POLITICAL CONNECTIONS, CAREERS, AND PERFORMANCE IN THE CIVIL SERVICE: EVIDENCE FROM U.S. FEDERAL JUDGES*

CARLO MEDICI† AND MASSIMO PULEJO‡

March 28, 2025

Click here for the latest version

INCOMPLETE. PLEASE DO NOT CIRCULATE WITHOUT PERMISSION.

ABSTRACT. This paper studies the role of political connections on the performance and career trajectories of civil servants. The focus is on U.S. federal judges, who are nominated by the president based on recommendations from their home-state senators. Leveraging individual-level data on judges and senators from 1789 to 2019, we employ difference-in-differences and event-study designs to compare judges' performance before and after their recommending senators leave office. Following their recommenders' exit from Congress, judges' performance decline. These negatives effects manifest in both quantity, as measured by fewer judicial opinions authored, and quality, indicated by shorter opinions and fewer citations made and received. The results are consistent with an erosion of career prospects driving these effects: after their recommenders leave office, district court judges become less likely to be promoted to upper-level courts. The findings highlight how political appointments can incentivize civil servants through career concerns but also show that these incentives are closely tied to the tenure of their political patrons.

---

*We thank Francesco Barilari, Leander Heldring, Antonio Leon, Matthew Notowidigdo, Nicola Persico, Jörg Spenkuch, Edoardo Teso, and seminar participants at Northwestern University, New York University, the University of Rome "Tor Vergata", the University of Naples "Federico II", Bocconi University, the 2024 "Brucchi Luchino" Labor Economics Workshop, and the 2025 Bolzano Applied Microeconomics Workshop for useful comments and suggestions.

†Brown University. carlo_medici@brown.edu.

‡Roma Tre University and CLEAN. massimo.pulejo@uniroma3.it.

1. INTRODUCTION

Political appointments are the primary method for selecting public workers worldwide (Lim and Snyder Jr, 2021). These appointments are crucial role for public servants' qualifications (Colonnelli et al., 2020), the provision of public goods (Akhtari et al., 2022; Aneja and Xu, 2024; Toral, 2024), corruption (Gagliarducci and Manacorda, 2020), fiscal capacity and financial performance (Xu, 2018; Vannutelli, 2022), and crisis management (Chen et al., 2022). Judicial appointments are no exception: by 2021, 70% of the world's nations filled court positions through presidential appointment (CIA, 2021). Despite this, there is little evidence on the consequences of politicians' influence in judicial nominations. This gap is particularly surprising, as the performance of courts shapes democratic quality (Montesquieu, 1991; Hamilton et al., 1998; Locke, 1976; La Porta et al., 2004) and economic prosperity (La Porta et al., 1997; Acemoglu and Johnson, 2005).

Recent studies have begun to address this gap by exploring how political appointments impact judicial performance. These studies compare the outcomes of elected versus appointed judges within the same country (Lim, 2013) or examine reforms that eliminated political appointments altogether (Mehmood, 2022). However, judges typically serve under the same appointment system throughout their careers, and their dependence on politicians changes as a result of individual rather than country-level shocks.

In this paper, we leverage the appointment process of U.S. federal judges to provide the first within-judge estimates of how political connections influence judicial performance. Federal judges in the United States are nominated based on recommendations from home state senators who share the same party affiliation as the president. Using individual-level data on judges and senators from 1789 to 2019, we link each judge to their recommending senators. We then use a stacked-by-event design (Cengiz et al., 2019; Deshpande and Li, 2019) to analyze how the departure of

recommending senators from Congress impacts judges' performance and career trajectories.

We find that district court judges produce 21% fewer judicial opinions after their recommending senators leave office. Furthermore, losing connections to recommending senators has a negative and significant impact on the quality of judicial opinions, as measured by length, number of citations included, and number of citations received. Event-study estimates confirm that judges' output begins to decline only after their recommenders leave the senate, supporting the parallel trends assumption underlying our identification strategy.

These negative effects are widespread. Heterogeneity analyses reveal that they hold across judges of different quality and partisan affiliations. The findings are robust to several identification and sensitivity checks. We also verify the robustness of our estimates to an alternative definition of political connections. To this end, we gather and read all the available minutes from the confirmation hearings of district court judges during our study period − 1,202 hearings, covering 56% of the connected judges in our sample. We then consider a judge and a senator as "connected" only if the senator publicly endorses the judge during the hearing. The results are robust − and larger in magnitude − when using this more fine-grained definition of connections.

To ensure that the negative effect on judicial productivity is driven by the loss of connections with home-state senators, we present two additional results. First, we document that the treatment effect remains negative and significant when a judge's recommender is replaced by a co-partisan. This implies that ties to specific senators − rather than parties − are what shapes judicial output. Second, we conduct a falsification test on judges who − at the time of their nomination − had no home-state senators from the president's party, and were thus appointed without senatorial recommendations. For these judges, there is no change in output once the senators in office at the time of their nomination leave Congress. In other words, senatorial turnovers only matter for judges' productivity if they sever connections between judges and senators.

Next, we explore the mechanisms linking the loss of senatorial connections to the observed decline in judges' productivity. Since senators are also actively involved in nominating upper-level judges (Domnarski, 2009), we posit that losing connections to senators may impair the career prospects of district court judges. Given that district court appointments are for life, longstanding economic theory (Rosen, 1986; Gibbons and Murphy, 1992) suggests that judges may have reduced incentives to maintain high effort once their recommenders leave office.

In line with this hypothesis, we find a strong negative impact of recommenders' exit on judges' career advancement. Specifically, judges experience a dramatic drop in their yearly probability of promotion to the U.S. courts of appeals after the exit of their recommenders, which essentially shuts the door to their advancement within the U.S. federal judiciary.

In line with the rules for federal judicial nominations, this impact emerges in years when judges share partisanship with the sitting president, and thus can benefit from the support of their senatorial connections. Like those on output, these effects apply irrespective of the reason for recommenders' departure, are homogeneous across judges of different quality, and do not significantly vary by partisan affiliation. This suggests that senatorial connections likely influence the career advancement of a large number of district court judges.

As with the findings on judicial output, we perform a battery of robustness tests on the results regarding promotions. The treatment effect holds when using the alternative definition of connections based on confirmation hearings. Additionally, the results are stable across different subsamples and approaches for clustering standard errors. Given that promotions of district court judges are rare, we also perform leave-one-out tests, which confirm that the estimates are not driven by any single state or administration.

This paper makes several contributions. First, it expands the growing literature on patronage. Recent studies (e.g., Gallo and Lewis, 2012; Xu, 2018; Colonnelli et al., 2020; Spenkuch et al.,

2023) document how patronage appointments erode the quality and performance of a wide range of organizations. Leveraging the institutional setting, we can study how the effects of patronage change dynamically throughout the course of civil servants' careers, as a result of the exit of their patrons from office. Our findings reveal that, while active, political connections may motivate civil servants through career incentives, affecting both the size and the quality of their output.

This connects the literature on patronage with studies on how promotion schemes affect incentives and performance (Ke et al., 2018; Voth and Xu, 2019; Bertrand et al., 2020). Since promotions are the key causal mechanism behind our main results, we also add to a vast scholarship at the intersection of political economy and organizational economics. This work has consistently shown how the lack of career incentives undermines the performance of public employees (Finan et al., 2017; Karachiwalla and Park, 2017; Bertrand et al., 2020; Kim, 2022; Nieddu and Pandolfi, 2022; Deserranno et al., 2024).

This literature has only tangentially studied promotion incentives in the judiciary, typically focusing on small and selected subsamples of judges (Schneider, 2005; Black and Owens, 2016). However, understanding how to motivate judges is a fundamental task, since judicial performance is essential to democratic quality (La Porta et al., 2004) and economic development (Acemoglu and Johnson, 2005). Leveraging our comprehensive data and empirical setting, we contribute to this endeavor by providing causal estimates for the universe of district court judges, over a period of more than two centuries, in one of the largest judiciaries in the world.

Third, our findings speak to the debate on the merits of alternative appointment procedures for high-level public officials (Huber and Ting, 2021). A host of studies has shown how the decisions of elected public officials may be driven by electoral concerns, and that judges are not immune from this source of bias (Huber and Gordon, 2004; Gordon and Huber, 2007; Berdejó and Yuchtman, 2013; Besley and Payne, 2013; Canes-Wrone et al., 2014; Lim et al., 2015). By documenting that

political considerations also influence the behavior of appointed judges, our study raises questions about whether lifetime nominations effectively address the challenges posed by electoral cycles.

Finally, this article augments our knowledge of the overall functioning of the U.S. Federal Judiciary, and the factors that shape judicial performance. Previous studies have largely focused on how federal judicial bias stems from judges' partisan affiliation (Sunstein et al., 2007; Cohen and Yang, 2019) or personal ideology (Schanzenbach and Tiller, 2008). Our study offers a different perspective by examining how personal connections to specific politicians affect judicial performance through their effects on career incentives. This approach advances our understanding of judicial behavior, highlighting how incentives may evolve dynamically over the course of a judge's career.

The remainder of the paper is organized as follows. Section 2 provides background information on the U.S. federal court system, with particular regard to the role of home state senators in the nomination process for district court judges. Section 3 details the sources and features of our data on federal judges and U.S. senators, as well as the procedure to match them. Section 4 presents the empirical strategy. Section 5 illustrates the main results on judicial performance, and Section 6 the ones on promotions − our hypotehsized causal mechanism. Finally, Section 7 concludes.

2. BACKGROUND

US Federal courts are in charge of dealing with both civil and criminal cases referred to the potential violation of one or more federal laws. The federal court system consists of three layers: 94 district courts, 13 courts of appeals (also referred to as circuit courts), and the US Supreme Court. Different from state-level judges, who are elected by citizens, federal judges are appointed for life by the President of the United States. However − while formally making the nominations − the

president is far from being the only one involved in the process. This is particularly true for the entry-level position in the US federal judiciary, the one of district court judge.

In fact, by a well-established custom, candidates for district court judgeships are put forward by home state senators who are from the same party as the president. Should there be no such senators, the president consults with other high-level officials from the state with whom he shares partisanship, such as House representatives (Rutkus, 2016). After vetting the candidate(s) identified by home state senators, the President refers one nominee to the Senate Judiciary Committee, which holds a confirmation hearing involving a question and answer session with the candidate.

Following the hearing, the committee reports the candidate to the Senate floor in one of three ways: favorably, unfavorably, or without recommendation. In the overwhelming majority of cases, candidates are reported favorably, and in a relatively quick way.[1] The Senate is then in charge of the final confirmation, which is usually voted by unanimous consent. On top of the US Senate, the only other institution having a say over proposed candidates is the *American Bar Association* (ABA, henceforth), which issues a non-binding evaluation before the nomination is passed on to the Judiciary committee.

Although not enshrined in the Constitution, the practice of accepting names for district judgeships from home state senators has been consistently applied throughout the years, by presidents from all parties. This led to the association of district court judges with their senatorial recommenders rather than with their nominating president. As effectively summarized by US Attorney General Robert F. Kennedy, "Basically it's senatorial appointment with the advice and consent of the president" (O'Brien, 1986, p. 40).

Such a practice has not been immune from criticisms, on the grounds that it may favor politi-

[1]However, longer confirmation times – and occasional rejections of candidates – have been taking place in more recent decades (see Binder and Maltzman, 2009).

cally connected candidates over more competent ones. As acknowledged by a US Senator himself, it constitutes an "important source of political patronage" for US senators (Tydings, 1977). Not surprisingly, factors concurring to the identification of candidates by senators include friendship, acquaintance, and family ties, among others (Domnarski, 2009). Furthermore, district judges are often chosen based on their political orientation, and a large majority of them were politically active before being appointed (Carp et al., 2019).

While home state senators are commonly regarded as determining only district court nominations, anecdotal evidence points to their active role in the appointment process of circuit court judges, as well (Domnarski, 2009). Notably, this qualitative evidence is largely corroborated by the official records of Congressional Hearings, which report strong written and oral endorsements of court of appeals nominees on behalf of one or more home state senators. This may imply that they suggest names for direct appointment to the circuit bench from outside the federal court system, or that they favor the promotion of judges that they first recommended for a district court position.

3. Data and Measurement

To study the impact of senators' tenure on the careers and performance of federal judges, we build a novel dataset combining information on both US federal judges and senators throughout the period 1789-2019.

3.1. US Federal Judges Data. Data on judges' 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 United States Government. The directory includes the biographies of judges appointed since 1789 on the US district courts, US courts of appeals, Supreme Court of the United States, and US Court of International Trade, as well as the former US

circuit courts, Court of Claims, US Customs Court, and US Court of Customs and Patent Appeals. The FJC data contain information on the full career of federal judges, with the specific dates of each appointment obtained.

Data on judicial opinions come from CourtListener, a free legal research website containing millions of legal opinions from federal and state courts, operated by the non-profit Free Law Project. At the moment we write, CourtListener contains information on 9,032,122 legal opinions from federal, state, and specialty courts, from the 1920s until today. Opinions – written comments that must be issued after deciding on each case – are the main output of judges' work, and have been consistently used by researchers as a tool to measure their performance (Posner, 2008; Ash and MacLeod, 2015, 2024).

For each judge-year observation, we compute four outcomes. The first gauges the *quantity* of output: the total number of judicial opinions written by a judge in a given year. Since an opinion must be issued after closing a case, the number of opinions that a judge produces corresponds to the number of cases they decided upon. As new cases are randomly assigned to judges on a rolling basis, the number of opinions exclusively depends on a judge's speed, not on the number and the type of cases they are assigned to.

The other three outcomes proxy for the *quality* of output: the average number of words contained in the opinions, the average number of forward citations and the average number of backward citations. Importantly, citations are not a measure of whether the decision is correct or not. But, on average, more forward citations signal a harder work by the judge to ground their decisions on previous cases, while more backward citations suggest that a judge's work proved more useful to her colleagues in the future.

3.2. US Senators Data. Data on senators are from three sources: the Biographical Directory of the United States Congress,[2] the website voteview.com,[3] and the Roster of Members of the United States Congress compiled by ICPSR.[4] Combining these sources provides us with complete information on the political careers of all US senators, from 1789 to 2019.

3.3. Matching of the Datasets. In the empirical analyses that follow we focus on the sample of federal judges who, over the 230 years of our sample period, were ever appointed as district court judges.[5] We follow their career in the district courts until their promotion, retirement, resignation, or death − whichever occurs first. In doing so, we also record if and when the senator(s) who recommended their nomination left office. To this end, we transform the FJC data into an unbalanced panel at the judge-year level.

In order to identify the senator(s) who recommended the nomination of each federal judge, we match this panel with the data on US senators. In particular, we link each judge to the senator − or pair of senators − who, at the time of her nomination as district court judge, were representing the state where she was appointed, and who were of the same party as the nominating president.[6] Finally, given that our treatment of interest is the end of the connection between the judge and her recommending senator, we exclude from our analysis judges who are appointed in states where there is no senator of the same party as the incumbent president at the time of nomination.

The final sample consists of 42,715 judge-year observations, covering 2,155 judges for the

[2]https://bioguideretro.congress.gov.

[3]https://voteview.com/data.

[4]https://www.icpsr.umich.edu/web/ICPSR/studies/7803.

[5]The following categories are not included in our sample: (i) judges appointed in years in which that State did not have any representative in the Senate yet; (ii) judges in the district courts of DC and Puerto Rico.

[6]The rationale for this matching procedure comes from the process through which senatorial recommendation of federal judges works, as detailed in Section 2 above.

period 1789-2019.[7] Table A1 reports summary statistics for a set of judges' characteristics. The average judge writes 4.91 opinions per year. These opinions have a mean length of 3,357 words and − on average − contain 11.95 citations of other judge's work and receive 3.78 citations by other judges. Approximately 11% of the judges in the sample get promoted from a district to an appellate court, after an average of 10 years from the first appointment. Figure A1 displays the number of promotions in each year, which ranges from a minimum of 0 to a maximum of 7. About half of the judges are appointed by a Democratic president, and half by a Republican one.

4. EMPIRICAL STRATEGY

The identification strategy leverages the staggered loss of political connections across judges. Since the treatment date differs for each judge based on the exit dates of their recommending senators, the empirical strategy compares the evolution over time of outcomes of judges who are treated earlier to judges who are treated later.

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 potential 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).[8]

The "stacked" method amounts to considering each senator exit wave as a separate subexperiment around which we construct a difference-in-differences estimate using judges affected and unaffected in that year. We then "stack" all individual event-specific difference-in-differences to

[7] Due to data limitations, the productivity outcomes are measured starting in 1924.

[8] In this context, one main concern is including already-treated judges as part of the control group. In the presence of heterogeneous treatment effects across judges experiencing senator exits at different points in time, this can lead to biased estimates.

estimate the pooled effect of senator exits across all waves.[9] As such, let j denote the senator exit wave, and let k be the years before or after the senator leaves Congress. Since k is centered around the date each senator exit occurs, negative values are years leading up to a senator exit event, and $k = 0$ denotes the year of the exit. We restrict the pooled sample to a 12-year window, i.e., six years with the senator in office and six years after they left office.[10] For judge i, senator exit year j, and the k-th year around the exit, we estimate

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

where $Treated_{ij} = 1$ if judge i loses their political connection in wave j, and 0 otherwise. The variable y_{ijk} is the outcome of interest. The variable $Post_k$ is defined as $Post_k = \mathbb{1}\{k \geq 0\}$, taking the value 1 post-exit, and 0 before. τ_{jk} are exit-specific year fixed effects, which absorb common temporal shocks. Since the same judge can serve both in the treatment and control groups in different exit waves, we estimate the judge fixed effects θ_{ij} separately for each exit wave.[11] The parameter β is the key estimate of interest, capturing the impact of experiencing the recommender's exit relative to control judges who do not change their connection status in event wave j. Standard errors are clustered at the judge level, accounting for the possibility of serial correlation over time and the repeated appearance of judges in different event waves as both treatment and control units.

For causal inference, we require that treated and control judges evolve along common trends in the absence of the senator exit. To investigate pre-trends as well as the dynamic evolution of the

[9] This research design makes explicit the comparison groups in each period. Specifically, the control groups are made of judges who have not experienced any senator exit by the end of the event window analyzed (i.e., judges "not-yet-treated").

[10] Six years corresponds to the term length of U.S. Senators. The findings are unaffected by alternative choices of the time window (results unreported for brevity but available upon request).

[11] Control judges who become treated within a given event study j's time window are excluded from that event study.

treatment effect, we also estimate a non-parametric event-study specification:

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

with the pre-exit year ($k = -1$) as the omitted category. In this specification, the coefficients of interest are the β_l's, measuring the change in outcomes of treated judges l years after treatment, relative to the the pre-treatment year, relative to the change in outcomes of control judges who have not been treated, yet.

As outlined in Section 2, judges with recommending senators may start their judicial career with either one or two active recommenders. Since we are chiefly interested in the effect of losing all ties to senators, throughout the paper we focus on the effects of the exit of the last active connection for each judge. This amounts to considering the exit of the unique recommender for judges who start with a single recommender, and of the recommender who leaves Congress last for those who start with two recommenders. Nonetheless, for this latter group, we also discuss the effects of each separate exit when presenting heterogeneity analyses.

5. Main Results: Connections and Performance

5.1. Effect of Connections on Performance. Table 1 shows estimates of β from Equation (1) for each of our four measures of judges' performance. Column (1) shows a clear drop in judges' output once their last active recommender leaves Congress: The loss of the connection causes a 21% reduction in the number of judicial opinions written.

As mentioned above, the amount of opinions that a judge issues on a given year only depends on how many cases they close on that year. Hence, the negative and significant coefficient of column (1) may be the result of two different dynamics. On the one hand, judges may take longer

to issue a verdict because they spend additional time and effort in crafting their decision, hence delivering a higher-quality work. Alternatively, it may simply be that they lower their efficiency, taking more time to complete cases while putting no additional quality into their work.

The coefficients in columns from (2) to (4) consistently support this latter hypothesis. Following the exit of the recommender, judges write opinions that − on average − are shorter (column 2), include fewer citations (column 3), and receive fewer citations (column 4). Taken together, these results indicate that losing the connection to their recommenders causes a decrease in judges' quantity and quality of output.

Table 1: Loss of Connection and Judicial Opinions

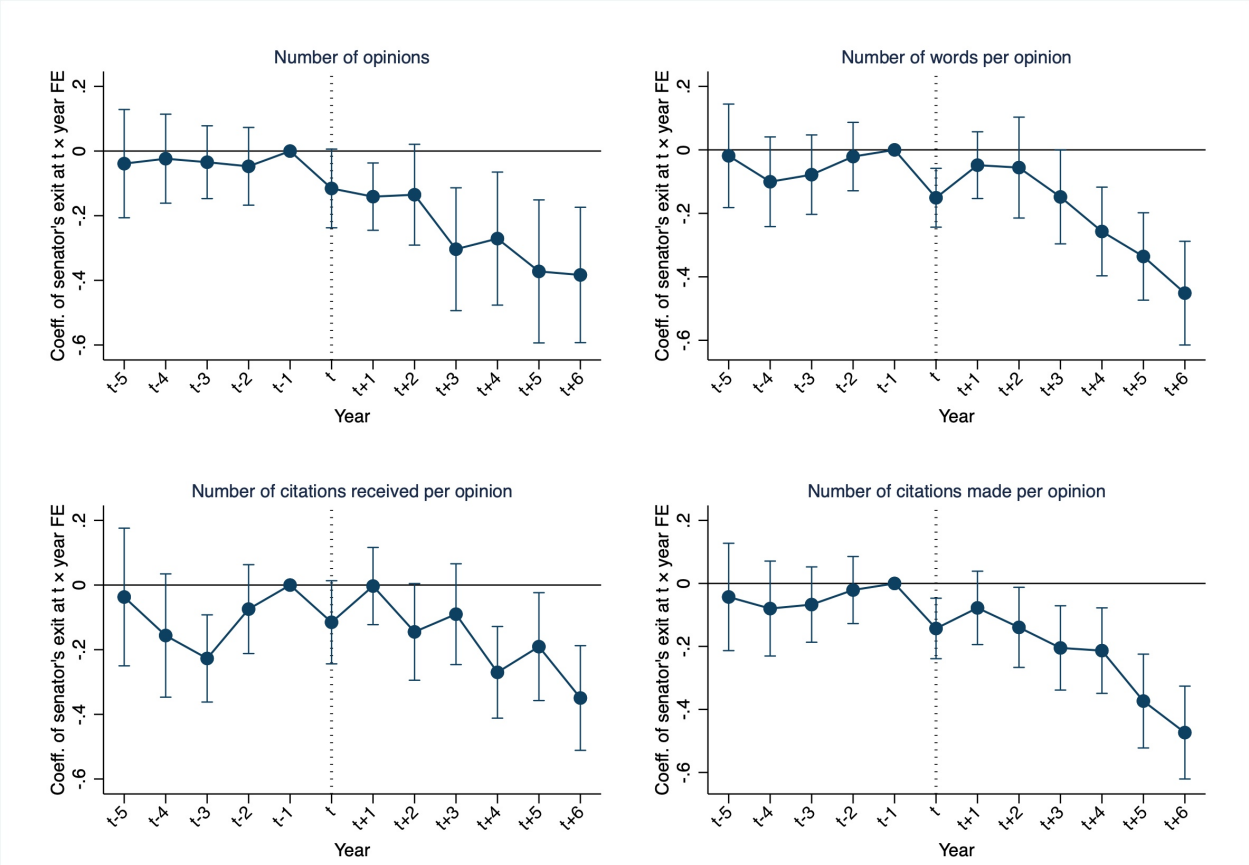
	Nr. of Opinions (1)	Length of Opinions (2)	Citations Made (3)	Citations Received (4)
Treated × Post	-0.21***	-0.14***	-0.07	-0.17***
	(0.07)	(0.05)	(0.05)	(0.05)
Observations	33,338	33,338	31,961	33,021
Mean of DV, Treated = 1 & Post = 0	4.85	3,401.45	4.96	11.80
Judge × Event FEs	Y	Y	Y	Y
Year × Event FEs	Y	Y	Y	Y

Notes: All coefficients are estimated using Poisson regressions. The unit of observation is the senator exit event × judge × year. The dependent variables in columns from (2) to (4) are averaged over the opinions issued by judge i in a given year. Treated is a dummy that is 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the exit year of interest. Standard errors clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1 plots estimates of the β_l's parameters from Equation (2), for $l = \{-5, +6\}$, with the pre-exit year ($l = -1$) as the omitted category. Each panel refers to one of our four measures of judges' performance. For all the outcomes, there is no evidence of pre-trends. All coefficients indicate a negative and statistically significant effect of connection loss on judges' performance.

The drop starts immediately after the senator's exit from Congress and persists for at least six years.

Figure 1: Effect of Losing Connection on Judges' Output



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 written by judge *i* in a given year (top left); the average number of words in the opinions written by judge *i* in a given year (top right); the average number of forward citations for the opinions written by judge *i* in a given year (bottom left); and the average number of backward citations for the opinions written by judge *i* in a given year (bottom right). All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

5.2. Heterogeneity Analyses. The results summarized above show that losing all senatorial connections can significantly reduce the judicial output of federal district court judges. But is this

effect specific to the exit of the last active recommender? Or does it apply also to judges who lose their first connection while still having an active recommender in Congress? This question is answered in Figures from A2 through A5, which provide two main insights.

First (Figure A2), the negative effect on productivity applies when considering all exits simultaneously, regardless of whether they refer to a judge with one or two recommenders and, for this latter group, to the exit of the first or the second recommender. Second, for judges who start with two recommenders, the drop in productivity is mostly driven by losing the second recommender (Figure A4), which leaves these judges without political connections to sitting senators.

Another relevant question is whether the effects of losing connections on productivity are homogeneous across judges with different characteristics. To answer this question, we introduce interaction terms into Equation (1) to test the heterogeneity of the results in Table 1 along three key dimensions.

We begin by testing the role of partisanship. Since the involvement of senators in the nomination process may differ widely across cases (Domnarski, 2009), it is possible that democratic and republican judges respond in different ways to the exit of their recommending senators. Yet, as shown in Figure A7, the drop in judicial productivity is essentially identical across judges nominated by democratic and republican presidents.

Second, we inquire the role of competence, to see whether the quality of a judge may mitigate the impact of political connections on their productivity. To do so, we match each judge to the evaluation they received by the American Bar Association (ABA) at the moment of their district court appointment, and divide judges into two groups. We consider as low-qualified judges those whose ABA rating is either "Not qualified" or "Qualified", and as high-qualified those who were rated as "Well qualified" or "Very well qualified". As displayed in Figure A8, the effect of losing connections on judicial output is negative and significant for both of these subsets, implying that

competence cannot limit the detrimental effects of losing political connections.

Finally, we study whether the effects differ depending on the reason for which a judge's recommenders left their seat. Specifically, we distinguish connections broken unexpectedly – i.e., because the senator died in office or lost an election – or due to the senator's retirement. Figure A9 plots the results of this exercise. Even though the magnitude of the effect is larger when the connection is lost unexpectedly, the coefficient is negative also for cases in which a senator retired, and the two effects are not statistically different from each other. Overall, the upshots of these heterogeneity analyses document that the negative effect of losing connections on productivity apply broadly across our sample of district court judges.

5.3. Robustness Checks. We now present the results of a battery of additional analyses, aimed at ensuring the robustness of the results presented in Table 1. First, we test whether the results hold under a different, more fine-grained way of defining connections between judges and senators. Rather than assuming that each judge was recommended by all the home-state senators who were co-partisans of the president at the time of nomination, we look for explicit endorsements by senators during a judge's confirmation hearing. As not all the minutes of confirmation hearings are publicly available, can do so for 56% of the connected judges in our sample, while we keep the original definition of connection for the others. Re-estimating (1) using this different measures leaves the results unchanged, as shown in Table A2.

Second, we test a different inferential scheme. Rather than clustering standard errors at the recommending senators level – i.e., at the level of treatment assignment – we cluster them at the judge level, in order to more systematically account for possible serial correlation. Table A3 documents that this alternative clustering choice does not change the significance of our estimates.

5.4. Alternative Explanations. Before moving to the analysis of causal mechanisms, we test two alternative explanations for the observed drop in judicial productivity. First, we test whether − rather than the connection to a specific senator − what really shapes judicial output is the link between a judge and the party that nominated them. To this end, we estimate differential effects by party of the replacing senator, and we plot them in Figure A10. The coefficients confirm that ties to specific senators, not to their party, are what drives the observed change in productivity.

Second, we take advantage of the fact that a relevant portion of the judges in our sample (846, or 28.2%) are nominated at times in which neither of their home-state senators are of the same party as the president. By constitutional rules, these judges are thus nominated without senatorial recommendations. This allows us to run a falsification test, whereby we check for whether the exit of the senators in office when these judges where appointed − who had no role in their nomination − has any effects on their productivity. The upshots of this placebo test − displayed in Table A4 − show that this is not the case, implying that senatorial turnovers only affect judicial output when they sever connections between judges and their recommenders.

6. MECHANISMS: CONNECTIONS AND PROMOTIONS

6.1. Effect of Connections on Promotions. In this section, we inquire whether − as per our hypothesis − the drop in judge's productivity after the loss of senatorial connections is due to an erosion of their career prospects. Specifically, we posit that − since senators may also actively sponsor judges for appellate courts − a district court judge may have higher chances of promotion while her initial senatorial recommenders are still in office. If this is the case, once their recommenders leave congress judges may lack the career incentives to keep working hard. To inquire whether this is the case, we estimate Equation (1) using as dependent variable an indicator for

whether judge *i* is promoted to an upper-level court at year *t*.

Consistent with our hypothesis, the coefficients in Table 2 show that losing senatorial connections does significantly erode a judge's promotion prospects. Since promotions are already quite rare, this implies that advancing to an appellate court becomes almost impossible without the support of senatorial recommenders. Notably, as shown in column (2) the effect plays out in years in which a judge shares partisanship with the sitting president, and would thus stand to exploit the efforts of their senatorial connection. To complement these results, Figure 2 plots event-study estimates from Equation (2).[12] Consistent with the parallel trends assumption, there is no evidence of anticipation effects.

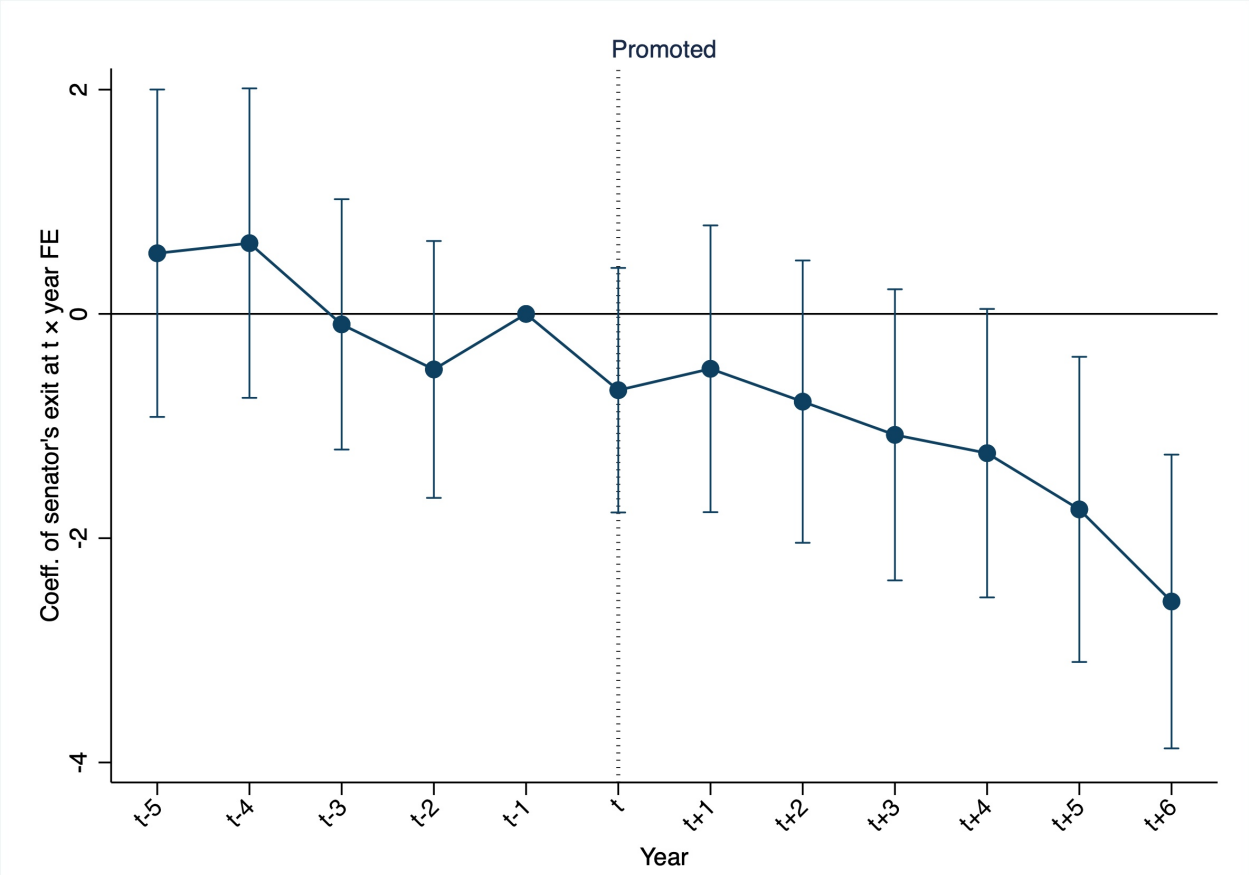
Table 2: Loss of Connection and Probability of Promotion

	Promoted	
	(1)	(2)
Treated × Post	-1.09**	-0.26
	(0.44)	(0.53)
Treated × Post × Same-party president		-1.63**
		(0.81)
Mean of DV, Treated = 1 & Post = 0	0.53	0.53
Observations	42,389	42,389
Judge × Event FEs	Y	Y
Time × Event FEs	Y	Y

Notes: The unit of observation is the senator exit event × judge × year. The dependent variable is an indicator for district judge *i* being promoted in a given year. Treated is a dummy that is 1 if the judge's recommender exits the Senate in the event wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the exit year of interest. Standard errors clustered at the judge level. Coefficients, standard errors and baseline means are multiplied by 100 to enhance readability. *** p<0.01, ** p<0.05, * p<0.1.

[12] Figure A6 plots the heterogeneous event-study estimates depending on the incumbent president's party. The findings confirm the results of column (2) of Table 2, indicating that the negative effects are driven by the years in which the judge and the president are politically aligned.

Figure 2: Effect of Losing Connection on Judges' Output



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 variable is an indicator for district judge i being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

6.2. Heterogeneity Analyses. As we did for those on productivity, we check whether the results on promotions hold for the exits of different recommenders. As displayed in Figures from A11 to A14, the drop in promotion applies when pooling all exits (Figure A11) and – for judges starting with two recommenders – it repeats with similar magnitudes after the exit of the first (Figure A12) and the second recommender (Figure A13).

Next, again following what we did for productivity, we inquire whether the effect on promo-

tions apply to judges of different partisan affiliation, different competence, and who lose their connection for different reasons. Once again − as shown in Figures from A15 to A17 − we do not find any significant heterogeneity along these dimensions. This implies that the drop in promotion consistently applies across a broad range of judges in our sample.

6.3. Robustness Checks. Like those on judicial output, the results on promotions are robust to the use of our alternative measure of connections based on confirmation hearings (Table A6), and to clustering standard errors at the judge level (Table A7).

Furthermore, given that promotions are a rare event, in Figure A18 we verify that our coefficient of interest is not determined by judges in a specific state (Panel A) or nominated by a specific president (Panel B). The only notable change in the coefficient's magnitude obtains when excluding the years of the Reagan administration (1981-1989). This is due to President Reagan's exceptional activism in promoting district court judges: of the 310 promotions in our sample, 33 (10.6 %) took place under his presidency, more than any other president in the history of the United States.

6.4. Alternative Explanations. Finally, to check that the effect on promotions is not due other reasons than the loss of senatorial connections, we run the same tests as we did in Section 5.4. The results we obtain are very reassuring. First, the effect holds regardless of the party of the replacing senator (Figure A19). Second, when running our placebo check exploiting judges without senatorial connections, we find no comparable drop in promotion probability (Table A8).

7. CONCLUSION

We provide the first within-judge estimates of the effects of political connections on judicial performance. Our setting is the US federal judiciary, where district court judges are appointed via

presidential nomination based on recommendations from home-state senators. Exploiting the exit of a judge's recommenders from Congress as a source of within-judge variation in connectedness, we show that losing ties to incumbent senators reduces both the quantity and the quality of judicial output, as proxied by the number of judicial opinions issued, their length, and the number of citations they contain and receive.

The key causal mechanism behind this effect is an erosion of career prospects. In fact, with an additional set of difference-in-differences estimates, we document that losing the tie to their recommending senator significantly reduces the yearly probability of promotion of district court judges. Consistent with the system of political appointments in place in the federal judiciary − and in many other federal agencies − such an effect emerges in years in which judges share partisan affiliation with the sitting president, and could thus benefit from their connection to a senator.

These findings are an important addition to extant work on institutions, political economy and organizational economics. Within the large empirical literature on patronage, our study is one of the few to examine the role of political patrons in determining both entry-level appointments and promotions. In this strand of work, our results complement those of Voth and Xu (2019), who study patronage promotions within the British Royal Navy and find that they can boost average performance if a majority of patrons prioritize merit over kinship. Here − thanks to our within-judge design − we show that patrons who favor their peers may not only fail to select good candidates, but also erode the performance of their own appointees over time.

Our study also contributes to a growing debate on the merits of different appointment systems in determining the efficiency and fairness of judicial systems across the globe. While scholars have extensively researched the biases that may arise from the direct election of judges, the role of political nominations has only recently started to be scrutinized. In this emerging empirical literature, our paper is the first to offer within-judge estimates of the effects of political connections

while holding constant the institutional setting in which judges operate. This approach holds great promise to further our understanding of the causes of judicial biases and inefficiencies, which constitute one of the main obstacles to political and economic development worldwide (Persson et al., 1997; Acemoglu and Johnson, 2005; Chemin, 2020).

REFERENCES

Acemoglu, D. and Johnson, S. (2005). Unbundling Institutions. *Journal of Political Economy*, 113(5):949–995.

Akhtari, M., Moreira, D., and Trucco, L. (2022). Political Turnover, Bureaucratic Turnover, and the Quality of Public Services. *American Economic Review*, 112(2):442–493.

Aneja, A. and Xu, G. (2024). Strengthening State Capacity: Civil Service Reform and Public Sector Performance during the Gilded Age. *American Economic Review*, 114(8):2352–2387.

Ash, E. and MacLeod, W. B. (2015). Intrinsic Motivation in Public Service: Theory and Evidence from State Supreme Courts. *The Journal of Law and Economics*, 58(4):863–913.

Ash, E. and MacLeod, W. B. (2024). Mandatory Retirement for Judges Improved the Performance of US State Supreme Courts. *American Economic Journal: Economic Policy*, 16(1):518–548.

Berdejó, C. and Yuchtman, N. (2013). Crime, Punishment, and Politics: An Analysis of Political Cycles in Criminal Sentencing. *Review of Economics and Statistics*, 95(3):741–756.

Bertrand, M., Burgess, R., Chawla, A., and Xu, G. (2020). The Glittering Prizes: Career Incentives and Bureaucrat Performance. *The Review of Economic Studies*, 87(2):626–655.

Besley, T. J. and Payne, A. A. (2013). Implementation of Anti-Discrimination Policy: Does Judicial Selection Matter? *American Law and Economics Review*, 15(1):212–251.

Binder, S. A. and Maltzman, F. (2009). *Advice and Dissent: The Struggle to Shape the Federal Judiciary*. Brookings Institution Press.

Black, R. C. and Owens, R. J. (2016). Courting the President: How Circuit Court Judges Alter Their Behavior for Promotion to the Supreme Court. *American Journal of Political Science*, 60(1):30–43.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting Event-Study Designs: Robust and Efficient Estimation. *Review of Economic Studies*, 91(6):3253–3285.

Canes-Wrone, B., Clark, T. S., and Kelly, J. P. (2014). Judicial Selection and Death Penalty Decisions. *American Political Science Review*, 108(1):23–39.

Carp, R. A., Manning, K. L., Holmes, L. M., and Stidham, R. (2019). *Judicial Process in America*. CQ Press.

Cengiz, D., Dube, A., Lindner, A., and Zipperer, B. (2019). The Effect of Minimum Wages on Low-Wage Jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454.

Chemin, M. (2020). Judicial Efficiency and Firm Productivity: Evidence from a World Database of Judicial Reforms. *Review of Economics and Statistics*, 102(1):49–64.

Chen, Q., Huang, Q., Liu, C., and Wang, P. (2022). Career incentives of local leaders and crisis response: A case study of COVID-19 lockdowns in China. *European Journal of Political Economy*, 75:102180.

CIA (2021). The World Factbook, 2021. Technical report, Central Intelligence Agency.

Cohen, A. and Yang, C. S. (2019). Judicial Politics and Sentencing Decisions. *American Economic Journal: Economic Policy*, 11(1):160–91.

Colonnelli, E., Teso, E., and Prem, M. (2020). Patronage and Selection in Public Sector Organizations. *American Economic Review*, 110(10):3071–3099.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–96.

Deserranno, E., Kastrau, P., and León-Ciliotta, G. (2024). Promotions and Productivity: The Role of Meritocracy and Pay Progression in the Public Sector. *Working Paper*.

Deshpande, M. and Li, Y. (2019). Who Is Screened Out? Application Costs and the Targeting of Disability Programs. *American Economic Journal: Economic Policy*, 11(4):213–248.

Domnarski, W. (2009). *Federal Judges Revealed*. Oxford University Press.

Finan, F., Olken, B. A., and Pande, R. (2017). The Personnel Economics of the Developing State. *Handbook of Economic Field Experiments*, 2:467–514.

Gagliarducci, S. and Manacorda, M. (2020). Politics in the Family: Nepotism and the Hiring Decisions of Italian Firms. *American Economic Journal: Applied Economics*, 12(2):67–95.

Gallo, N. and Lewis, D. E. (2012). The Consequences of Presidential Patronage for Federal Agency Performance. *Journal of Public Administration Research and Theory*, 22(2):219–243.

Gibbons, R. and Murphy, K. J. (1992). Optimal Incentive Contracts in the Presence of Career Concerns: Theory and Evidence. *Journal of Political Economy*, 100(3):468–505.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of econometrics*, 225(2):254–277.

Gordon, S. C. and Huber, G. (2007). The Effect of Electoral Competitiveness on Incumbent Behavior. *Quarterly Journal of Political Science*, 2(2):107–138.

Hamilton, A., Madison, J., and Jay, J. (1998). *The Essential Federalist: A New Reading of the Federalist Papers*. Number 3. Rowman & Littlefield.

Huber, G. A. and Gordon, S. C. (2004). Accountability and Coercion: Is Justice Blind When It Runs for Office? *American Journal of Political Science*, 48(2):247–263.

Huber, J. D. and Ting, M. M. (2021). Civil Service and Patronage in Bureaucracies. *The Journal of Politics*, 83(3):902–916.

Karachiwalla, N. and Park, A. (2017). Promotion incentives in the public sector: Evidence from Chinese schools. *Journal of Public Economics*, 146:109–128.

Ke, R., Li, J., and Powell, M. (2018). Managing Careers in Organizations. *Journal of Labor Economics*, 36(1):197–252.

Kim, T. (2022). Promotion Incentives, Career Decisions, and Police Performance. *Working Paper*.

La Porta, R., Lopez-de Silanes, F., Pop-Eleches, C., and Shleifer, A. (2004). Judicial Checks and Balances. *Journal of Political Economy*, 112(2):445–470.

La Porta, R., Lopez-de Silanes, F., Shleifer, A., and Vishny, R. W. (1997). Legal Determinants of External Finance. *The Journal of Finance*, 52(3):1131–1150.

Lim, C. S. and Snyder Jr, J. M. (2021). What Shapes the Quality and Behavior of Government Officials? Institutional Variation in Selection and Retention Methods. *Annual Review of Economics*, 13(1):87–109.

Lim, C. S., Snyder Jr, J. M., and Strömberg, D. (2015). The Judge, the Politician, and the Press: Newspaper Coverage and Criminal Sentencing across Electoral Systems. *American Economic Journal: Applied Economics*, 7(4):103–135.

Lim, C. S. H. (2013). Preferences and Incentives of Appointed and Elected Public Officials: Evidence from State Trial Court Judges. *American Economic Review*, 103(4):1360–1397.

Locke, J. (1976). The Second Treatise of Government, 3d ed., edited by j. w. gough.

Mehmood, S. (2022). The Impact of Presidential Appointment of Judges: Montesquieu or the Federalists? *American Economic Journal: Applied Economics*, 14(4):411–445.

Montesquieu, C. d. S. (1991). The Spirit of the Laws, 1748, ed. *JV Pritchard, trans. Thomas Nugent, Littleton, CO: Rothman & Co*.

Nieddu, M. and Pandolfi, L. (2022). The effectiveness of promotion incentives for public employees: evidence from Italian academia. *Economic Policy*, 37(112):697–748.

O'Brien, D. M. (1986). *Storm Center: The Supreme Court in American Politics*, volume 86. Norton New York.

Persson, T., Roland, G., and Tabellini, G. (1997). Separation of Powers and Political Accountability. *The Quarterly Journal of Economics*, 112(4):1163–1202.

Posner, R. A. (2008). *How Judges Think*. Harvard University Press.

Rosen, S. (1986). Prizes and Incentives in Elimination Tournaments. *American Economic Review*, 76:701–715.

Rutkus, D. S. (2016). *The Appointment Process for US Circuit and District Court Nominations*. Congressional Research Service.

Schanzenbach, M. M. and Tiller, E. H. (2008). Reviewing the Sentencing Guidelines: Judicial Politics, Empirical Evidence, and Reform. *The University of Chicago Law Review*, 75(2):715–760.

Schneider, M. R. (2005). Judicial Career Incentives and Court Performance: An Empirical Study of the German Labour Courts of Appeal. *European Journal of Law and Economics*, 20:127–144.

Spenkuch, J. L., Teso, E., and Xu, G. (2023). Ideology and Performance in Public Organizations. *Econometrica*, 91(4):1171–1203.

Sunstein, C. R., Schkade, D., Ellman, L. M., and Sawicki, A. (2007). *Are Judges Political?: An Empirical Analysis of the Federal Judiciary*. Brookings Institution Press.

Toral, G. (2024). How Patronage Delivers: Political Appointments, Bureaucratic Accountability, and Service Delivery in Brazil. *American Journal of Political Science*, 68(2):797–815.

Tydings, J. D. (1977). Merit Selection for District Judges. *Judicature*, 61:112.

Vannutelli, S. (2022). From Lapdogs to Watchdogs: Random Auditor Assignment and Municipal Fiscal Performance. Technical report, National Bureau of Economic Research.

Voth, H.-J. and Xu, G. (2019). Patronage for productivity: Selection and performance in the age of sail. *CEPR Discussion Paper No. DP13963*.

Xu, G. (2018). The Costs of Patronage: Evidence from The British Empire. *American Economic Review*, 108(11):3170–98.

APPENDIX

ADDITIONAL TABLES

Table A1: Summary Statistics - Regression Sample

	Mean	Stand. Dev.	Min	Max
Panel A. Cross-Sectional Variables				
Ever Promoted	0.106	0.308	0	1
Connections at Appointment	1.510	0.500	1	2
Connections at Promotion	1.467	0.500	1	2
Total Tenure	19.82	12.22	1	56
Tenure at Promotion	9.843	5.932	1	28
Party of Appointment				
Democratic	0.484	0.500	0	1
Republican	0.490	0.500	0	1
Federalist	0.012	0.107	0	1
Jeffers. Republican	0.011	0.103	0	1
Whig	0.003	0.057	0	1
	Mean	Stand. Dev.	Min	Max
Panel B. Time-Varying Variables				
Opinions Written	4.910	7.906	0	131
Words in Opinions	3,357	3,585	0	140,646
Forward Citations	11.95	11.99	0	163.3
Backward Citations	3.776	4.561	0	203
Promoted at Year t (x 100)	0.536	7.302	0	100
Same-Party President	0.530	0.499	0	1
Lost Connection (Unique)	0.631	0.483	0	1
Lost Connection (First)	0.710	0.454	0	1
Lost Connection (Second)	0.407	0.491	0	1
Tenure at Year t	14.18	10.20	1	56

Notes: Panel A only includes judges nominated to district court for a state in which there was at least one senator from the same party as the president at the time of nomination. In Panel B, statistics are computed for the 42,715 judge-year observations part of our sample, as described in Section 3.3. Statistics on opinions are for 32,729 judge-year observation covered by CourtListener.

Table A2: Loss of Connection and Judicial Opinions, Alternative Measure of Connection, Judges with One Recommender

	(1)	(2)	(3)	(4)
	Nr. of Opinions	Length of Opinions	Citations Made	Citations Received
ConnectionLost	-0.15***	-0.04	-0.06*	-0.01
	(0.03)	(0.03)	(0.03)	(0.04)
Observations	17,234	16,018	15,895	15,252
Judge FEs	Y	Y	Y	Y
State × Year FEs	Y	Y	Y	Y
Tenure FEs	Y	Y	Y	Y

Notes: All coefficients are estimated using Poisson regressions. The outcomes in columns from (2) to (4) are averaged over the opinions issued by judge i at year t. A judge is defined as "connected" to a senator if and only if the senator spoke to their favor during the confirmation hearing for their nomination. This sample includes only district court judges who had one connection at the time of appointment. Standard errors clustered by recommending senator(s)-year in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Loss of Connection and Judicial Opinions, Clustering SEs at Judge Level, Judges with One Recommender

	(1)	(2)	(3)	(4)
	Nr. of Opinions	Length of Opinions	Citations Made	Citations Received
ConnectionLost	-0.12***	-0.00	-0.03	0.01
	(0.06)	(0.04)	(0.04)	(0.04)
Observations	16,222	16,222	16,105	15,468
Judge FEs	Y	Y	Y	Y
State × Year FEs	Y	Y	Y	Y
Tenure FEs	Y	Y	Y	Y

Notes: All coefficients are estimated using Poisson regressions. The outcomes in columns from (2) to (4) are averaged over the opinions issued by judge i at year t. This sample includes only district court judges who had one connection at the time of appointment. Standard errors clustered by judge in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Loss of Connection and Judicial Opinions, Placebo on Judges without Recommending Senators

	(1)	(2)	(3)	(4)
	Nr. of Opinions	Length of Opinions	Citations Made	Citations Received
1stSenatorOut	-0.07	-0.13	-0.11	-0.14
	(0.14)	(0.12)	(0.12)	(0.20)
2ndSenatorOut	0.10	0.05	-0.10	-0.03
	(0.19)	(0.15)	(0.13)	(0.18)
Observations	6,591	6,591	6,501	6,195
Judge FEs	Y	Y	Y	Y
State $\times$ Year FEs	Y	Y	Y	Y
Tenure FEs	Y	Y	Y	Y

Notes: All coefficients are estimated using Poisson regressions. The outcomes in columns from (2) to (4) are averaged over the opinions issued by judge i at year t. This sample includes only district court judges who had no connections at the time of appointment. Standard errors clustered by recommending senator(s)-year in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Loss of Connection and Probability of Promotion, Judges with Two Recommenders, Effects of Each Exit

	(1)	(2)	(3)
1stConnectionLost	0.13	0.16	1.01
	(0.31)	(0.33)	(0.75)
1stConnectionLost ×	-0.65	-0.74	-1.37
Same-Party President	(0.42)	(0.47)	(0.98)
2ndConnectionLost	0.78**	0.78**	1.53**
	(0.31)	(0.33)	(0.64)
2ndConnectionLost ×	-0.28	-0.31	-0.70
Same-Party President	(0.28)	(0.32)	(0.61)
Same-Party President	1.24***	1.36***	1.53**
	(0.34	(0.38)	(0.64)
1stConnectionLost +	-0.52	-0.58	-0.36
1stConnectionLost × *Same-Party President*	(0.40)	(0.43)	(0.84)
2ndConnectionLost +	0.50	0.47	0.83
2ndConnectionLost × *Same-Party President*	(0.42)	(0.45)	(0.80)
Mean Probability of Promotion			
(ConnectionLost = 0)			
Same-Party President = 0	0.06	0.07	0.15
Same-Party President = 1	0.80	0.88	1.53
Years in Sample	All Years	Any Vacancy	Vacancy in CoA
Observations	17,659	16,296	6,644
Judge FEs	Y	Y	Y
State × Year FEs	Y	Y	Y
Tenure FEs	Y	Y	Y

Notes: In all models, the dependent variable is an indicator for district judge *i* being promoted at year *t*. Coefficients, standard errors and baseline means are multiplied by 100 to enhance readability. This sample includes only district court judges who had two connections at the time of appointment. Standard errors clustered by recommending senator(s) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Loss of Connection and Probability of Promotion, Alternative Measure of Connection, Judges with One Recommender

	(1)	(2)	(3)
ConnectionLost	0.15	0.16	0.33
	(0.25)	(0.26)	(0.51)
ConnectionLost ×	-0.84***	-0.87***	-1.51**
Same-Party President	(0.31)	(0.33)	(0.62)
Same-Party President	1.34***	1.41***	2.29***
	(0.28)	(0.30)	(0.56)
ConnectionLost +	-0.70***	-0.71***	-0.66***
ConnectionLost × *Same-Party President*	(0.27)	(0.28)	(0.23)
Mean Probability of Promotion *(ConnectionLost = 0)*			
Same-Party President = 0	0.15	0.16	0.35
Same-Party President = 1	1.10	1.18	1.83
Years in Sample	All Years	Any Vacancy	Vacancy in CoA
Observations	22,303	18,961	8,624
Judge FEs	Y	Y	Y
State × Year FEs	Y	Y	Y
Tenure FEs	Y	Y	Y

Notes: In all models, the dependent variable is an indicator for district judge *i* being promoted at year *t*. Coefficients, standard errors and baseline means are multiplied by 100 to enhance readability. A judge is defined as "connected" to a senator if and only if the senator spoke to their favor during the confirmation hearing for their nomination. This sample includes only district court judges who had one connection at the time of appointment. Standard errors clustered by recommending senator(s) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Loss of Connection and Probability of Promotion, Clustering SEs at Judge Level, Judges with One Recommender

	(1)	(2)	(3)
ConnectionLost	0.25	0.27	0.57
	(0.29)	(0.31)	(0.60)
ConnectionLost ×	-1.04***	-1.07***	-2.06***
Same-Party President	(0.39)	(0.41)	(0.75)
Same-Party President	1.48***	1.56***	2.60***
	(0.37)	(0.39)	(0.72)
ConnectionLost +	-0.79**	-0.80**	-1.49**
ConnectionLost × *Same-Party President*	(0.35)	(0.37)	(0.65)
Mean Probability of Promotion *(ConnectionLost = 0)*			
Same-Party President = 0	0.15	0.17	0.37
Same-Party President = 1	1.19	1.24	1.94
Years in Sample	All Years	Any Vacancy	Vacancy in CoA
Observations	20,395	18,943	8,720
Judge FEs	Y	Y	Y
State × Year FEs	Y	Y	Y
Tenure FEs	Y	Y	Y

Notes: In all models, the dependent variable is an indicator for district judge i being promoted at year t. Coefficients, standard errors and baseline means are multiplied by 100 to enhance readability. This sample includes only district court judges who had one connection at the time of appointment. Standard errors clustered by judge in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

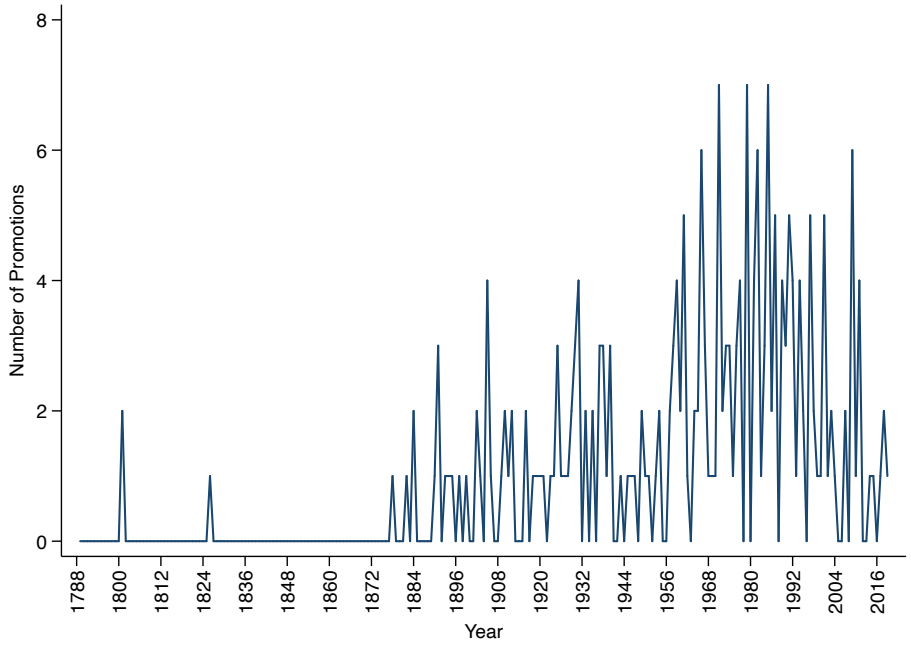
Table A8: Loss of Connection and Probability of Promotion, Placebo on Judges without Recommending Senators

	(1)	(2)	(3)
1stSenatorOut	-0.12	-0.15	0.75
	(0.53)	(0.58)	(1.36)
1stSenatorOut ×	-0.36	-0.33	-1.65
Same-Party President	(0.52)	(0.59)	(1.06)
2ndSenatorOut	0.11	0.17	0.33
	(0.37)	(0.41)	(0.97)
2ndSenatorOut ×	-0.18	-0.20	0.33
Same-Party President	(0.67)	(0.76)	(1.25)
Same-Party President	1.16**	1.23**	3.21***
	(0.56)	(0.60)	(1.17)
1stSenatorOut +	-0.48	-0.48	-0.90
1stSenatorOut × *Same-Party President*	(0.78)	(0.65)	(1.36)
2ndSenatorOut +	-0.06	-0.03	0.66
2ndSenatorOut × *Same-Party President*	(0.61)	(0.84)	(1.58)
Mean Probability of Promotion *(SenatorOut = 0)*			
Same-Party President = 0	0.07	0.10	0.25
Same-Party President = 1	0.72	0.81	1.31
Years in Sample	All Years	Any Vacancy	Vacancy in CoA
Observations	8,914	8,208	3,434
Judge FEs	Y	Y	Y
State × Year FEs	Y	Y	Y
Tenure FEs	Y	Y	Y

Notes: In all models, the dependent variable is an indicator for district judge *i* being promoted at year *t*. Coefficients, standard errors and baseline means are multiplied by 100 to enhance readability. This sample includes only district court judges who had no connections at the time of appointment. Standard errors clustered by recommending senator(s)-year in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

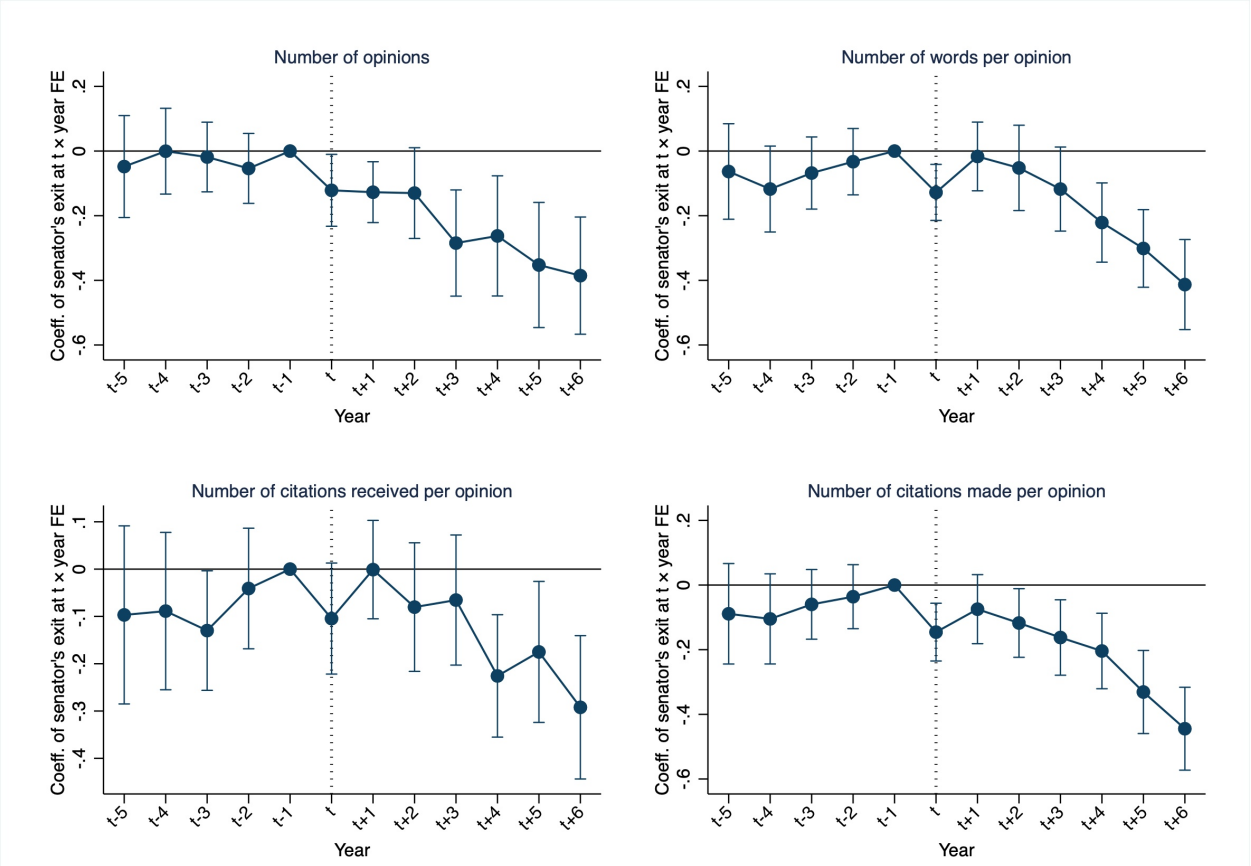
ADDITIONAL FIGURES

Figure A1: Promotions of District Court Judges in the Period 1789-2019



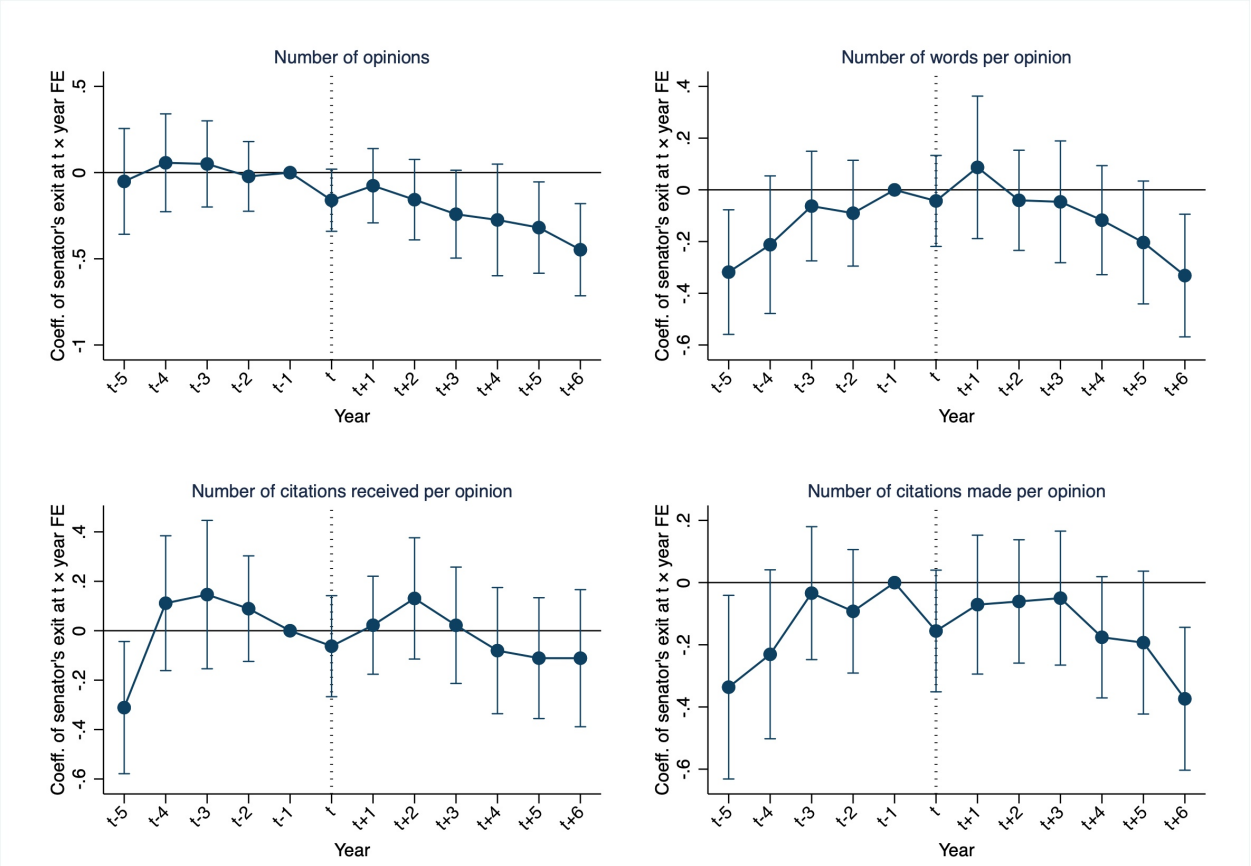
Notes: The figure reports the number of federal district court judges, who are part of our sample as described in Section 3.3 and got promoted to an appellate court, in every year from 1789 to 2019.

Figure A2: Effect of Losing Connection on Judges' Output – All Senator Exits



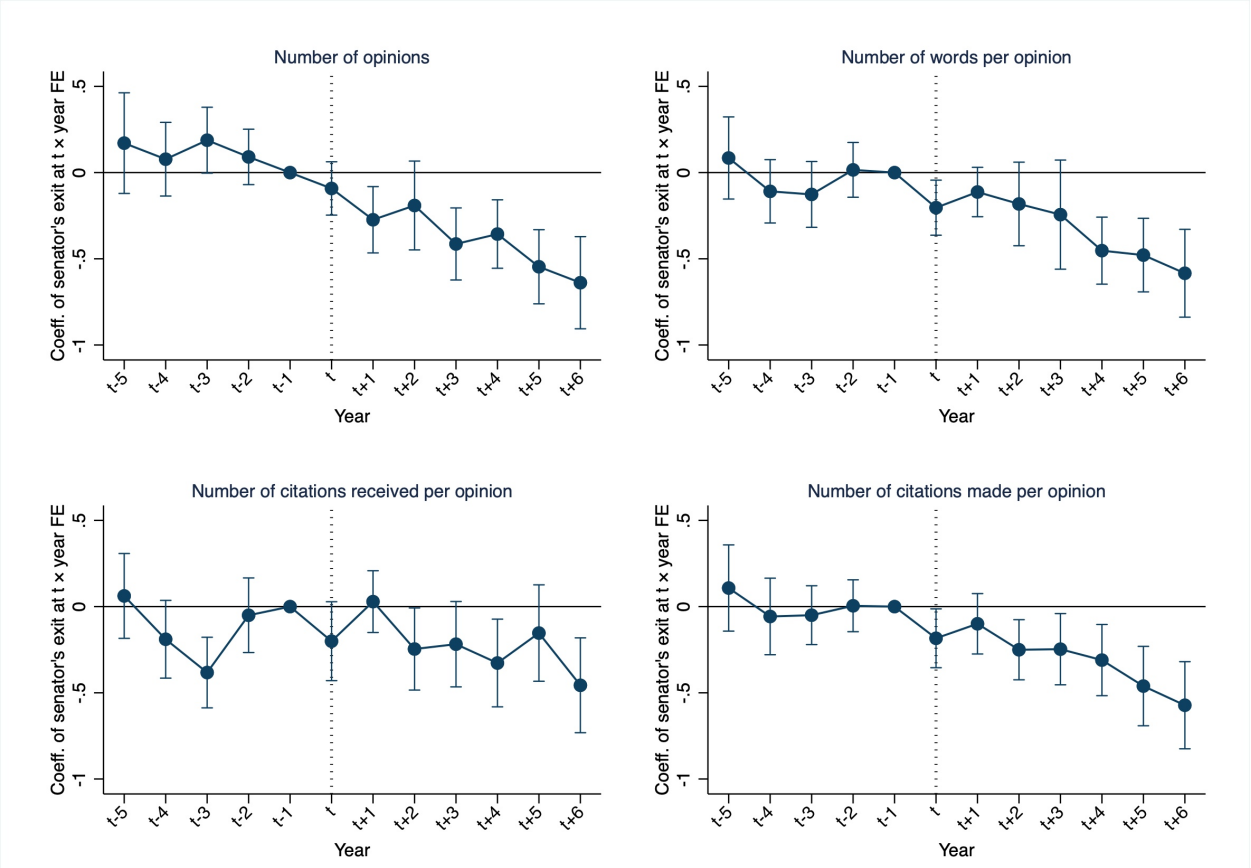
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are the number of opinions written by judge *i* in a given year (top left); the average number of words in the opinions written by judge *i* in a given year (top right); the average number of forward citations for the opinions written by judge *i* in a given year (bottom left); and the average number of backward citations for the opinions written by judge *i* in a given year (bottom right). All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A3: Effect of Losing Connection on Judges' Output – Only First Senator Exits



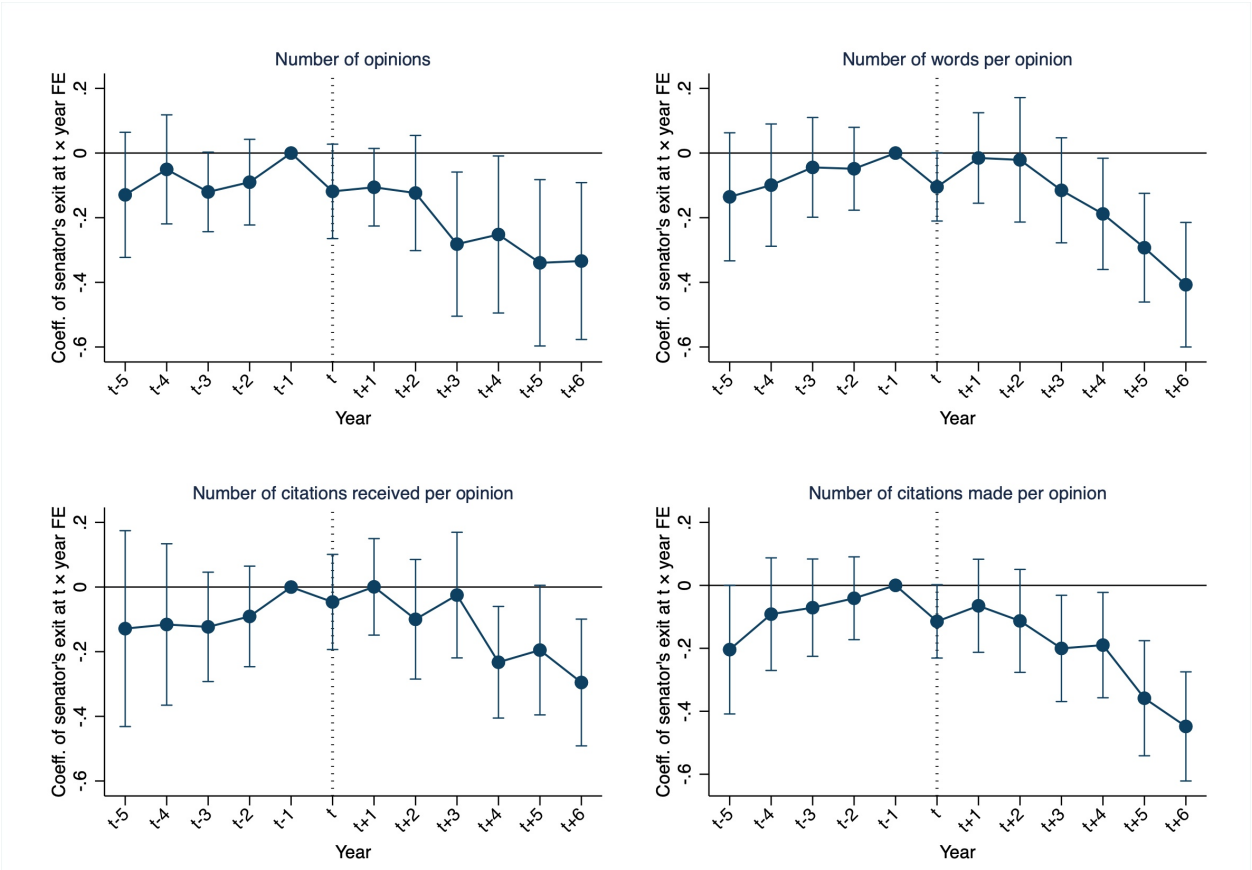
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are the number of opinions written by judge *i* in a given year (top left); the average number of words in the opinions written by judge *i* in a given year (top right); the average number of forward citations for the opinions written by judge *i* in a given year (bottom left); and the average number of backward citations for the opinions written by judge *i* in a given year (bottom right). All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A4: Effect of Losing Connection on Judges' Output – Only Second Senator Exits



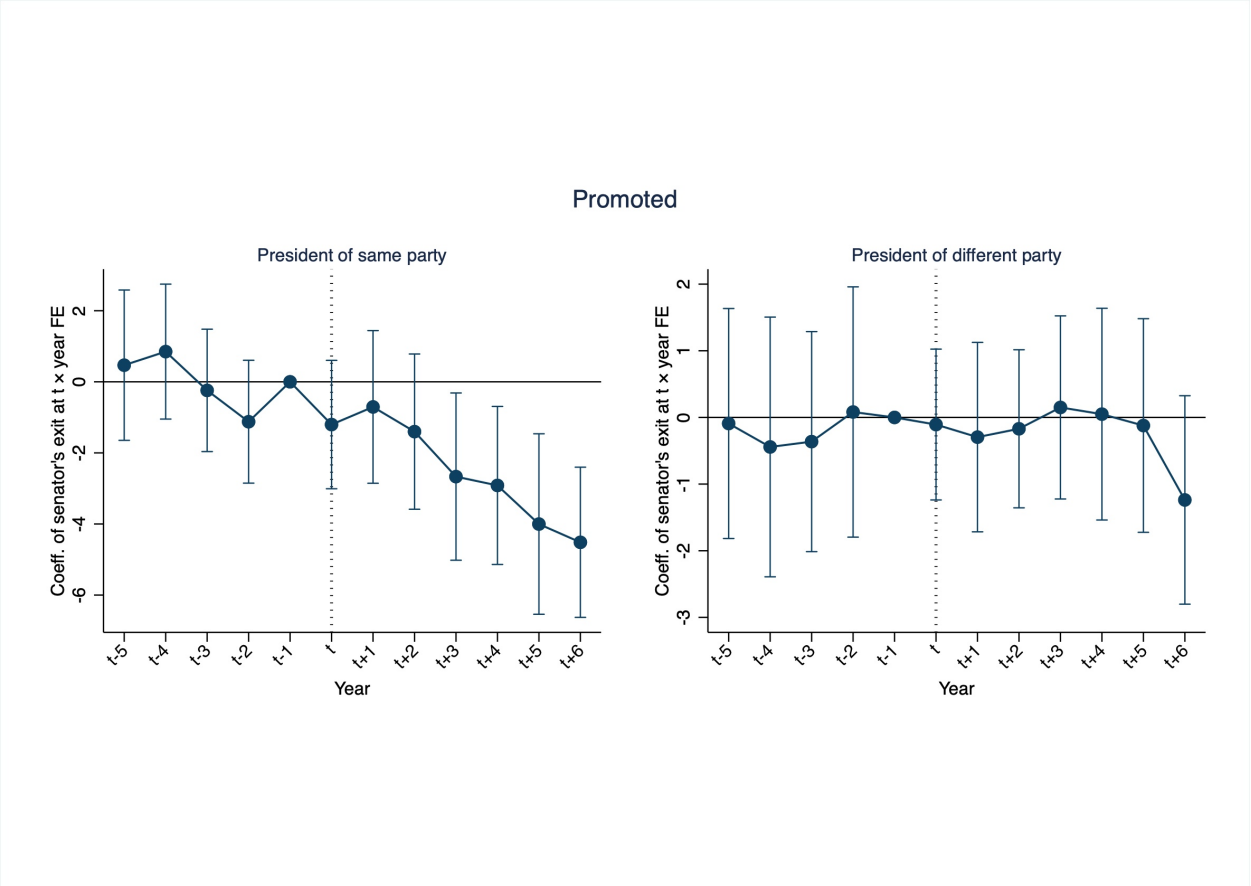
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are the number of opinions written by judge *i* in a given year (top left); the average number of words in the opinions written by judge *i* in a given year (top right); the average number of forward citations for the opinions written by judge *i* in a given year (bottom left); and the average number of backward citations for the opinions written by judge *i* in a given year (bottom right). All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A5: Effect of Losing Connection on Judges' Output – Only Unique Senator Exits



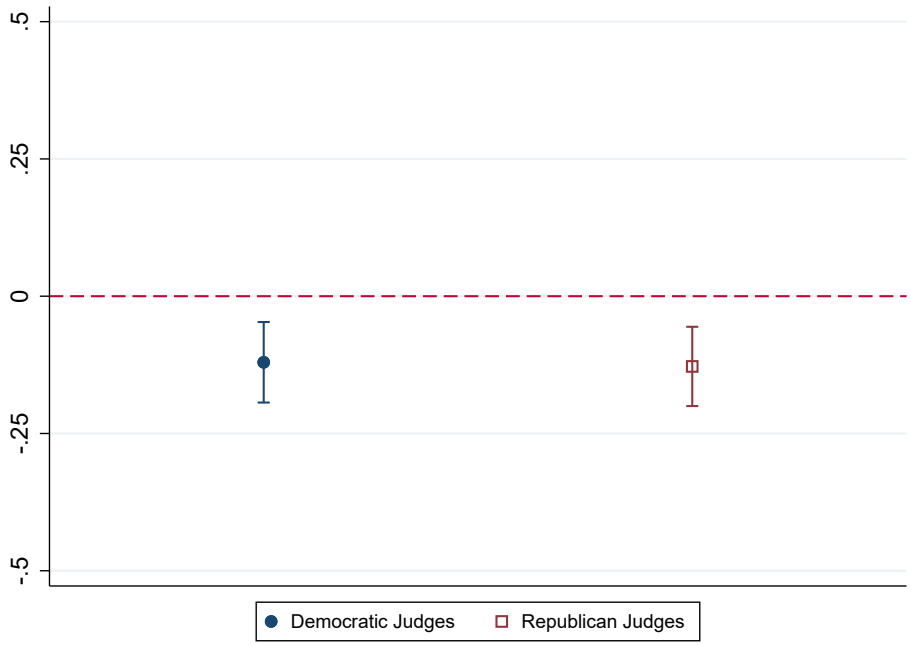
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variables are the number of opinions written by judge *i* in a given year (top left); the average number of words in the opinions written by judge *i* in a given year (top right); the average number of forward citations for the opinions written by judge *i* in a given year (bottom left); and the average number of backward citations for the opinions written by judge *i* in a given year (bottom right). All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A6: Effect of Losing Connection on Judges' Promotions, by President's Party



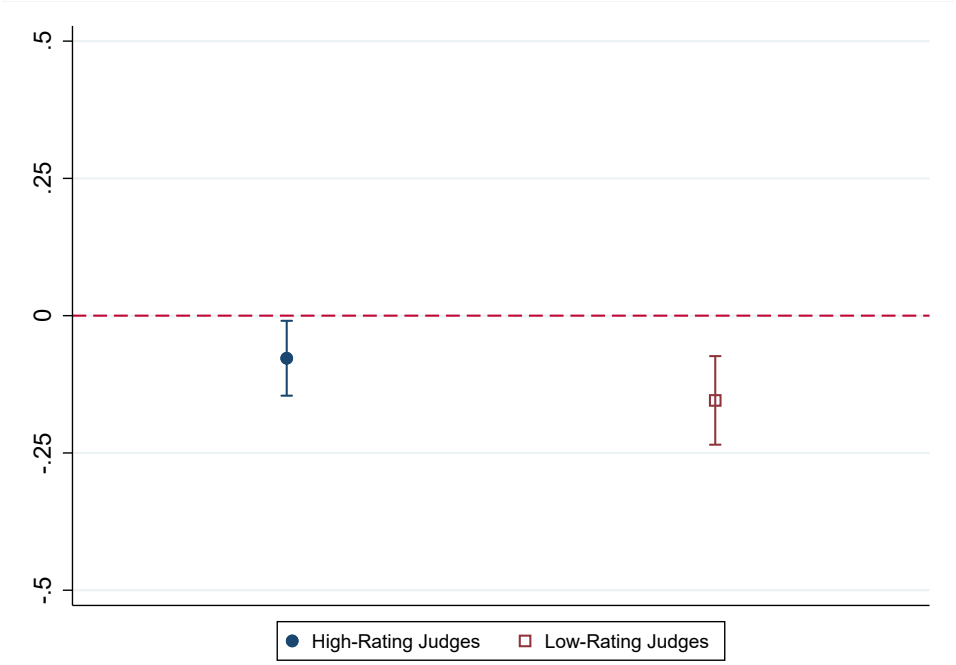
Notes: The figure reports the estimates from equation (2), interacted with an indicator for whether the incumbent president is of the same party as the judge (left panel) or of a different party (right panel). The dependent variable is an indicator for district judge *i* being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge × event and year × event fixed effects.

Figure A7: Effect of Losing Connection on Number of Opinions, Heterogeneity by Judge's Partisan Affiliation – Unique Exit



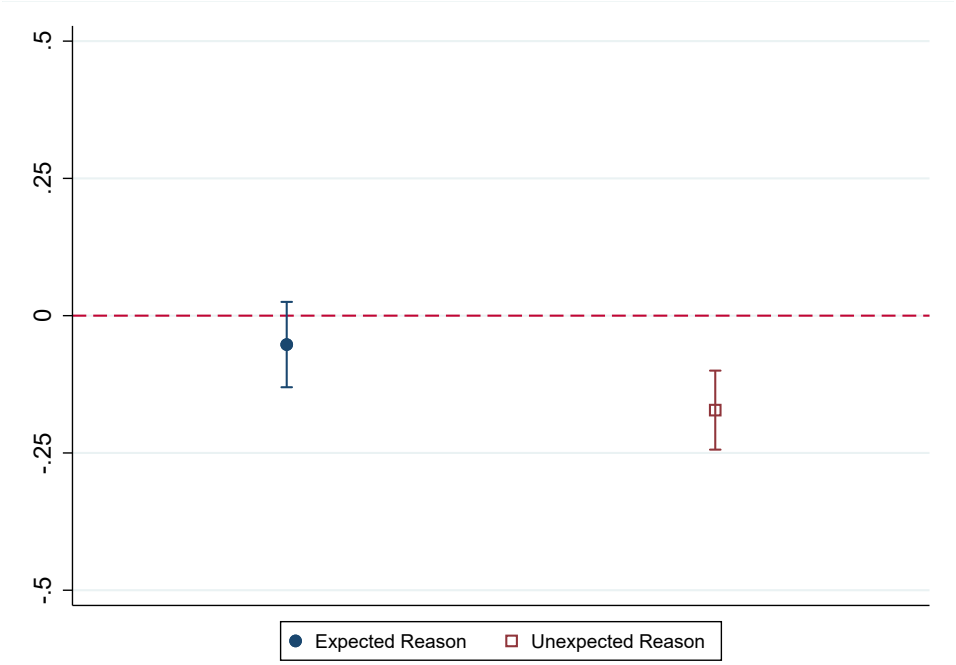
Notes: The dependent variables is the number of opinions written by judge i in year t. All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A8: Effect of Losing Connection on Number of Opinions, Heterogeneity by Judge's ABA Rating – Unique Exit



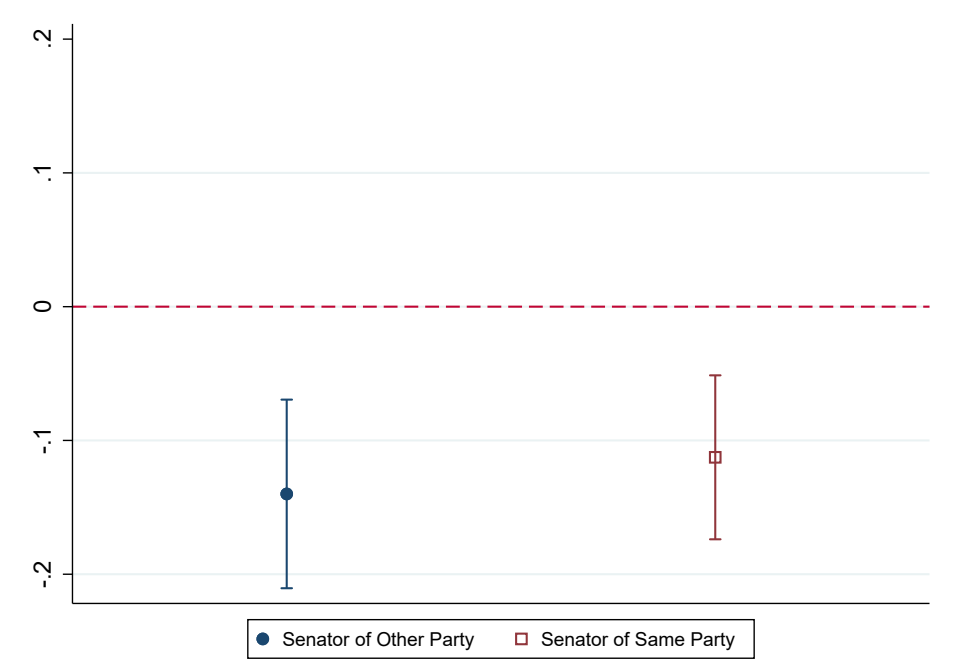
Notes: The dependent variables is the number of opinions written by judge i in year t. All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A9: Effect of Losing Connection on Number of Opinions, Heterogeneity by Senator's Reason of Exit – Unique Exit



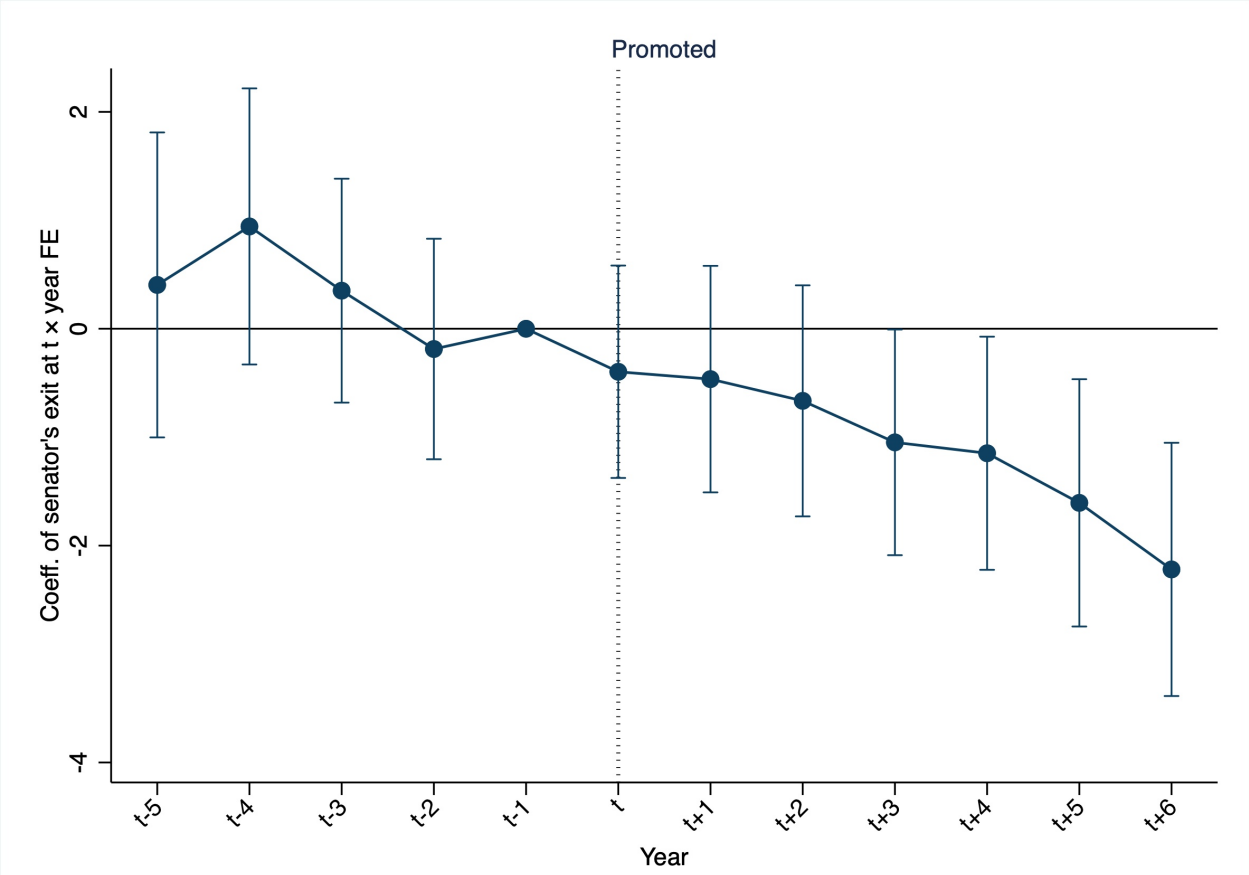
Notes: The dependent variables is the number of opinions written by judge i in year t. All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A10: Effect of Losing Connection on Number of Opinions, Heterogeneity by Party of Replacing Senator – Unique Exit



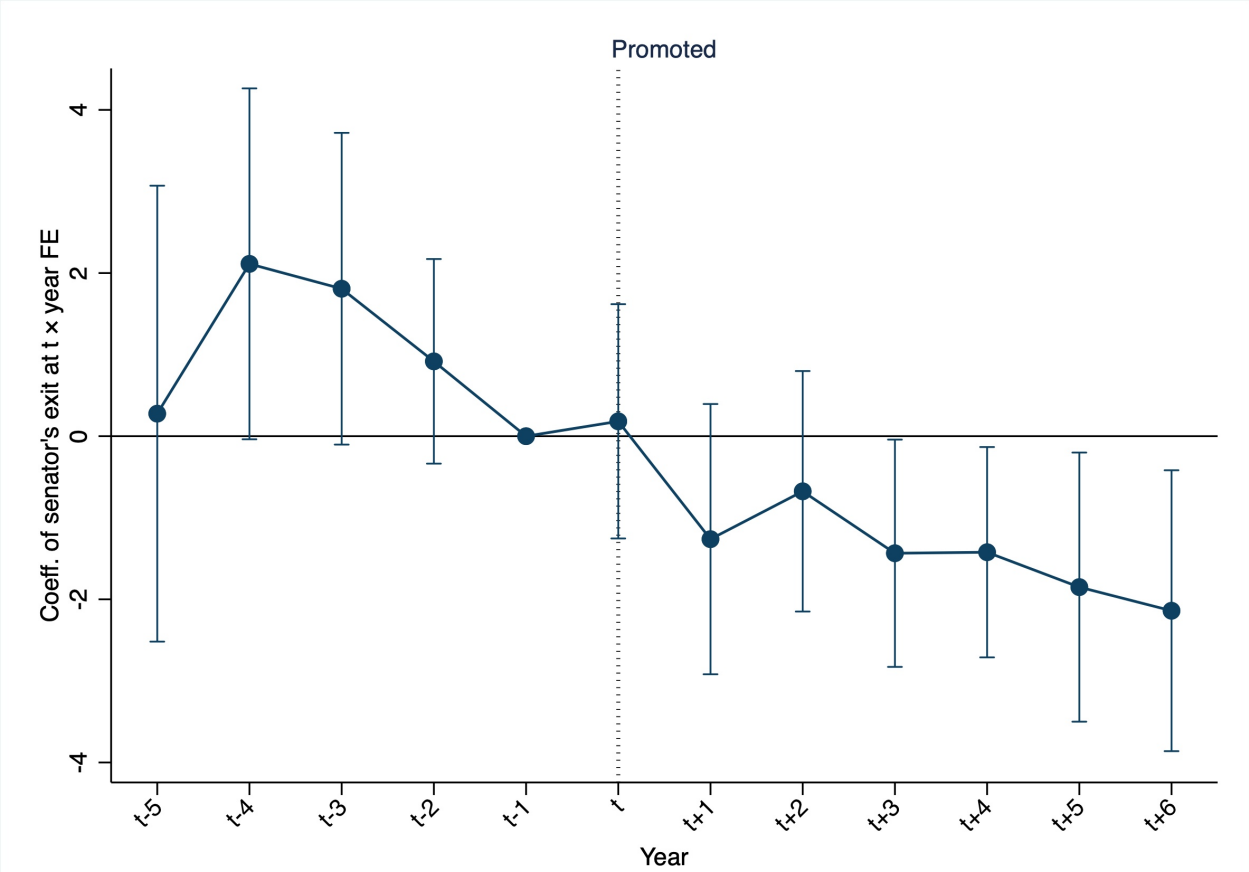
Notes: The dependent variables is the number of opinions written by judge i in year t. All coefficients are estimated using Poisson regressions. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A11: Effect of Losing Connection on Judges' Promotions – All Senator Exits



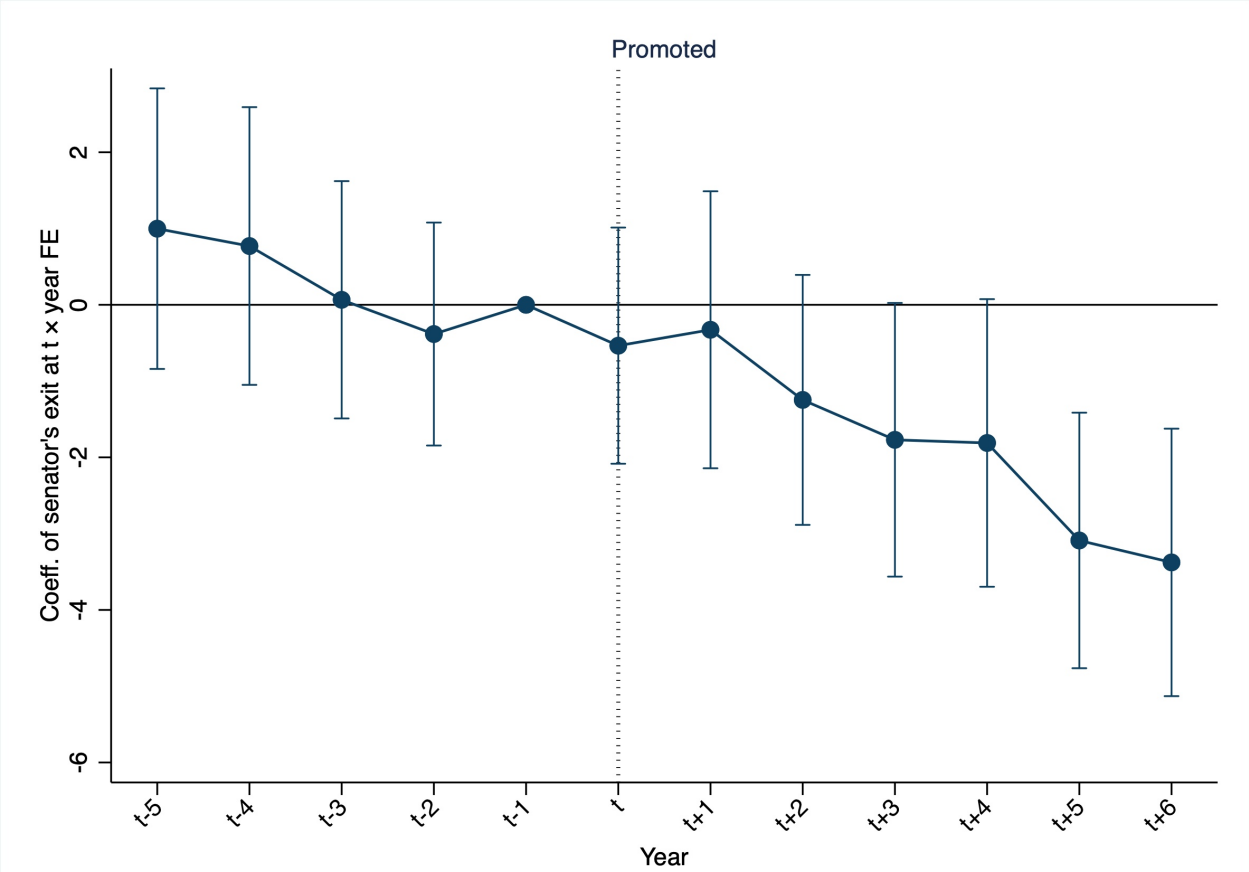
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variable is an indicator for district judge i being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A12: Effect of Losing Connection on Judges' Promotions – Only First Senator Exits



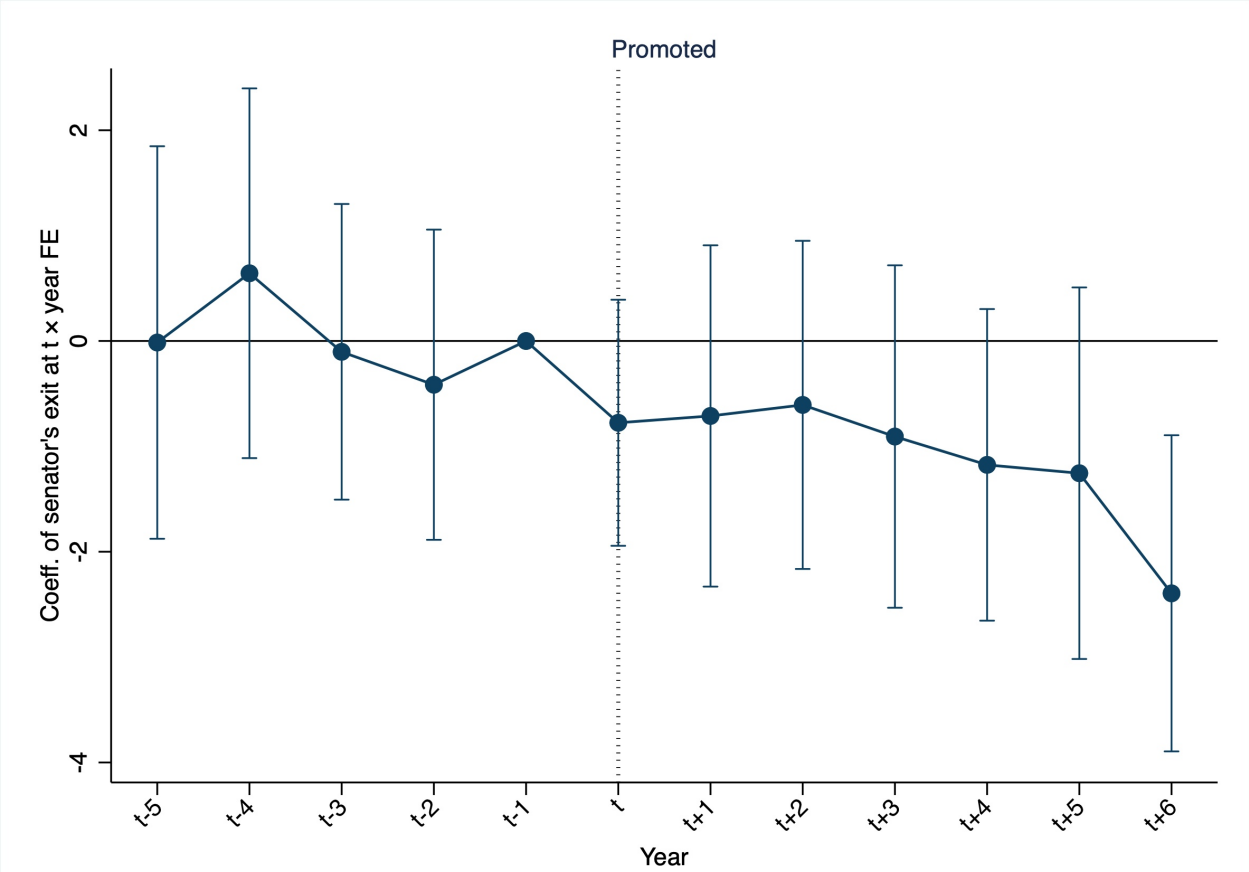
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variable is an indicator for district judge i being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A13: Effect of Losing Connection on Judges' Promotions – Only Second Senator Exits



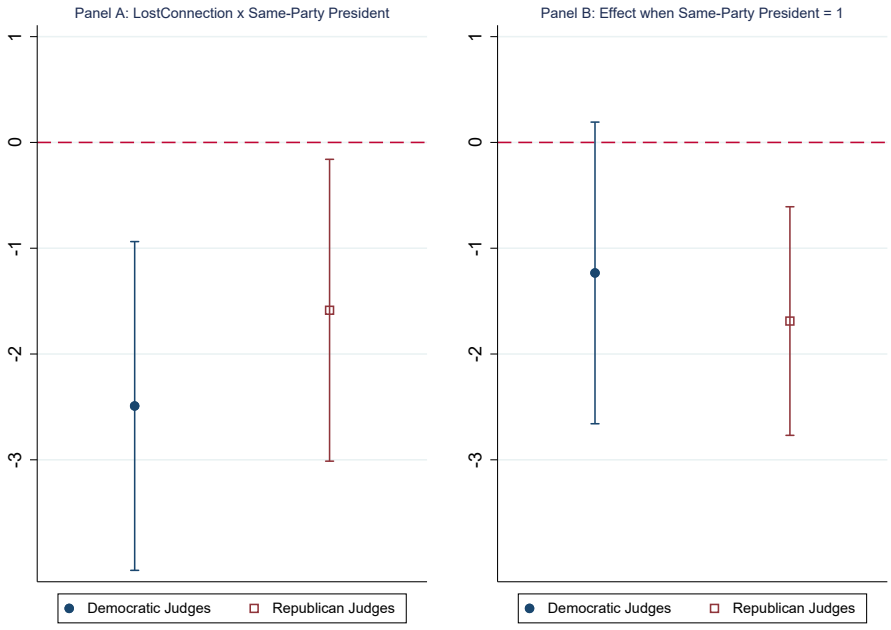
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variable is an indicator for district judge i being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A14: Effect of Losing Connection on Judges' Promotions – Only Unique Senator Exits



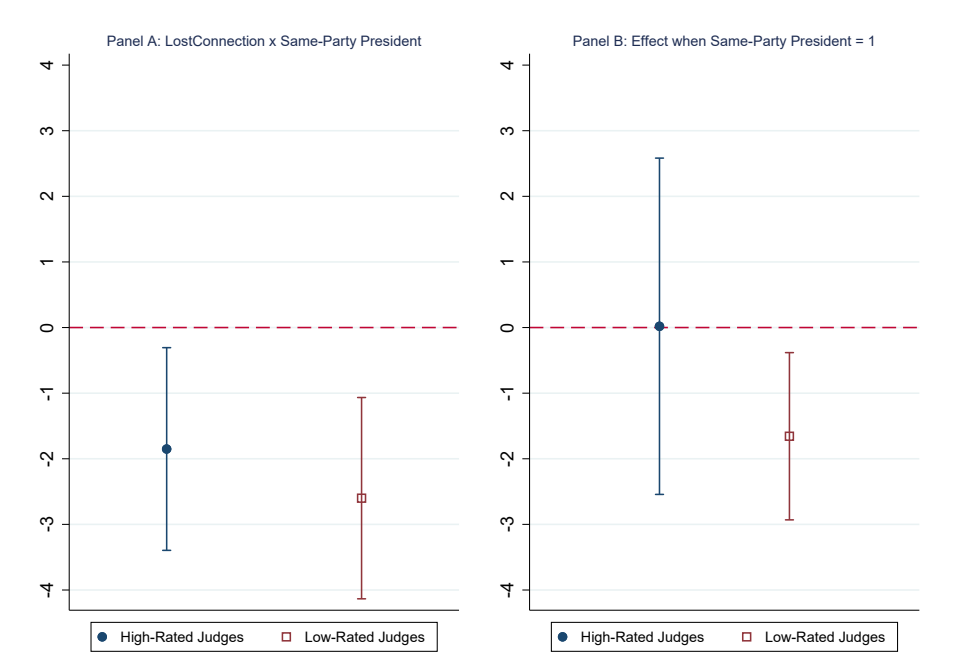
Notes: The figure reports the estimates from equation (2), which corresponds to an augmented version of equation (1), where the estimated difference between treated and control judges is allowed to vary for each year around the senator exit wave. The dependent variable is an indicator for district judge i being promoted in a given year (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the judge level. All regressions include judge $\times$ event and year $\times$ event fixed effects.

Figure A15: Effect of Losing Connection on Promotions,
Heterogeneity by Judge's Partisan Affiliation - Unique Exit



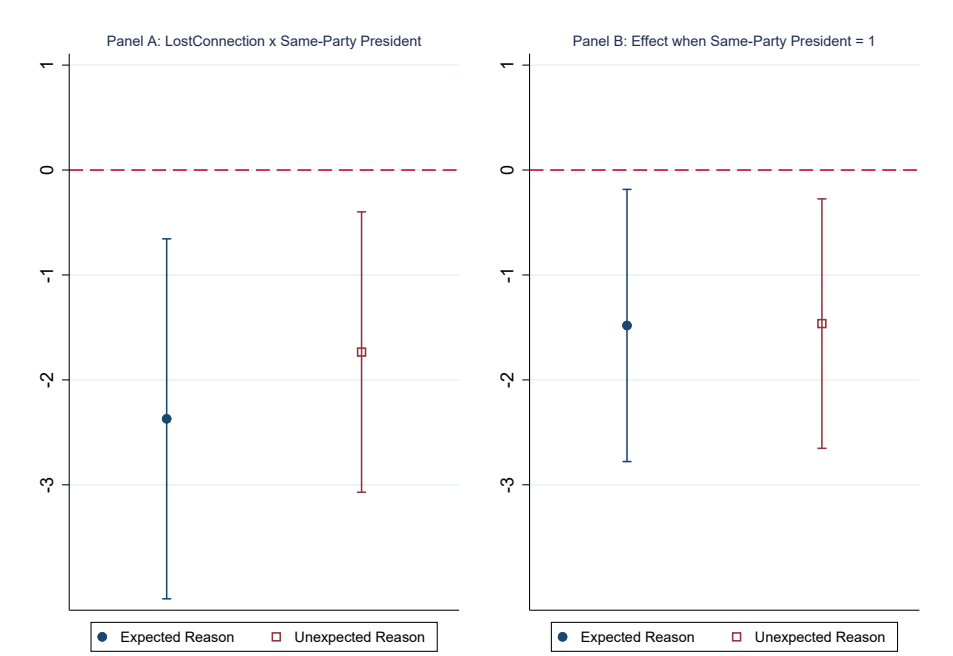
Notes: The dependent variable is an indicator for district judge i being promoted at year t (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A16: Effect of Losing Connection on Promotions,
Heterogeneity by Judge's ABA Rating - Unique Exit



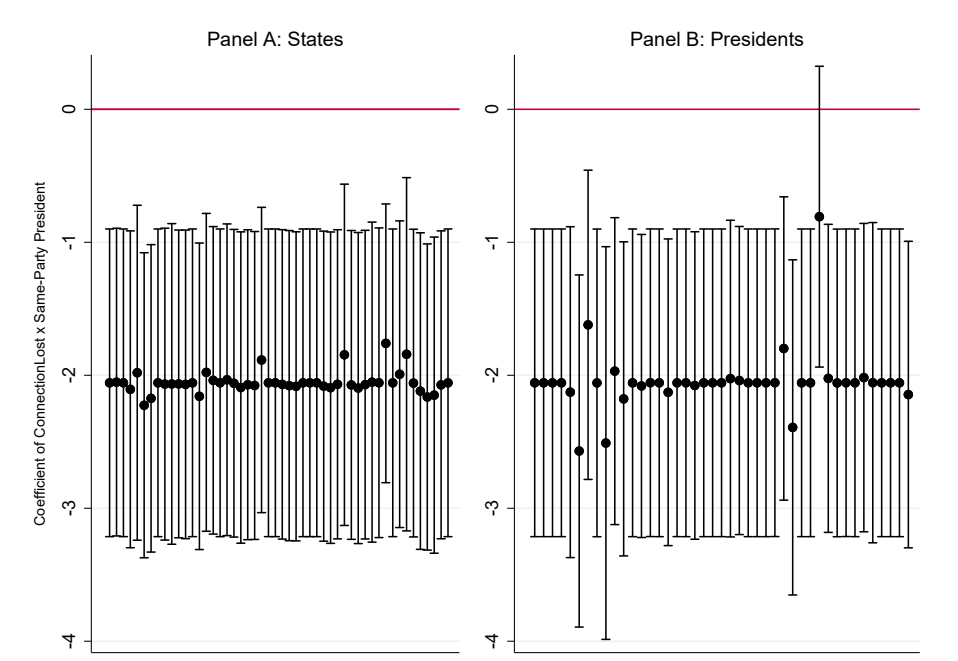
Notes: The dependent variable is an indicator for district judge *i* being promoted at year *t* (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A17: Effect of Losing Connection on Promotions,
Heterogeneity by Senator's Reason of Exit - Unique Exit



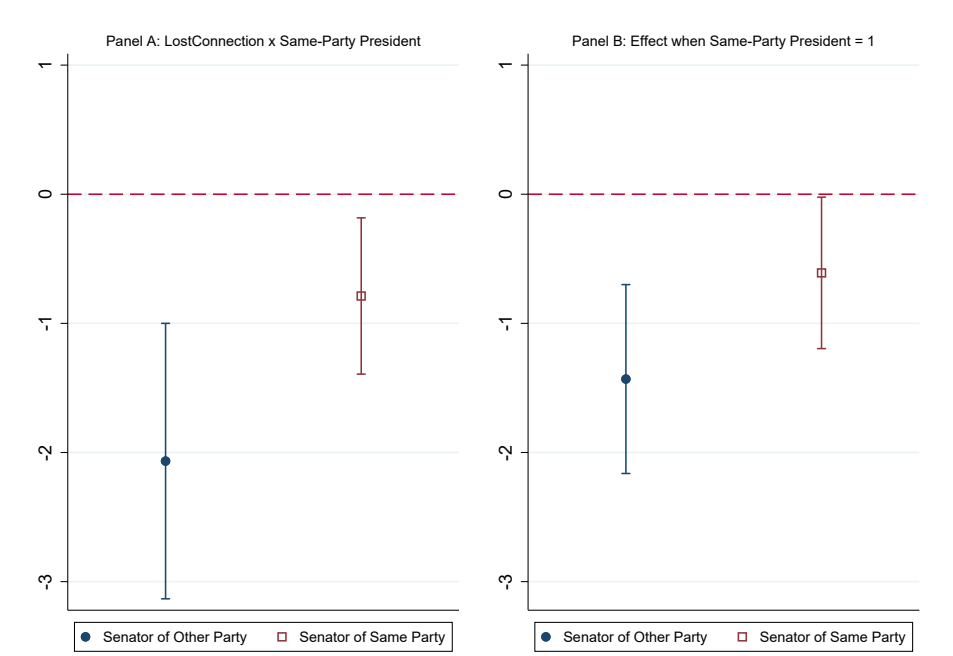
Notes: The dependent variable is an indicator for district judge *i* being promoted at year *t* (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.

Figure A18: Effect of Losing Connection on Promotions,
Excluding States and Presidents − Unique Exit



Notes: The dependent variable is an indicator for district judge i being promoted at year t (multiplied by 100). Point estimates are the marginal effect of losing the connection with the recommending senator when the president is of the same party as the judge. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level.

Figure A19: Effect of Losing Connection on Promotions,
Heterogeneity by Party of Replacing Senator – Unique Exit



Notes: The dependent variable is an indicator for district judge *i* being promoted at year *t* (multiplied by 100). Vertical lines are 95% confidence intervals based on robust standard errors clustered at the recommending senator-year level. Regressions include the following sets of FEs: judge, state by year, and judge's experience. This sample includes only district court judges who had one connection at the time of appointment.