangle these two components and test for the existence of recognition effects, we exploit the fact that citations accumulate over time. Specifically, we decompose the effect of senatorial exits on the citations received by a judge's opinions by whether the cited opinion was written before or after the exit of the recommending senator. This distinction allows us to separate changes in the quality of newly produced opinions from changes in the recognition of existing work. If the departure of a recommender reduces only the quality of subsequent opinions, then citations should decline exclusively for opinions written after the senator exits. By contrast, if citations also decline for opinions written while the recommender was still in office, this would suggest that the judge's existing work becomes less visible or less likely to be recognized by colleagues once the political sponsor departs. The results of this exercise are plotted in Figure 6.

The decline in the likelihood of citation is pervasive, encompassing both recently-authored opinions (upper panels) and older opinions (bottom panels). Furthermore, the magnitude of coefficients is extremely similar across the different windows of time considered. This suggests that not only do recognition effects exist, but that they most likely trump quality effects, justifying most (if not all) the overall drop in citations received that we documented in Section 5. Therefore, a lack of recognition from colleagues constitutes a plausible additional channel that — together with an erosion of career prospects — links the exit of senatorial recommenders to the observed decline in the quantity and quality of judges' work.

To sum up, our analyses of mechanisms point to two main channels. The departure of recommending senators sharply reduces judges' prospects of promotion and lowers the visibility of their work within the judiciary, as reflected in subsequent citation patterns. In contrast, we find no evidence that the results are driven by a higher probability that

judges take senior status or experience changes in their caseload following the exit of their recommenders. Overall, the evidence suggests that political sponsors matter primarily because they shape judges' career opportunities and professional recognition.

7. Discussion and Conclusion

This paper studies how political appointments shape the performance of public officials throughout their career. We examine the U.S. federal judiciary, where district court judges are appointed through presidential nomination based on recommendations from home-state senators. Exploiting the staggered exit of recommending senators from Congress, we provide the first within-judge estimates of the consequences of losing a political sponsor for judicial performance.

Our findings reveal substantial declines in both the quantity and quality of judicial output following the departure of a judge's recommender. Judges author fewer opinions, produce shorter opinions that contain fewer citations, receive fewer citations from subsequent judicial decisions, accumulate larger case backlogs, and are more likely to have their rulings reversed on appeal. These effects emerge only after the senator leaves office and are robust across a wide range of specifications, subsamples, and measures of performance.

The evidence points to career concerns as the central mechanism. The departure of recommending senators significantly reduces judges' promotion prospects. These results suggest that political sponsors influence not only who enters public office, but also how officeholders perform once appointed. Even among life-tenured federal judges — arguably one of the most insulated groups of public servants in the United States — career incentives remain sufficiently powerful to shape day-to-day effort.

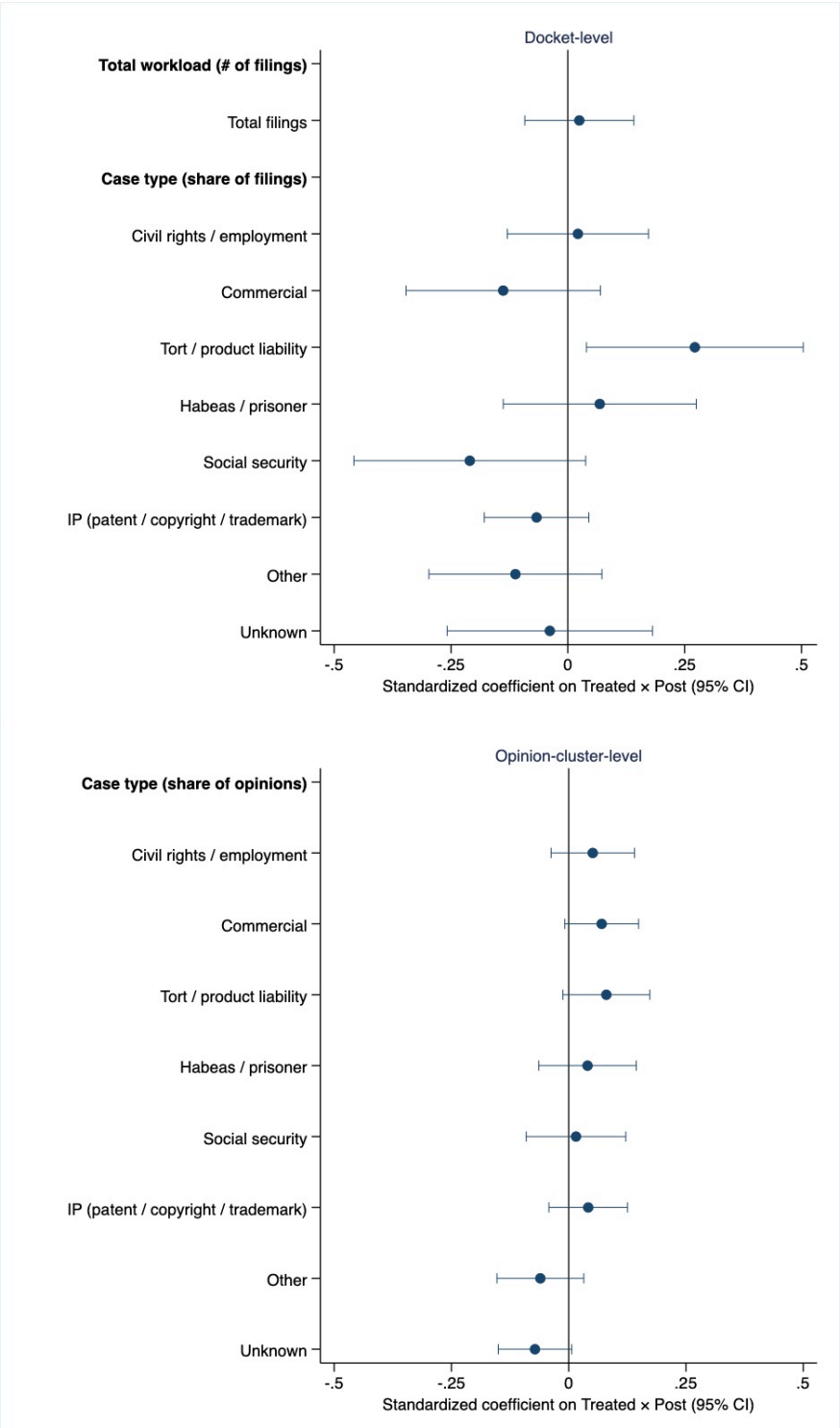
The magnitude of these effects is economically meaningful. After losing their recommender, judges accumulate approximately six additional civil cases that remain pending for more than three years, an increase of about 124%. This implies that, if extended to all the judges currently sitting in U.S. district courts, the effects of recommenders' exit may lead to the accumulation of around 4,000 additional long-delayed cases nationwide ev-

ery year. This roughly corresponds to the annual case-closing output of 16 district court judges at the end of our sample period, i.e., 2.4% of all district court judges in the United States. As reform proposals typically focus on expanding the number of judgeships to address delays, our findings suggest that the lack of incentives faced by some sitting judges may be an additional and previously overlooked determinant of court inefficiency.

These considerations point to a broader lesson for the design of public-sector institutions. Discussions of patronage systems often focus on their effects at the moment of selection, asking whether politically connected individuals are more or less qualified than alternative candidates. Our results highlight a different channel: Political connections create continuing incentive relationships that may persist for decades after appointment. As long as sponsors remain politically relevant, these relationships can sustain effort by preserving promotion opportunities and professional recognition. When those ties dissolve, performance may deteriorate even though formal employment protections remain unchanged.

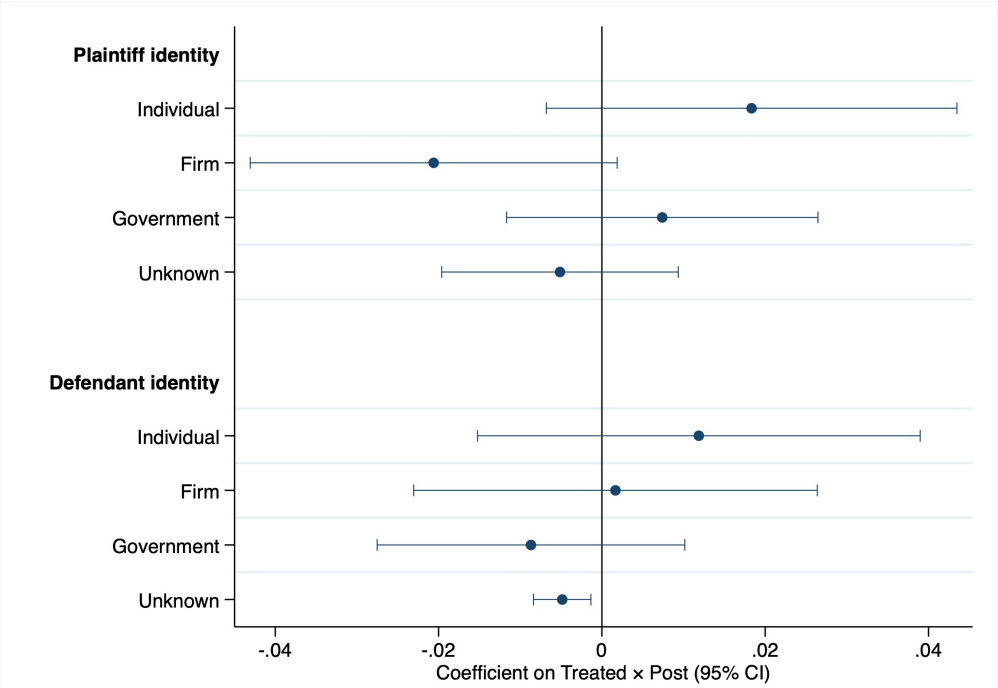
This insight has implications extending beyond the judiciary. Many bureaucracies around the world combine political discretion in appointments with internal promotion systems. In such environments, political sponsors may affect organizational performance not only through whom they select, but also through the incentives they create over the course of workers' careers. Understanding these dynamic effects is essential for evaluating the long-run consequences of political appointments and for designing institutions that balance political accountability, professional advancement, and organizational effectiveness.

Figure 4: Effect of Senator's Exit on Caseload Volume and Composition
Docket and Opinion-Cluster Levels



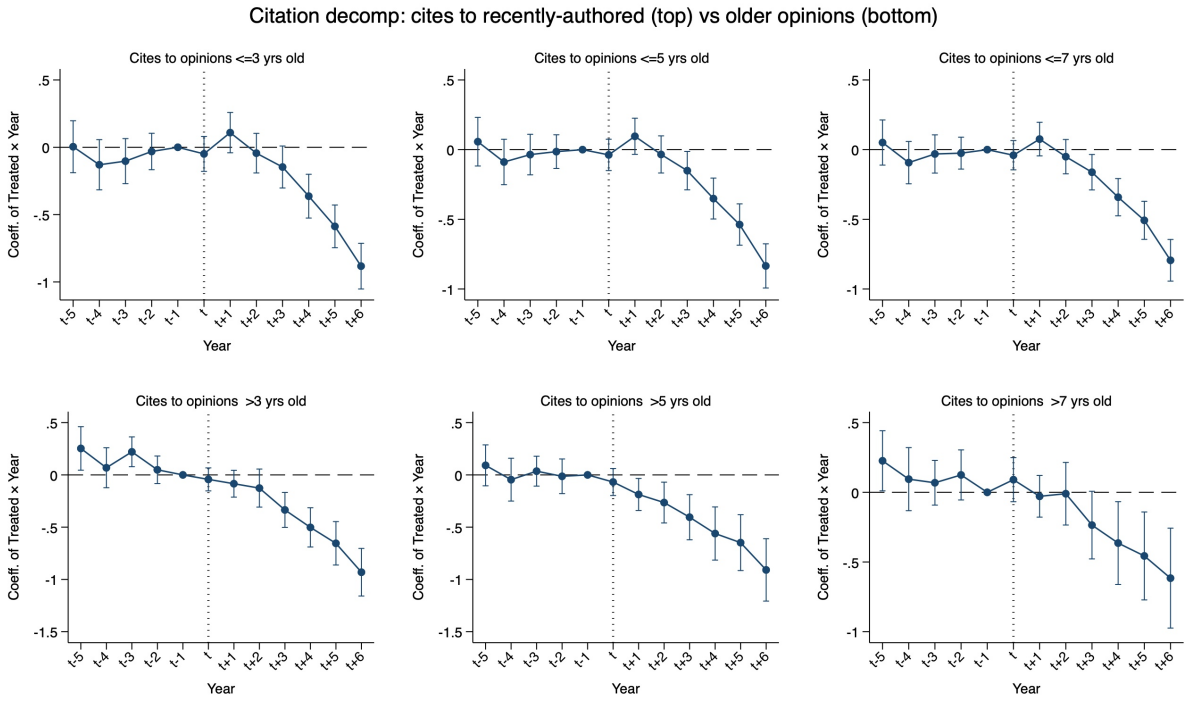
Notes: The figure reports estimates of the coefficient on *Treated* × *Post* from Equation (1). Outcomes are standardized by their standard deviation so that the total number of filings (a count) and the case-type shares (bounded between 0 and 1) can be shown on a common scale. The upper panel uses docket-level data (all cases assigned to a judge, 2000 onward): the first row is the total number of filings assigned to judge i in year t, and the remaining rows are the share of those filings accounted for by each case type. The lower panel uses opinion-cluster-level data (cases terminating in an opinion): each row is the share of judge i's opinions accounted for by each case type. All regressions include judge × event and year × event fixed effects. Horizontal lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure 5: Effect of Senator's Exit on the Identity of Litigants
Opinion-Cluster Level



Notes: The figure reports estimates of the coefficient on *Treated* × *Post* from Equation (1), run on opinion-cluster-level data. In the upper block, each dependent variable is an indicator equal to 1 if at least one plaintiff in the cluster is of the type indicated on the vertical axis, and 0 otherwise; in the lower block, the indicator is for the defendant's type. All regressions include judge × event and year × event fixed effects. Horizontal lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure 6: Effect of Senator's Exit on Judge's Reputation,
Dynamic Effects by Time of Opinion Authorship



Notes: The figure reports estimates from Equation (2). The dependent variable is the average number of citations received in each year by the opinions authored by judge i in different time periods relative to year t: during the last three years (upper left); during the last five years (upper middle); during the last seven years (upper right); more than three years before t (bottom left); more than five years before t (bottom middle); more than seven years before t (bottom right). All regressions include judge $\times$ event and year $\times$ event fixed effects. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

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ADDITIONAL TABLES AND FIGURES

Table A.1: Summary Statistics

	Mean	Stand. Dev.	Min	Max
Panel A. Judge-Year-Event Level				
Opinions Authored	8.33	12.65	0	424
Words in Opinions[a]	3,452	3,106	0	69,940
Citations Included	14.04	12.48	0	139
Citation Rate	0.35	0.39	0	8.60
Civil Cases Pending	9.48	20.12	0	784
Share Opinions Reversed	0.19	0.29	0	1
Tenure at Year t	6.75	5.81	1	46
Promoted at Year t (x 100)	0.74	8.58	0	100
Same-Party President	0.61	0.49	0	1
	Mean	Stand. Dev.	Min	Max
Panel B. Judge Level				
Recommenders at Appointment	1.47	0.50	1	2
Total Tenure	9.29	7.03	1	46
Promoted	0.06	0.24	0	1
Recommenders at Promotion	1.36	0.48	1	2
Tenure at Promotion	7.14	4.33	1	21
Party of Appointment				
Democratic	0.49	0.50	0	1
Republican	0.49	0.50	0	1

Notes: In Panel A, statistics are computed at the judge-year-event level, covering all observations included in our sample as described in Sections 3.3 and 4. In Panel B, statistics are computed at the judge level. [a] Excluding citations made.

Table A.2: Robustness: Alternative Measure of Recommenders

VARIABLES	Judicial Opinions					
	Total Authored	N. of Words	Citations Included	Citations Received	Share Reversed	Cases Pending
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post	-0.10*	-0.11***	-0.12***	-0.15***	0.04**	0.92***
	(0.06)	(0.03)	(0.03)	(0.04)	(0.02)	(0.30)
Observations	41,826	41,826	41,656	40,302	7,854	9,609
Mean of DV	7.54	3214.23	12.69	0.34	0.14	5.21
Judge x Event FE	Y	Y	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y	Y	Y
Estimator	Poisson	Poisson	Poisson	Poisson	OLS	Poisson

Notes: The unit of observation is judge $\times$ year $\times$ senator exit wave. The dependent variables are: the number of opinions authored by judge i in each year (column 1); the average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the average number of citations included in the opinions authored by judge i in each year (column 3); the average number of citations received in year t by the opinions authored by judge i up to t (column 4); the share of opinions that were reversed on appeal (sample is limited to years from 2000 to 2019, column 5); and the number of civil cases assigned to judge i that have been pending for more than three years as of year t (measurement starts in 1998, column 6). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge $\times$ event and year $\times$ event fixed effects. The mean of each dependent variable in the third row is calculated for judges that lost their recommender in wave j, for the years prior to the recommender's exit. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.3: Robustness: SEs Clustered by Recommending Senators

VARIABLES	Judicial Opinions					
	Total Authored	N. of Words	Citations Included	Citations Received	Share Reversed	Cases Pending
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post	-0.14**	-0.15***	-0.15***	-0.17***	0.05*	0.81**
	(0.07)	(0.04)	(0.04)	(0.06)	(0.03)	(0.33)
Observations	41,194	41,194	41,022	39,561	7,560	9,282
Mean of DV	7.58	3144.76	12.60	0.33	0.16	5.13
Judge x Event FE	Y	Y	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y	Y	Y
Estimator	Poisson	Poisson	Poisson	Poisson	OLS	Poisson

Notes: The unit of observation is judge $\times$ year $\times$ senator exit wave. The dependent variables are: the number of opinions authored by judge i in each year (column 1); the average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the average number of citations included in the opinions authored by judge i in each year (column 3); the average number of citations received in year t by the opinions authored by judge i up to t (column 4); the share of opinions that were reversed on appeal (sample is limited to years from 2000 to 2019, column 5); and the number of civil cases assigned to judge i that have been pending for more than three years as of year t (measurement starts in 1998, column 6). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge $\times$ event and year $\times$ event fixed effects. The mean of each dependent variable in the third row is calculated for judges that lost their recommender in wave j, for the years prior to the recommender's exit. Standard errors are clustered at the recommending senators level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4: Robustness: Log Transformation of Outcomes Based on Judicial Opinions

VARIABLES	Judicial Opinions			
	Total Authored (1)	N. of Words (2)	Citations Included (3)	Citations Received (4)
Treated x Post	-0.20***	-0.65***	-0.21***	-0.05***
	(0.04)	(0.13)	(0.05)	(0.01)
Observations	42,389	42,389	42,389	42,389
Mean of DV	1.62	6.64	2.05	0.25
Judge x Event FE	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y
Estimator	OLS	OLS	OLS	OLS

Notes: The unit of observation is judge $\times$ year $\times$ senator exit wave. The dependent variables are: the log number of opinions authored by judge i in each year (column 1); the log average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the log average number of citations included in the opinions authored by judge i in each year (column 3); the log average number of citations received in year t by the opinions authored by judge i up to t (column 4). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge $\times$ event and year $\times$ event fixed effects. The mean of each dependent variable in the third row is calculated for judges that lost their recommender in wave j, for the years prior to the recommender's exit. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5: Robustness: Inverse Hyperbolic Sine Transformation of Outcomes Based on Judicial Opinions

VARIABLES	Judicial Opinions			
	Total Authored (1)	N. of Words (2)	Citations Included (3)	Citations Received (4)
Treated x Post	-0.24***	-0.70***	-0.25***	-0.06***
	(0.05)	(0.15)	(0.06)	(0.01)
Observations	42,389	42,389	42,389	42,389
Mean of DV	2.03	7.21	2.52	0.30
Judge x Event FE	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y
Estimator	OLS	OLS	OLS	OLS

Notes: The unit of observation is judge $\times$ year $\times$ senator exit wave. The dependent variables are: the inverse hyperbolic sine of the number of opinions authored by judge i in each year (column 1); the inverse hyperbolic sine of the average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the inverse hyperbolic sine of the average number of citations included in the opinions authored by judge i in each year (column 3); the inverse hyperbolic sine of the average number of citations received in year t by the opinions authored by judge i up to t (column 4). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge $\times$ event and year $\times$ event fixed effects. The mean of each dependent variable in the third row is calculated for judges that lost their recommender in wave j, for the years prior to the recommender's exit. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.6: Robustness: Alternative Transformations of Pending Cases

VARIABLES	No Cases in MDL (1)	Raw N. Cases (2)	Log N. Cases +1 (3)
Treated x Post	0.805**	1.730***	0.233
	(0.325)	(0.565)	(0.156)
Observations	9,282	9,580	9,924
Mean of DV	5.13	5.10	1.21
Judge x Event FE	Y	Y	Y
Time x Event FE	Y	Y	Y
Estimator	PPML	PPML	OLS

Notes: The unit of observation is judge $\times$ year $\times$ senator exit wave. The dependent variables are: the number of civil cases assigned to judge *i* that have been pending for more than three years as of year *t*, excluding Multi-District Litigations (column 1); the number of civil cases assigned to judge *i* that have been pending for more than three years as of year *t* (column 2); the log number of civil cases (augmented by 1) assigned to judge *i* that have been pending for more than three years as of year *t* (column 3). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge $\times$ event and year $\times$ event fixed effects. The mean of each dependent variable in the third row is calculated for judges that lost their recommender in wave *j*, for the years prior to the recommender's exit. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7: Correlation of Performance Measures and Promotion Probability

VARIABLES	Promoted					
	(1)	(2)	(3)	(4)	(5)	(6)
Opinions Authored	0.005					
	(0.112)					
N. of Words in Opinions		0.129				
		(0.106)				
Citations Included			0.258*			
			(0.137)			
Citations Received				0.281***		
				(0.108)		
Pending Cases					0.174	
					(0.193)	
Reversal Rate						-0.300**
						(0.145)
Observations	11,342	11,342	11,342	11,342	2,431	1,977
Year FE	Y	Y	Y	Y	Y	Y
Estimator	OLS	OLS	OLS	OLS	OLS	OLS

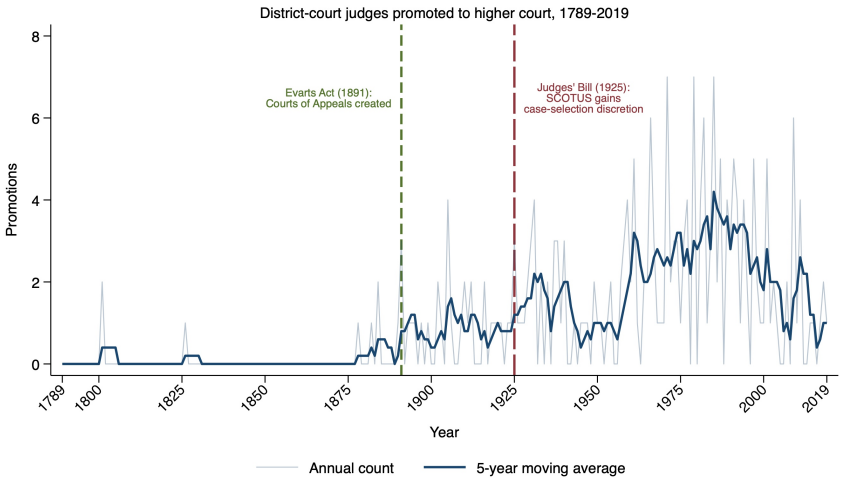
Notes: The unit of observation is judge $\times$ year. In all regressions, the dependent variable is an indicator equal to 1 if judge i is promoted at year t, (multiplied by 100). In each regression, the main explanatory variables are: the number of opinions authored by judge i in each year (column 1); the average number of words in the opinions authored by judge i in each year (column 2); the average number of citations included in the opinions authored by judge i in each year (column 3); the average number of citations received by the opinions authored by judge i in each year, normalized by the number of opinions authored up to that year (column 4); the number of civil cases assigned to judge i that have been pending for more than three years as of year t (column 5); and the share of opinions that were reversed on appeal (column 6). All regressions include year fixed effects. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.8: Robustness: Effect of Senator's Exit on Judge's Performance, Excluding Senior Judges

VARIABLES	Judicial Opinions					
	Total Authored (1)	N. of Words (2)	Citations Included (3)	Citations Received (4)	Share Reversed (5)	Cases Pending (6)
Treated x Post	-0.09	-0.12***	-0.12***	-0.15***	0.04	0.87**
	(0.06)	(0.03)	(0.03)	(0.05)	(0.03)	(0.37)
Observations	39,320	39,320	39,157	37,541	6,702	8,156
Judge x Event FE	Y	Y	Y	Y	Y	Y
Time x Event FE	Y	Y	Y	Y	Y	Y
Estimator	PPML	PPML	PPML	PPML	OLS	PPML
Sample period	1789–2019	1789–2019	1789–2019	1789–2019	2000–2019	1998–2019

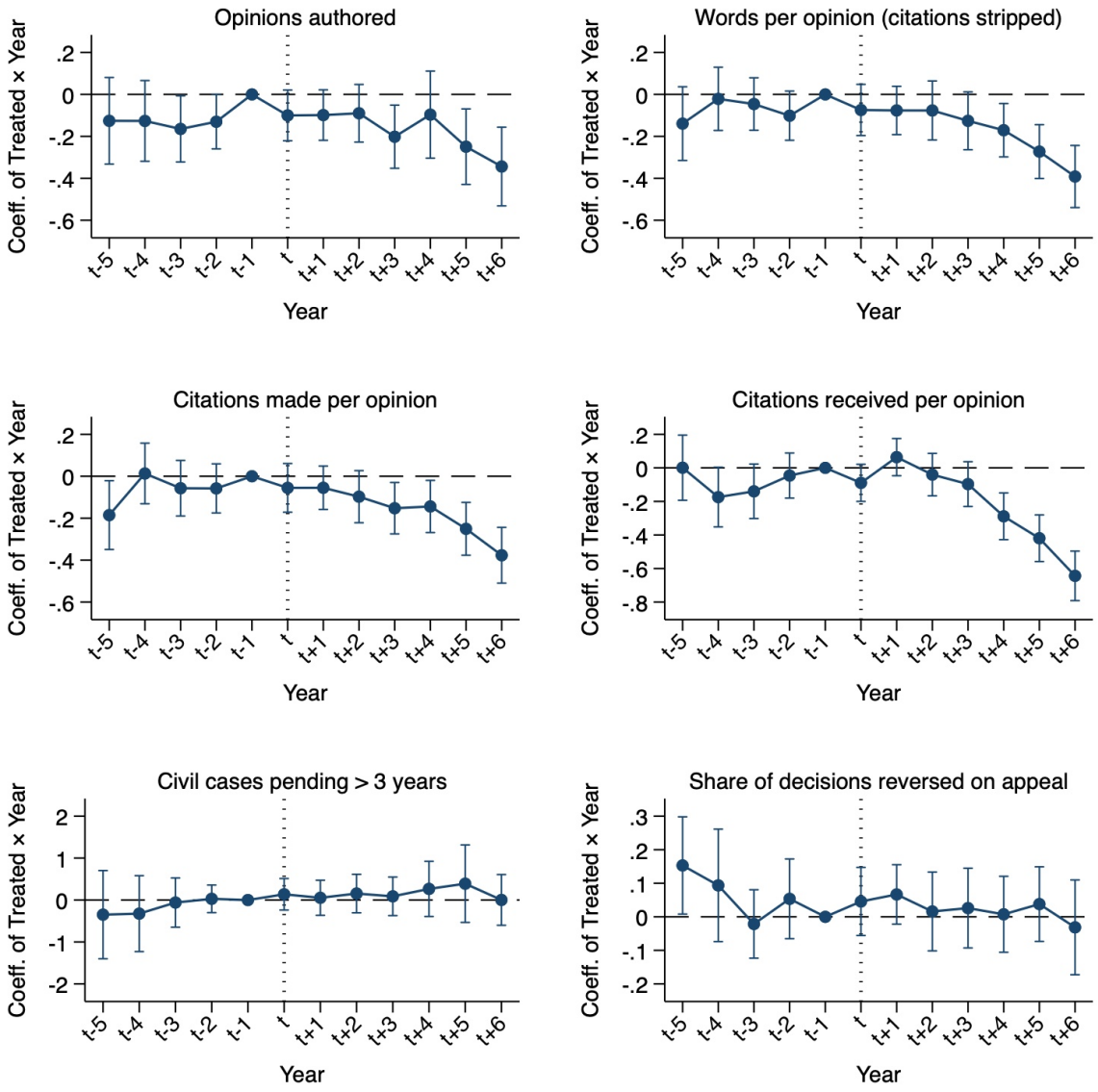
Notes: The unit of observation is judge × year × senator exit wave. The sample only includes judge-year observations for judges who have not yet taken senior status as of year t. The dependent variables are: the number of opinions authored by judge i in each year (column 1); the average number of words in the opinions authored by judge i in each year, excluding citations (column 2); the average number of citations included in the opinions authored by judge i in each year (column 3); the average number of citations received in year t by opinions authored by judge i, normalized by the number of opinions authored by judge i up to t (column 4); the share of judge's i decisions reversed on appeal (column 5); and the number of civil cases assigned to judge i that have been pending for more than three years as of year t (column 6). Treated is an indicator equal to 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is an indicator equal to 1 starting in the event year. All regressions include judge × event and year × event fixed effects. The mean of each dependent variable is calculated for judges that lost their recommender in wave j, for the years prior to the recommenders' exit. Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure A.1: Promotions of District Court Judges, 1789-2019



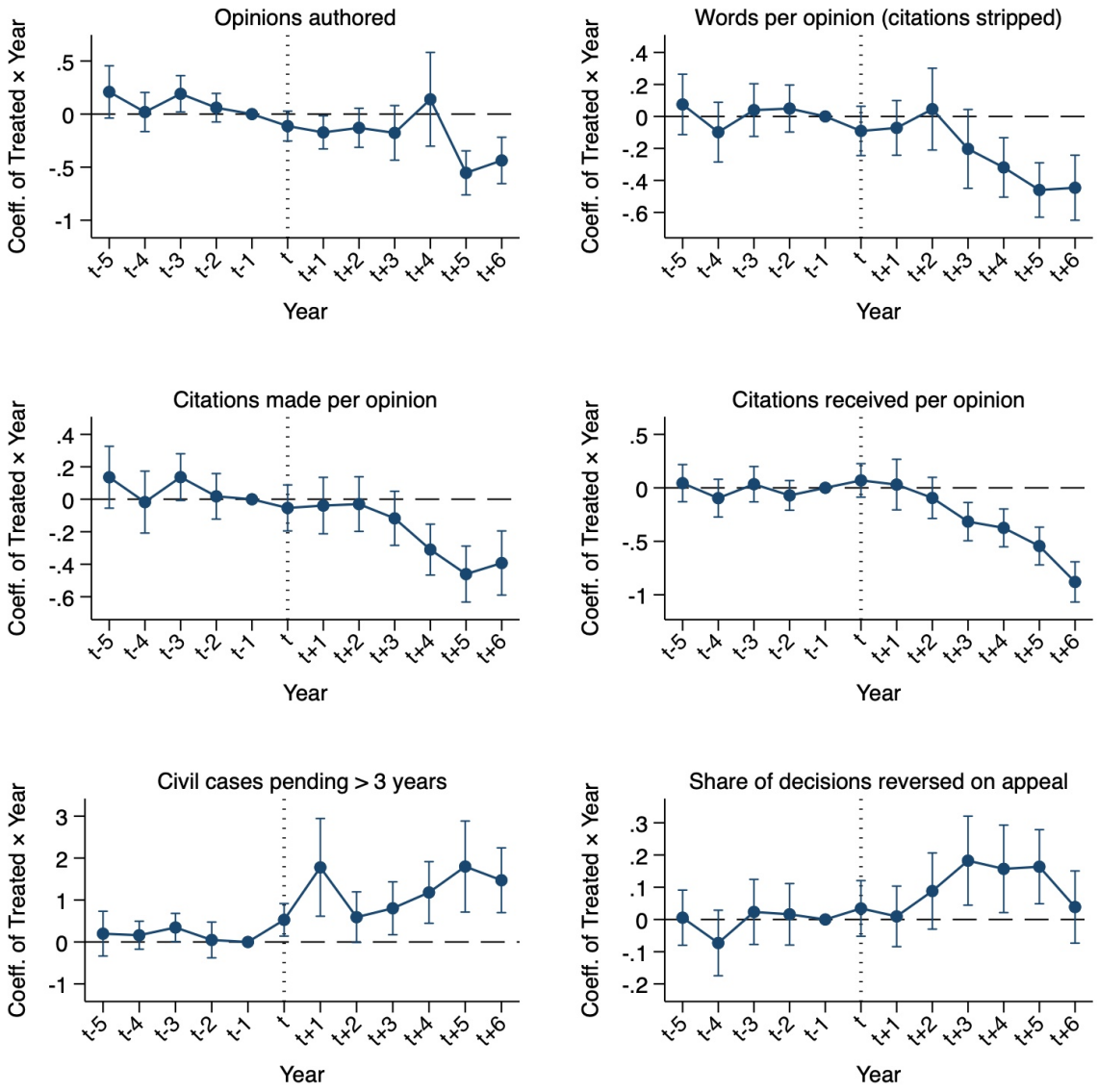
Notes: The figure reports the number of federal district court judges in the sample (as described in Section 3.3) who were promoted to an upper-level court between 1789 and 2019.

Figure A.2: Effect of Senator's Exit on Judicial Performance
Unique Recommenders' Exits Only



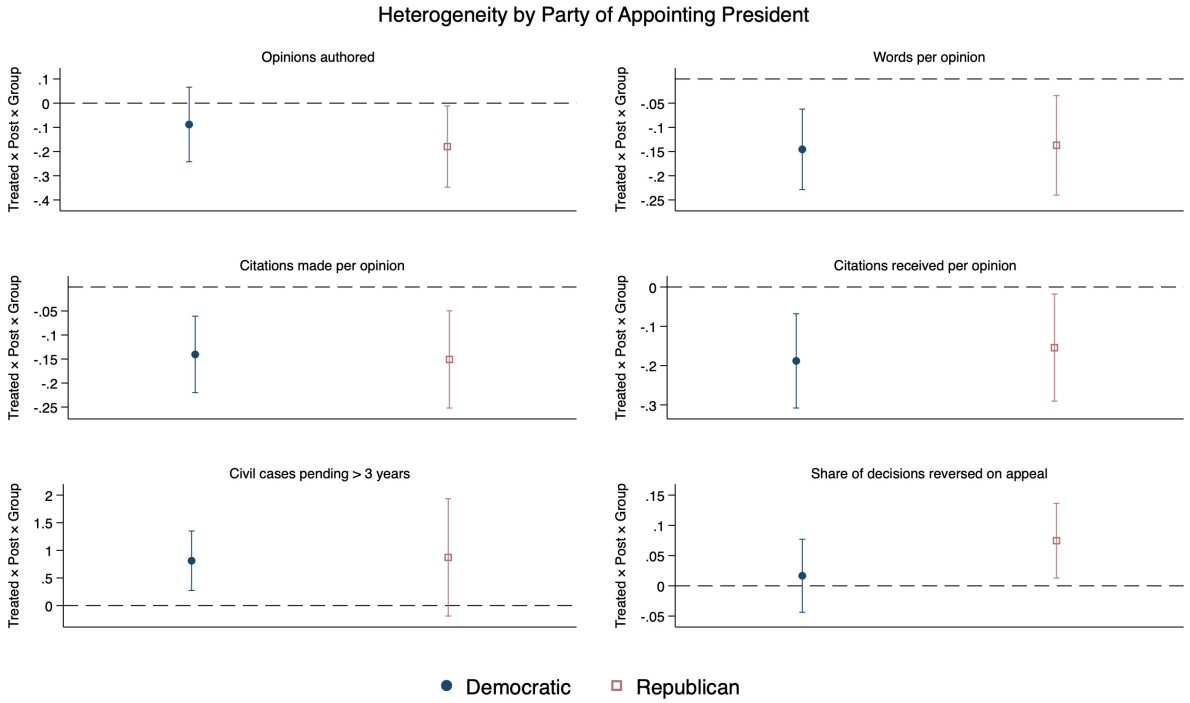
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations received by opinions authored by judge i in each year (middle left); the average number of citations included in the opinions authored by judge i in each year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left) and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All regressions include judge $\times$ event and year $\times$ event fixed effects and are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.3: Effect of Senator's Exit on Judicial Performance
Second Recommenders' Exits Only



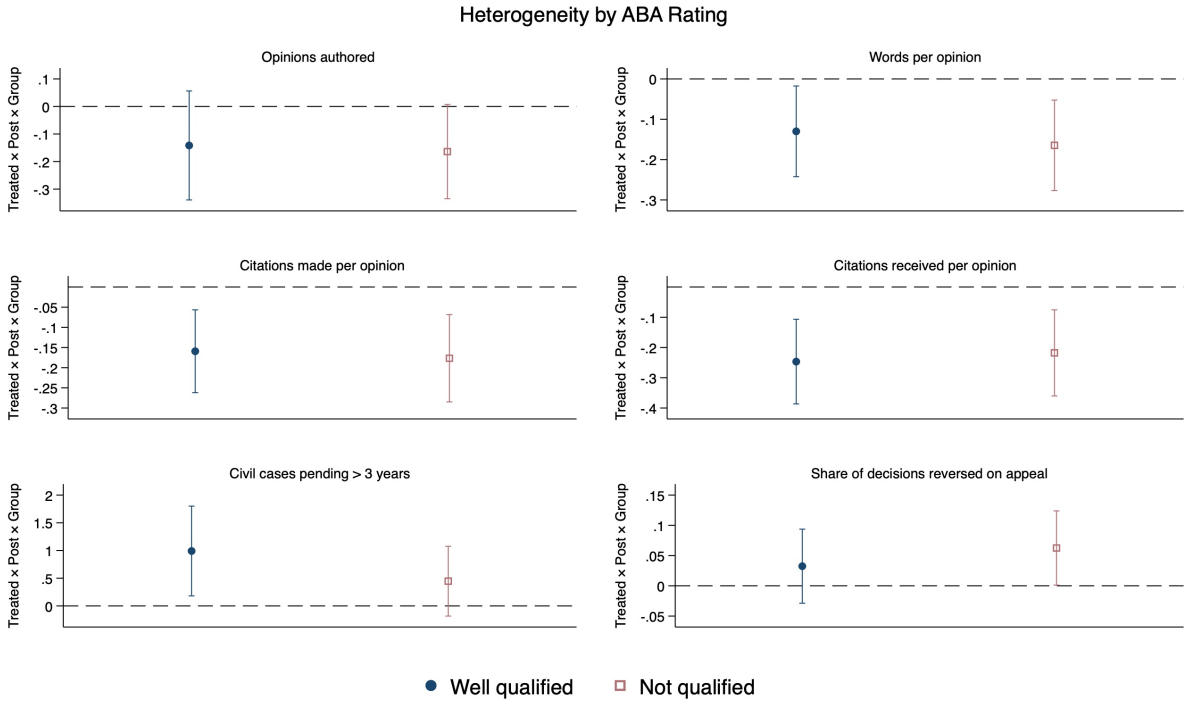
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations received by opinions authored by judge i in each year (middle left); the average number of citations included in the opinions authored by judge i in each year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left) and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All regressions include judge $\times$ event and year $\times$ event fixed effects and are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.4: Effect of Senator's Exit on Judge's Performance,
Heterogeneity by Party of Appointing President



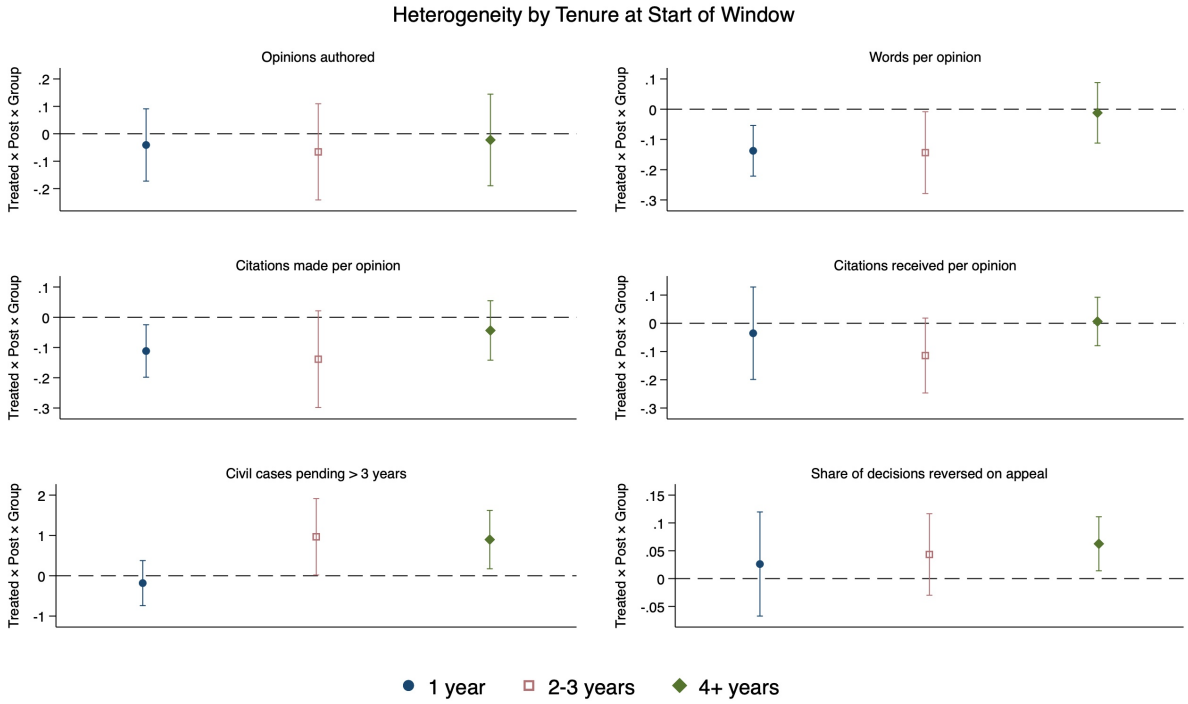
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether judge *i* was appointed by a Democratic (blue dot) or Republican president (red square). The dependent variables are: the number of opinions authored by judge *i* in each year (top left); the average number of words in the opinions authored by judge *i* in each year (top right); the average number of citations included in the opinions authored by judge *i* in each year (middle left); the average number of citations received in each year by the opinions authored by judge *i* up to that year (middle right); the number of civil cases assigned to judge *i* which at year *t* have been pending for more than three years (bottom left); and the share of opinions authored by judge *i* at year *t* that are reversed by appellate courts (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression, except those using the reversal rate as the outcome (bottom right), which are estimated via OLS. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.5: Effect of Senator's Exit on Judge's Performance,
Heterogeneity by Judge's Qualification at Time of Appointment



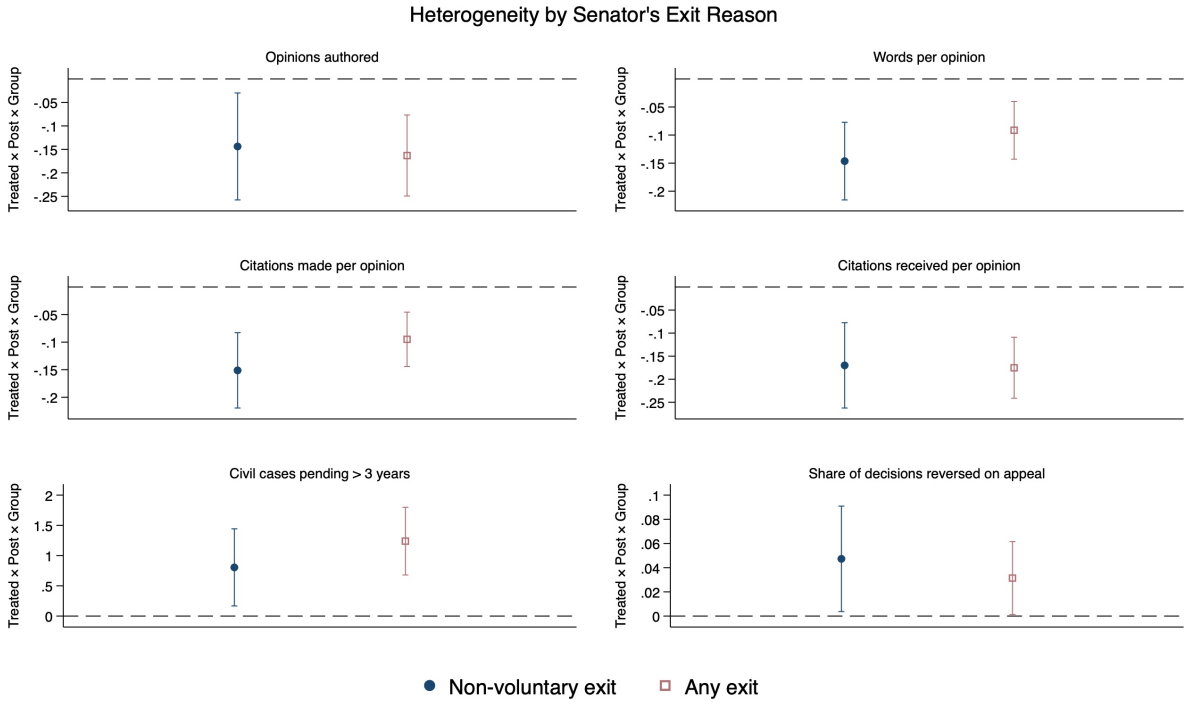
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether judge *i* received a rating of Well qualified or Exceptionally well qualified (blue dot) or Not qualified or qualified (red square) by the American Bar Association. The dependent variables are: the number of opinions authored by judge *i* in each year (top left); the average number of words in the opinions authored by judge *i* in each year (top right); the average number of citations included in the opinions authored by judge *i* in each year (middle left); the average number of citations received in each year by the opinions authored by judge *i* up to that year (middle right); the number of civil cases assigned to judge *i* which at year *t* have been pending for more than three years (bottom left); and the share of opinions authored by judge *i* at year *t* that are reversed by appellate courts (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression, except those using the reversal rate as the outcome (bottom right), which are estimated via OLS. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.6: Effect of Senator's Exit on Judge's Performance, Heterogeneity by Judge's Tenure at Start of Exit Wave



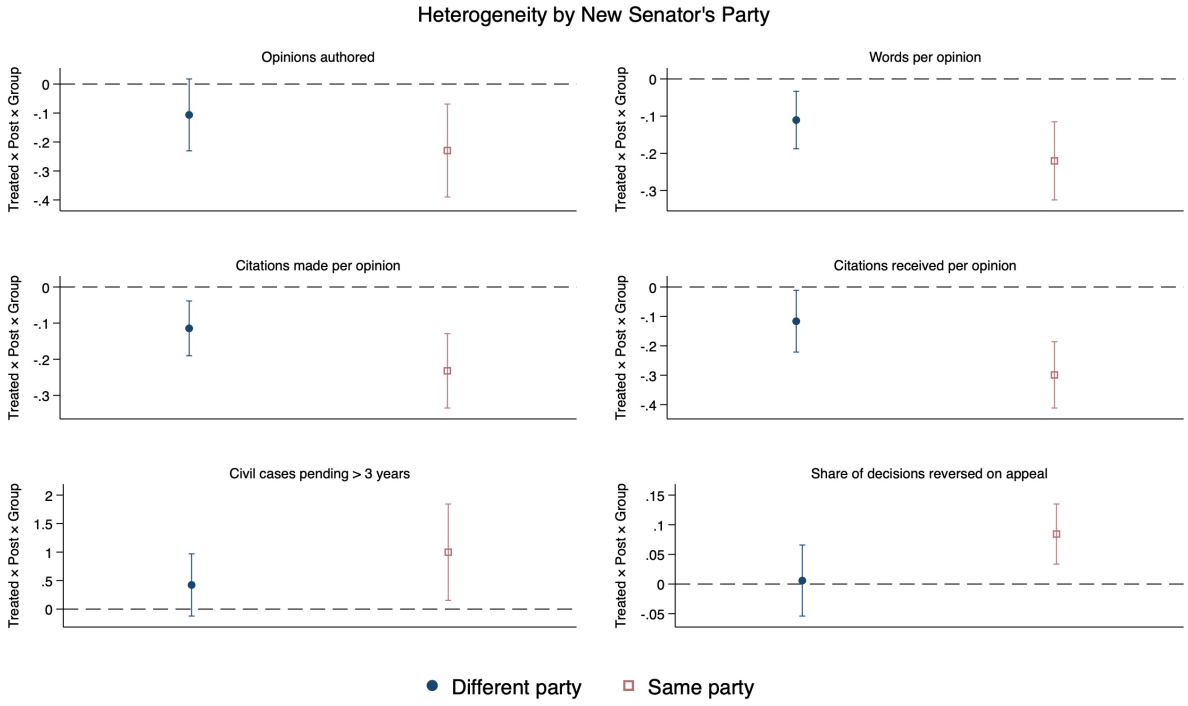
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether judge i, at the beginning of the event time window, had been in office for 1 year (blue dot), 2-3 years (red square), or at least 4 years (green diamond). The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (middle left); the average number of citations received in each year by the opinions authored by judge i up to that year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left); and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression, except those using the reversal rate as the outcome (bottom right), which are estimated via OLS. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.7: Effect of Senator's Exit on Judge's Performance, Heterogeneity by Senator's Reason of Exit



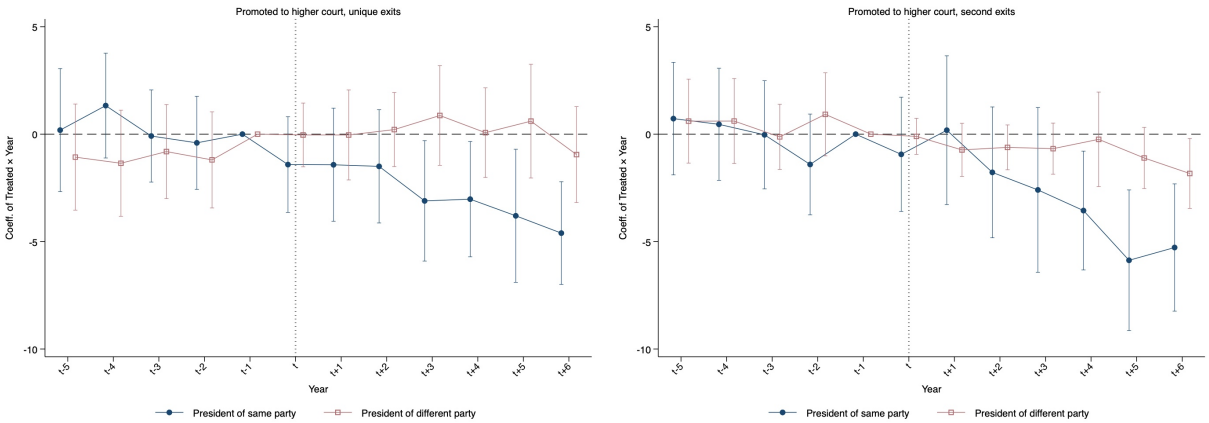
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* $\times$ *Post* (1) considering only senators' exits that occurred for non-voluntary reasons (i.e., loss at primary or general election, or death in office, blue dot) or considering all senators' exits (red square). The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (middle left); the average number of citations received in each year by the opinions authored by judge i up to that year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left); and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All regressions include judge $\times$ event and year $\times$ event fixed effects. All regressions are estimated via Poisson regression, except those using the reversal rate as the outcome (bottom right), which are estimated via OLS. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.8: Effect of Senator's Exit on Judge's Performance, Heterogeneity by Party of Replacing Senator



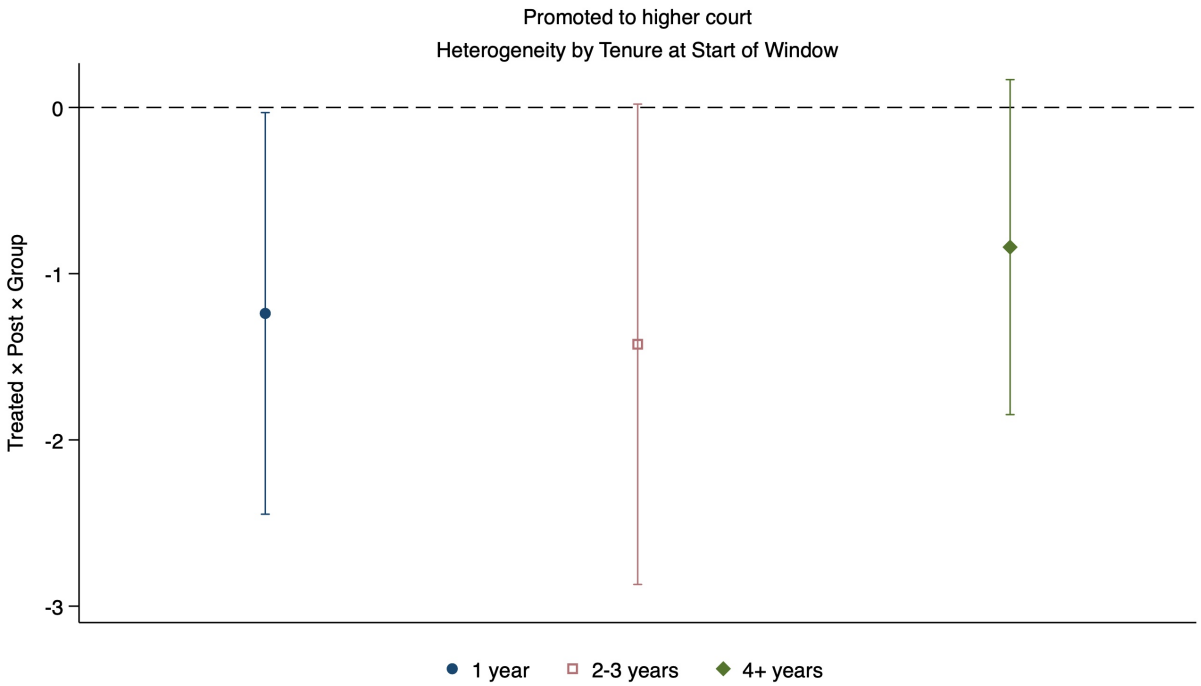
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether the senator replacing the one exiting at time t is of a different party (blue dot) or of the same party (red square). The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (middle left); the average number of citations received in each year by the opinions authored by judge i up to that year (middle right); the number of civil cases assigned to judge i which at year t have been pending for more than three years (bottom left); and the share of opinions authored by judge i at year t that are reversed by appellate courts (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression, except those using the reversal rate as the outcome (bottom right), which are estimated via OLS. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.9: Effect of Senator's Exit on Judge's Promotion, Dynamic Effects, Heterogeneity by Unique vs. Second Exit



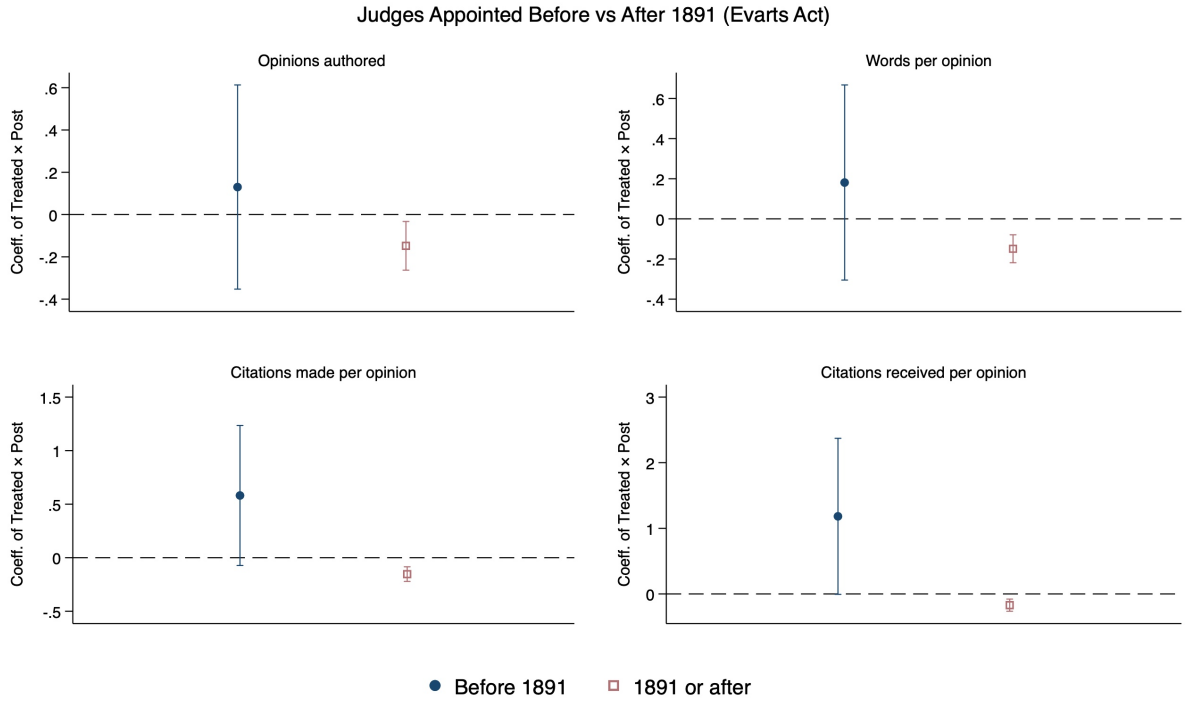
Notes: The figure reports estimates from Equation (2), interacted with an indicator for whether the incumbent president is of the same party as judge i (blue dots) or of a different party (red squares). In the left panel, we restrict the sample to judges appointed by a single recommender. In the right panel, we restrict the sample to judges appointed by two recommenders, and construct the *Treated* indicator based on the exit of the last active recommender. The dependent variable is an indicator equal to 1 if judge i is promoted to an upper-level court in a given year (multiplied by 100). All regressions include judge $\times$ event and year $\times$ event fixed effects. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.10: Effect of Senator's Exit on Judge's Promotion, Dynamic Effects, Heterogeneity by Judge's Tenure at Start of Exit Wave



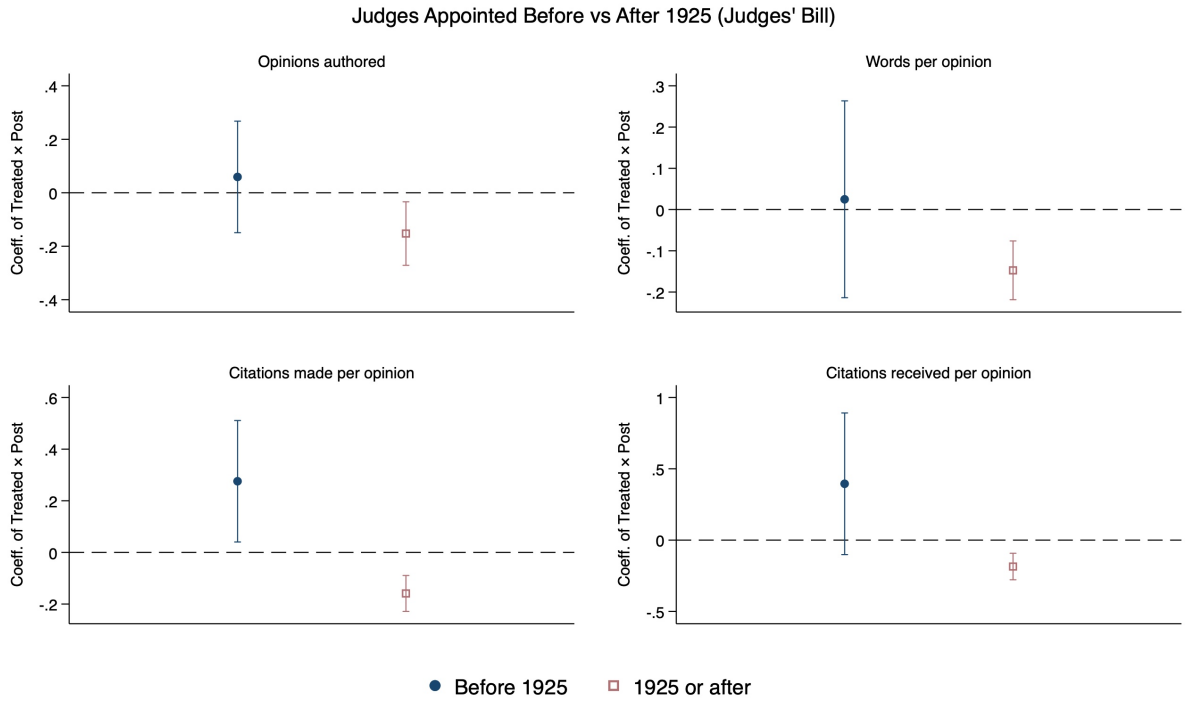
Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether judge *i*, at the beginning of the event time window, had been in office for 1 year (blue dot), 2-3 years (red square), or at least 4 years (green diamond). The dependent variable is an indicator equal to 1 if judge *i* is promoted to an upper-level court in a given year (multiplied by 100). All regressions include judge × event and year × event fixed effects. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.11: Effect of Senator's Exit on Judge's Performance, Heterogeneity before vs. after Evarts Act (1891)



Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether the judge was appointed before (blue dot) or after (red square) 1891, the year of enactment of the Evarts Act. The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (bottom left); the average number of citations received in each year by the opinions authored by judge i up to that year (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.

Figure A.12: Effect of Senator's Exit on Judge's Performance, Heterogeneity before vs. after Judges' Bill (1925)



Notes: The figure reports the estimates of the interaction between the main coefficient *Treated* × *Post* of equation (1) and an indicator for whether the judge was appointed before (blue dot) or after (red square) 1925, the year of enactment of the Judges' Bill. The dependent variables are: the number of opinions authored by judge i in each year (top left); the average number of words in the opinions authored by judge i in each year (top right); the average number of citations included in the opinions authored by judge i in each year (bottom left); the average number of citations received in each year by the opinions authored by judge i up to that year (bottom right). All regressions include judge × event and year × event fixed effects. All regressions are estimated via Poisson regression. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level.