A Judge in the Family:

The Value of Kinship Ties in Legal Markets

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Abstract

Can social ties to public officials generate private returns when formal rules block favoritism? I argue that social ties can create value beyond serving as a channel for favoritism by changing how third parties behave toward connected actors. I study São Paulo's appellate court—the world's largest court by caseload—where mandatory recusal bars judges from hearing cases involving relatives. I combine court records, a federal registry of law-firm partners, payroll data, and judicial appointment rosters to identify firms whose equity partners are related to appellate judges. I first show that 22.5 percent of sitting judges have a relative who is a law-firm partner. Using the staggered timing of judicial appointments, I find that connected firms gain corporate business—about two additional corporate clients per year, roughly doubling the typical firm's corporate clientele. Connected lawyers already win more often before their relative's appointment, but their win rate does not increase afterward. Judges virtually never adjudicate cases involving their relatives' firms. Judicial appointments therefore increase the market value of relatives' firms without improving outcomes for the firms' clients in court, consistent with third parties responding to the firm's visible proximity to judicial power. Finally, judges' relatives are more likely than unconnected lawyers to be law firm partners, consistent with family ties conferring advantages on both sides of the legal market.

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1. Introduction

While formal laws aimed at curbing favoritism and nepotism are increasingly prevalent, social ties—and family ties in particular—may still affect the functioning of institutions and markets. Existing work has focused on family ties connecting firms and politicians or operating within public bureaucracies. In this article, I focus on harder-to-observe family ties between the judiciary—appellate judges—and law firms. I study the role of such ties in the São Paulo State Court, the largest appeals court in the world (TJSP, 2024).

I argue that social ties have audience value as well as instrumental value: they shape not only what connected actors can do for one another but also how third parties respond to them. Specifically, I show how this mechanism operates in the context of proximity to public office, where formal rules constrain direct favoritism. A judicial appointment transforms a preexisting family tie into a visible connection to judicial power, and prospective clients respond to this connection.

I evaluate this argument in São Paulo's state court system, one of the world's largest by caseload. I focus on law firms with an equity partner whose relative is appointed to São Paulo's appellate court. Mandatory recusal bars judges from hearing cases handled by relatives, removing the most direct way the appointment could benefit the firm—adjudication by the relative. Yet recusal does not prevent prospective clients from responding to the affiliation and thereby increasing demand for the connected firm.

I combine case-level records extracted from São Paulo's appellate court daily electronic gazette with Brazil's register of company partners, the matched employer–employee census (RAIS), and judicial rosters. I use these sources to build two datasets. The first is a case-by-lawyer panel of nearly 8.85 million observations, which I use to examine whether connected lawyers obtain more favorable outcomes. The second is a firm-by-year panel of 497 connected and matched unconnected São Paulo law firms observed from 2008 to 2024. Using the staggered appointment of lawyers' relatives to the court, I estimate how these appointments affect firms' caseloads and other outcomes. The mapping identifies 410 firms connected to a judge; the event-study analysis uses the 50 firms whose connected judge was appointed during the study period.[1]

I document three sets of findings. First, family ties between judges and law firms are a feature of São Paulo's legal market. In 2024, roughly 22 percent of judges sitting on São Paulo's appellate court—81 of 360—had a relative registered as a law-firm partner. Across all judges observed in the data, 410 of São Paulo's 8,339 registered law firms (4.9 percent) were connected to at least one judge and 31 percent of connected firms were linked to two or more judges.

Second, connected lawyers have a win rate 2.9 percentage points higher in cases whose judge is assigned by lottery. This difference predates the relative's appointment and does not increase afterward. Appointment to the bench therefore creates no additional advantage for the firm's clients in court. Appointed judges almost never adjudicate cases handled by their relatives' firms, consistent with effective enforcement of the recusal rule.

Third, after a relative becomes a judge, connected firms gain corporate business. The number of corporate clients increases by 0.341 log points ($p = 0.020$)—about two additional corporate clients per year, roughly doubling the typical firm's corporate clientele—and corporate caseload increases by 0.309 log points ($p = 0.048$). Corporate clients increase within the first year after appointment and then remain flat. Overall caseload increases more gradually, but the effect is imprecise (0.287 log points, $p = 0.13$). Connected law firms also experience an increase in the number of partners, but the effect is noisily estimated. I find no effects on the number of employees or wages.

These results show that public office can generate private returns through family ties even when recusal bars the official from handling relatives' cases and the appointment does not improve outcomes for the firm's clients in court. Formal rules such as mandatory recusal may constrain direct favoritism without eliminating the private market value of family connections.

[1] Of the 410 connected firms, 142 have a usable appointment date. Sixty-three are connected to judges appointed before 2011 and are therefore always treated in the 2008–2024 panel, leaving 79 firms with an in-window appointment. Of these, 52 were connected at the appointment itself—the relative was already a partner—and 50 remain once two firms are excluded because a second relative was already deciding cases when the appointment occurred; those 50 constitute the analysis sample (Supplementary Information (SI) Section E).

This article contributes to three literatures. First, a growing literature documents how connections affect judicial decisions. Huang, Roychowdhury and Sletten (2025) show that U.S. firms fare better in securities class actions when their executives share educational connections with the assigned judge. Similarly, in the same setting as this study, Souza (2026) shows that an educational connection between lawyers and reporting judges improves the appellee's probability of winning. Lu, Pan and Zhang (2015) document that state-owned and politically connected private firms obtain better court outcomes in corporate lawsuits in China. Liu et al. (2025) show that former Chinese judges who become lawyers win more often because of both expertise and connections to former colleagues. Across these studies, connections—both political and judicial—benefit litigants or lawyers by producing favorable judicial treatment. I shift the focus to a different outcome: whether a judicial appointment increases demand for a relative's law firm even in the absence of favorable adjudication.

Second, I add to the literature on the private benefits of public office (Fisman, Schulz and Vig, 2014). Some of this work shows that the returns to officeholding extend beyond officeholders to their families by improving the labor-market outcomes of politicians' relatives (Fafchamps and Labonne, 2017; Folke, Persson and Rickne, 2017). Other studies show that private firms strategically hire officials' relatives, with evidence consistent with a quid pro quo logic (Szakonyi, 2019; Gagliarducci and Manacorda, 2020). Recent work on nepotism examines officials' use of discretion to benefit relatives inside bureaucracies and courts (Ríos-Figueroa, 2022; Riaño, 2024; Brassiolo et al., 2026). Like Szakonyi (2019), I study family ties that span public institutions and private firms. But I identify a different mechanism through which public-private family connections generate economic returns: rather than improving relatives' employment prospects or facilitating favorable government treatment, a judicial appointment increases demand for a relative's firm.

Finally, this paper contributes to the literature on family ties. A broad comparative literature studies whether kinship-based institutions support or hinder political and economic development (Alesina and Giuliano, 2011; Banfield, 1958; Fukuyama, 2011; Greif, Mokyr and Tabellini, 2025;

Henrich, 2020). Some studies examine how kinship ties solve cooperation problems and give relatives an advantage in collective action (Enke, 2019; McNamara and Henrich, 2017). More recent work examines how family ties operate in strategic settings, shaping regime dynamics (Naidu, Robinson and Young, 2021), authoritarian redistribution (Bandiera, Larreguy and Mangonnet, 2026), and cooperation within firms in ways that can limit the effectiveness of campaign finance reform (Balán, Dodyk and Puente, 2026). This literature typically studies the benefits of network centrality or the instrumental value of kinship ties—what relatives can do for one another. By contrast, I document how kinship ties acquire economic value through the responses of third-party observers, suggesting that measures focused only on direct exchanges between relatives may underestimate their value. More broadly, the fact that kinship ties have economic value and link actors on both sides of the legal market despite formal recusal rules illustrates how kinship can be a pre-institutional source of capture and institutional weakness (Brinks, Levitsky and Murillo, 2019).

2. How proximity to power creates private value

Connections to public officials can generate substantial private returns. Officeholders' relatives experience gains in employment and earnings (Folke, Persson and Rickne, 2017; Fafchamps and Labonne, 2017), while politically connected firms may gain access to officials, government contracts, or favorable regulatory treatment (Szakonyi, 2019). Social connections also shape hiring, promotion, and pay within public bureaucracies (Riaño, 2024). Across these settings, connections are valuable because they provide—or are thought to provide—resources, access, or preferential treatment.

I argue that social ties have audience value as well as instrumental value: they affect what connected actors can do for one another and how third parties evaluate them. I show how this audience value operates in the context of proximity to public office, even when formal rules constrain direct favoritism. I build on work in economic sociology that distinguishes between networks as pipes, which convey resources and information between connected actors, and networks as prisms, through which observers use affiliations to infer the quality or status of potential exchange partners (Podolny, 2001). In this sense, social ties generate perceptual externalities.

Existing work examines how affiliations with prominent market actors certify uncertain quality (Podolny, 1993, 2001; Stuart, Hoang and Hybels, 1999). I shift the focus to proximity to power as an audience heuristic. Work on political dynasties treats family names as brands that signal reputation, name recognition, organizational resources, and political capital, helping voters evaluate candidates (Smith, 2018). However, because ties to power may provide actual access while also generating expectations of access, existing work often cannot separate what the connection delivers from how outsiders respond to it.

I evaluate this argument by studying law firms with an equity partner whose relative is appointed to São Paulo's appellate court. Mandatory recusal prohibits judges from hearing cases handled by their relatives; it removes the most direct way in which the appointment could benefit the firm: adjudication by the relative. However, recusal rules do not prevent potential clients from responding to the firm's association with judicial office: the appointment transforms a family tie into a visible connection to judicial power in a context where direct favoritism is formally constrained. Appointments are public acts, published in the official gazette and announced on the court's news portal, so the signal is visible to the market.[2] Because the quality of legal representation and the likelihood of success are difficult to assess ex ante, visible affiliations—such as a connection to a judge—can change how potential clients behave toward a firm.

Although direct favoritism and the proposed audience mechanism generate the same main prediction—demand for a lawyer's firm increases when the lawyer's relative is appointed to the court—their observable implications differ. If appointment enables favoritism, it should create an advantage in court, causing the firm's win rate to increase after appointment. By contrast, if the appointment changes third-party behavior rather than the firm's treatment in court, any performance

[2] See SI Section G.

advantage should exist prior to the appointment. The two mechanisms also imply different demand dynamics: An advantage based on favoritism should fade when the judge leaves the bench, whereas demand generated by a change in third-party behavior may persist.

3. Setting

Appellate court organization. The São Paulo State Court (*Tribunal de Justiça do Estado de São Paulo*, TJSP) hears appeals from trial courts distributed across the state's 320 judicial districts (*comarcas*). It is considered the world's largest court by caseload. In 2024, its second instance comprised 360 appellate judges (*desembargadores*) (TJSP, 2024). The second instance is organized into three subject-matter divisions: Private Law, Public Law, and Criminal Law. Each division is subdivided into chambers of five judges. Panels composed of three judges decide cases assigned to a chamber (Tribunal de Justiça do Estado de São Paulo, 2026, arts. 30, 34, and 41). Each appeal is assigned to a reporting judge (*relator*), who reviews the record and prepares the initial vote. Unless one of the statutory grounds for a single-judge decision applies, the panel decides the appeal and publishes the resulting opinion as an *acórdão* (Brazilian Civil Procedure Code, 2015, arts. 931–941 and 1,011).

Appointment to the appellate court. TJSP appellate judges (*desembargadores*) are appointed. Four-fifths of the seats are filled by career judges, who reach the appellate bench through promotions that alternate between seniority and merit (Brasil, 1988, art. 93, II–III). Seniority promotions ordinarily go to the most senior eligible judge, whereas candidates for merit promotion must satisfy tenure and seniority requirements. Since 2010, merit promotions have been decided in public sessions through nominal, open, and reasoned voting based on objective criteria, including performance, productivity, promptness, and professional development (Conselho Nacional de Justiça, 2010, arts. 1, 3–4). The judiciary controls these decisions: the Conselho Superior da Magistratura submits the promotion lists, the 25-member Órgão Especial evaluates them, and the TJSP president signs the appointment.[3] The remaining fifth of the seats is reserved for lawyers and public prosecutors with at least ten years of professional experience. Their professional body submits a six-name list, the court reduces it to three, and the governor selects one nominee within twenty days (Brasil, 1988, art. 94). The "constitutional fifth" is the only route in which the executive selects the appointee.

Vacancies arise from death, appointment to a higher court, or retirement. Compulsory retirement occurred at age 70 until 2015 and at age 75 thereafter (Brasil, 2015*a*,*b*). Vacancies, not actions by candidates' family firms, therefore set the appointment calendar. The seniority queue makes the identity of a likely career-track appointee partly predictable. Compulsory retirement also makes some vacancy dates foreseeable. The shifted treatment dates reported in SI Section C test for anticipatory changes around appointment. I define the treatment date as the date of swearing-in (*posse*), when the appointee takes the oath before the TJSP president and the court records a formal term.[4] Each appointment is publicly announced on the court's news portal. The announcements usually mention the incoming judge's family—in four cases naming the matched partner (SI Section G).

Of the 50 treated firms, 41 are connected to career-track judges, 7 to judges appointed through the constitutional fifth (five prosecutors and two lawyers), and 2 to judges whose appointment track the records do not resolve.

Case assignment. An electronic lottery assigns new appeals at the TJSP. The system alternates cases at random among eligible judges with "rigorous equality," and the court publishes the distribution lists in the official gazette (Brazilian Civil Procedure Code, 2015, arts. 285 and 930). The TJSP holds public draws on fixed days of the week, subject to prevention and impediment rules. Each case then goes directly to the drawn reporting judge (*relator*) (Tribunal de Justiça do Estado de São Paulo, 2026, arts. 181 and 183). The lottery operates within the competent judicial organ. The subject stated in the initial pleading determines which division and chambers can hear the

[3] RITJSP arts. 8, 13(II)(h), 16(I), and 26(II)(g).
[4] RITJSP art. 57.

case, and later changes do not alter that competence (Tribunal de Justiça do Estado de São Paulo, 2026, arts. 103–104).

Cases that do not enter the lottery are routed through different rules. Under the *prevention* rule, the first appeal fixes the reporting judge for every later appeal in the same or a connected case (Brazilian Civil Procedure Code, 2015, art. 930, sole paragraph). Prevention also attaches to the chamber and survives the departure of its judges (Tribunal de Justiça do Estado de São Paulo, 2026, art. 105). Under the *dependency* rule, connected or refiled claims return to the judge already assigned (Brazilian Civil Procedure Code, 2015, art. 286). The *exclusive competence* rule sends certain subjects to the TJSP's reserved chambers.

Parties cannot choose a judge. The natural-judge principle and the lottery bar that choice (Brasil, 1988, art. 5, XXXVII and LIII). Dismissing and refiling a claim does not produce a new draw: dependency returns the claim to the same judge even if the parties change in part (Brazilian Civil Procedure Code, 2015, art. 286, II). Under CPC art. 288 and RITJSP art. 182, the court corrects distribution errors through compensated redistribution, preserving equality across judges (Brazilian Civil Procedure Code, 2015; Tribunal de Justiça do Estado de São Paulo, 2026). A party can frame the subject of the initial pleading and thereby affect the pool of eligible judges, but cannot select a judge within that pool.

Each e-SAJ record identifies the route through which the case was assigned. The lottery covers 36,387 of the 60,070 case–lawyer observations in the analysis sample (61 percent).[5] Within lottery cases and chamber-years, connected and unconnected lawyers face the same distribution of judges (Souza, 2026; Huang, Roychowdhury and Sletten, 2025).

Recusal rule. Brazilian law bars judges from adjudicating cases in which a spouse, partner, or relative within the third degree acts as counsel. The restriction extends to cases handled by another lawyer from the relative's firm, even when the relative does not participate personally (Brazilian

[5] Of the remaining cases, 12,745 are routed by exclusive subject-matter competence and 1,547 by prevention—routing to the judge already seized of a related case; for 9,391 observations the record does not state the route in a parsable form. The unparsed observations enter the full-sample estimates but not the route-specific ones.

Civil Procedure Code, 2015, art. 144, III, §§ 1 and 3). Similar restrictions operate in the United States, Portugal, Spain, and Italy, although these systems vary in whether disqualification extends to other lawyers in the relative's firm.[6] The Brazilian rule removes the most direct way in which an appointment could benefit the relative's firm: adjudication by the appointed judge.

4. Data

I combine five data sources to construct two analysis datasets. The first is a case-lawyer panel comprising 8,849,474 observations, each with a coded judicial disposition.[7] I use this panel to test whether lawyers with a relative on the court receive more favorable decisions. The second is a firm-year panel of connected and unconnected law firms. I use this panel to estimate the firm-level effects of having a relative appointed to the court across a range of outcomes.[8]

1. **Lawyers.** To generate a list of lawyers, I extract case-level data from the São Paulo State Court electronic gazette (*Diário da Justiça Eletrônico*, DJE). I focus on appellate court cases (*Caderno 2*). The gazette is published every business day. The sample comprises 4,202 issues spanning 17 years (2008–2024), totaling nearly 11.8 million cases, of which 9.26 million have a dispositive outcome. Each issue contains information on the case, type of case, outcome, each lawyer's name and unique identifier, procedural side, and the deciding judge. This dataset contains 588,065 unique lawyers.

2. **Judges.** I obtain data on appellate (second-instance) judges (*desembargadores*) from three sources: (1) the official TJSP seniority roster (*Antiguidade*) of the sitting bench, comprising 360 judges; (2) a document (*Curriculum*) containing nearly 1,300 biographical entries

[6] See 28 U.S.C. § 455(b)(5) and Judicial Conference Advisory Opinion No. 58 (United States); Code of Civil Procedure, art. 115 (Portugal); Organic Law of the Judiciary, arts. 217 and 219 (Spain); and Code of Civil Procedure, art. 51 (Italy).

[7] The unit of observation is the lawyer appearance, not the case: a dispositive case contributes one row per lawyer whose name and identifier parse from its gazette entry, and a case whose entry lists no parsable lawyer contributes none, which is why this count is smaller than the 9.26 million dispositive cases.

[8] SI Section H defines every variable in both panels.

for judges dating back to the nineteenth century; and (3) the TJSP electronic gazette, which records each judge's first appearance as a deciding judge. The treatment variable—a lawyer's appointment date as a judge (*posse*)—is obtained from the first two sources and cross-referenced with judges' appearances as reporting judges (*relatores*) in the gazette. To identify kinship ties between judges and law firms, I measure each surname's rarity across the 588,065 lawyers appearing in the court gazette. I match each law-firm partner to a judge and classify them as kin under one of five rules: an identical full name up to a generational ending; one shared rare surname, held by approximately 0.003 percent of the lawyer roster (for example, partner Raissa Hisami Onita Ichihara and Judge Yoshiaki Ichihara); two shared surnames that are jointly rare, each held by no more than 0.4 percent of the roster (for example, partner José Gilberto Diniz Junqueira and Judge Paulo Diniz Junqueira), applied at two rarity thresholds; or one moderately rare shared surname that also appears in the firm's registered name. SI Section A states the exact thresholds and the count each rule contributes.

3. **Electronic case records.** I supplement the case data from the São Paulo State Court's electronic judicial gazette (DJE) with records from e-SAJ, the court's electronic case-record system. These records provide the assignment route, distribution date, disposition text, and, where reported, vote record. I use these fields to distinguish lottery from non-random assignment, measure time to decision, code partial relief and procedural disposal, and identify panel splits. The resulting case–lawyer panel contains 36,387 observations assigned by lottery, 12,745 by exclusive competence, and 1,547 by prevention.[9] I pool exclusive competence and prevention as non-random assignment.

4. **Law-firm partners.** I identify law firm partners using Brazil's official registry of firm owners and partners (*Receita Federal Sócios*), which contains firms' unique identifiers, partner names, and the dates on which they joined each firm.

[9] Another 9,391 observations lack a parsable assignment route and enter only the full-sample estimates.

5. **Law-firm outcomes.** I use data on the universe of formal establishments and the matched employer–employee census (*Relação Anual de Informações Sociais*, RAIS) to identify law firms (CNAE 6911-7/01, "escritórios de advocacia") and obtain data on each firm's number of employees, payroll, and worker tenure.

4.1. Prevalence of family ties

Family ties between appellate judges and law firms are common. As Table 1 shows, 81 of the 360 judges sitting on the TJSP in 2024 (22.5%) had at least one relative who was a registered partner of a São Paulo law firm. The prevalence is similar across appointment tracks: 22.9% among career judges, 25.0% among prosecutors appointed through the constitutional fifth rule, and 16.7% among lawyers appointed through that route. Kinship ties are thus not limited to politically appointed judges.[10]

[10] The pattern is stable over time: 65 of the 290 judges appointed between 2011 and 2024 (22.4%) have a matched partner-relative. This count combines ties that predate the appointment with relatives who became partners afterward; among matched judges appointed after 2010 whose partnership-entry and swearing-in dates are both recorded, the relative was already a partner at the appointment in 50 of 70 cases (SI Section E).

Table 1: The prevalence of family ties between judges and law firms.

	Judges	With a partner-relative in a law firm	Share
Career track	288	66	22.9%
Constitutional fifth: prosecutors	36	9	25.0%
Constitutional fifth: lawyers	36	6	16.7%
All sitting judges (2024)	360	81	22.5%
Judges appointed 2011–2024	290	65	22.4%

Notes: A judge has a partner-relative when the preferred matching definition (SI Section A) links them to at least one registered partner of a São Paulo law firm. The first three rows split the 2024 sitting bench by appointment track from the seniority roster; the last row counts all judges first appointed in 2011–2024, whether or not still sitting. The 81 connected sitting judges link to 133 partner-relatives at 118 firms. Because the matching rule can both include unrelated namesakes and miss relatives it cannot detect, the prevalence figure is bounded rather than exact: applying the false-positive rates from the documentary audit (a floor of 6.3 percent and a point estimate of 13.5 percent among the cases the records could settle) and the false-negative bound of 8 percent from the missed-relatives audit gives a range of 19.5 to 24.5 percent around the 22.5 percent point estimate.

These ties connect judges to a substantial segment of the legal market. The 81 connected judges sitting in 2024 were linked to 133 partner-relatives at 118 firms. Extending the matching procedure to all appellate judges observed in the data identifies 529 partner-relatives at 410 of São Paulo's 8,339 registered law firms (4.9%). Connected firms are also disproportionately active: they constitute 3.8% of firms with any appellate caseload but handle 7.4% of the 2010–2024 appellate caseload. That is, their share of appellate litigation is nearly twice their representation among litigating firms.[11]

[11] SI Section B reports the sample-to-market comparison and the distribution of connections across appointment cohorts.

5. Empirical strategy

5.1. Firm-level analysis

Given firms i and calendar years t, let G_i be the year in which firm i gets treated (a relative of a partner is appointed as an appellate judge). For unconnected (control) firms, $G_i = \infty$. Let $D_{it} = 1[t \geq G_i]$ be an absorbing treatment—it records whether the firm has ever experienced a partner-relative's appointment and remains one even after the judge retires or leaves the bench. Y_{it} is the observed outcome, $Y_{it}(g)$ is the firm's potential outcome in year t if the firm was treated in year g, and $Y_{it}(\infty)$ is the potential outcome if the firm remains untreated. The target estimand is the group-time $ATT(g,t)$: the average effect in calendar year t of a relative's appointment in year g, among firms first treated in year g, relative to the counterfactual in which those firms had not yet experienced an appointment.

$$ATT(g,t) \;=\; \mathbb{E}[\, Y_{it}(g) - Y_{it}(\infty) \mid G_i = g \,]$$

This can be estimated with the following differences-in-differences specification:

$$ATT(g,t) = \mathbb{E}[Y_{it} - Y_{i,g-1} \mid G_i = g] - \mathbb{E}[Y_{it} - Y_{i,g-1} \mid i \in C_{g,t}]\,,$$

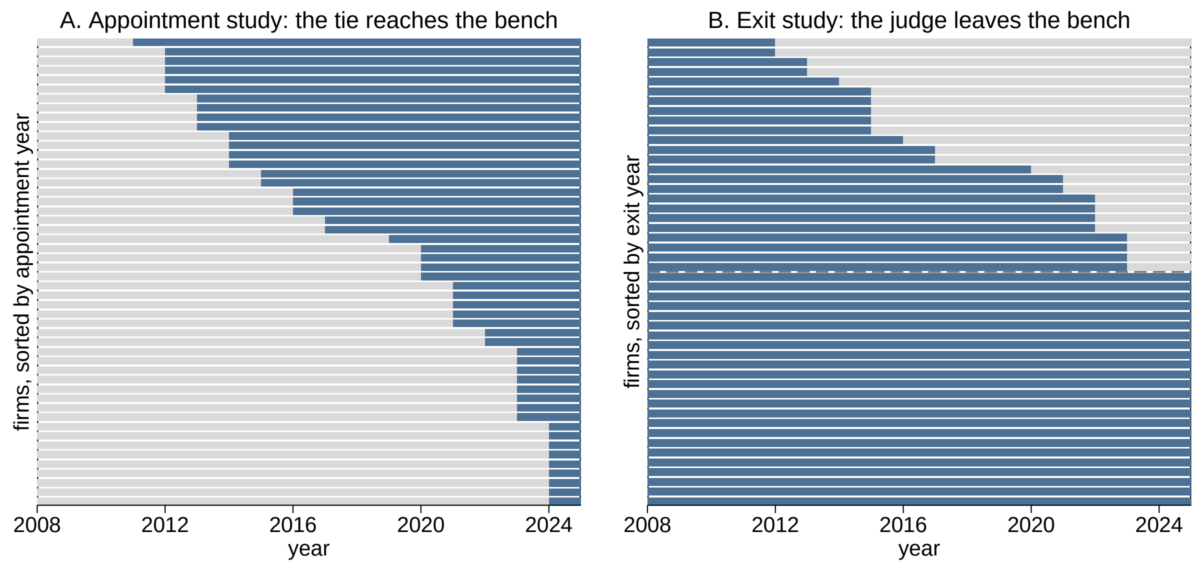
where $C_{g,t}$ denotes the firms eligible to serve as controls for cohort g in year t. This compares changes in the outcome from the last pretreatment year $g-1$ to year t for cohort g, with changes in the outcome for a comparison group (of not-yet-treated or never-treated firms). The comparison identifies the ATT under two assumptions: absent the appointment, treated firms' outcomes would have followed the control firms' trends, and firm behavior does not change in anticipation of the appointment. The event study coefficient estimates the effect at time e, a weighted average of the treatment effects for all cohorts that can be observed e years before or after treatment.

$$ATT(e) = \sum_{g \in \mathcal{G}_e} w_{g,e}\, ATT(g, g+e), \qquad e = t - g, \qquad \sum_{g \in \mathcal{G}_e} w_{g,e} = 1.$$

My preferred specification uses not-yet-treated connected firms as controls. This design holds fixed the presence of a judicial family connection and identifies the effect from variation in the timing of the appointment. A complementary design replaces the timing-only comparison with matched never-treated controls. The donor pool starts from the 8,339 registered firms and removes every firm with any connection or surname overlap with a judge. Each treated firm is then matched to its five nearest neighbors on four pre-appointment covariates—mean caseload, caseload trend, partner count, and firm age—with a caliper discarding the worst-matched decile of pairs; each control serves a single treated firm. SI Section C reports covariate balance, the two designs' event studies overlaid, and the stability of the matched estimate across neighbor counts and calipers. I report three estimators: Callaway and Sant'Anna (CSA), Borusyak, Jaravel and Spiess (BJS), and de Chaisemartin and D'Haultfoeuille (dCdH); the latter two are reported in SI Section C.

A second design examines what happens when the connected judge leaves the bench. I use connected firms for which the judge's appointment predates the observation window, so the exit is the only judicial event observed in the panel. I define exit as the last year between 2011 and 2022 in which the judge issued at least 50 decisions as reporting judge. For every exit I can verify against court biographical records, the dates agree within one year. The primary sample requires the kinship tie to predate the panel—the judge's relative was already a partner by 2009—and compares 24 exiting firms with 24 connected firms whose judge still decided cases in 2023–2024. A broader sample requires only that the tie predate the exit and includes 42 exiting and 58 comparison firms. The appointment and exit designs share no treated firms. Figure 1 shows the treatment timing in both research designs.

Figure 1: Treatment timing in the appointment and exit studies.



Notes: Each panel plots firms as rows and years as columns. Navy marks the years in which a firm's relative serves on the bench; gray marks all other years. Panel A shows the appointment study: the 50 analysis firms, sorted by appointment year. Each row starts gray and turns navy when the relative reaches the bench. The color changes at thirteen different years between 2011 and 2024, so the navy edge forms a descending staircase. This staircase provides the identifying variation: at any date, the still-gray firms serve as controls for the already-navy firms. Panel B shows the exit study. Every firm enters the panel already navy: the relative was appointed before 2008, so the appointment is not observed in this window. The top 24 rows are the exit firms, sorted by exit year. Each row starts navy and turns gray when the judge leaves, at ten different years between 2011 and 2022—the same staircase, reversed. The 24 rows below the dashed line remain navy through 2024: their judges still decide cases and serve as the comparison group. All firms in both panels have a family tie to a judge; the comparison is never between connected and unconnected firms. SI Section F details the exit design.

5.2. Case-level analysis

For the case-level analysis, I estimate the following equation:

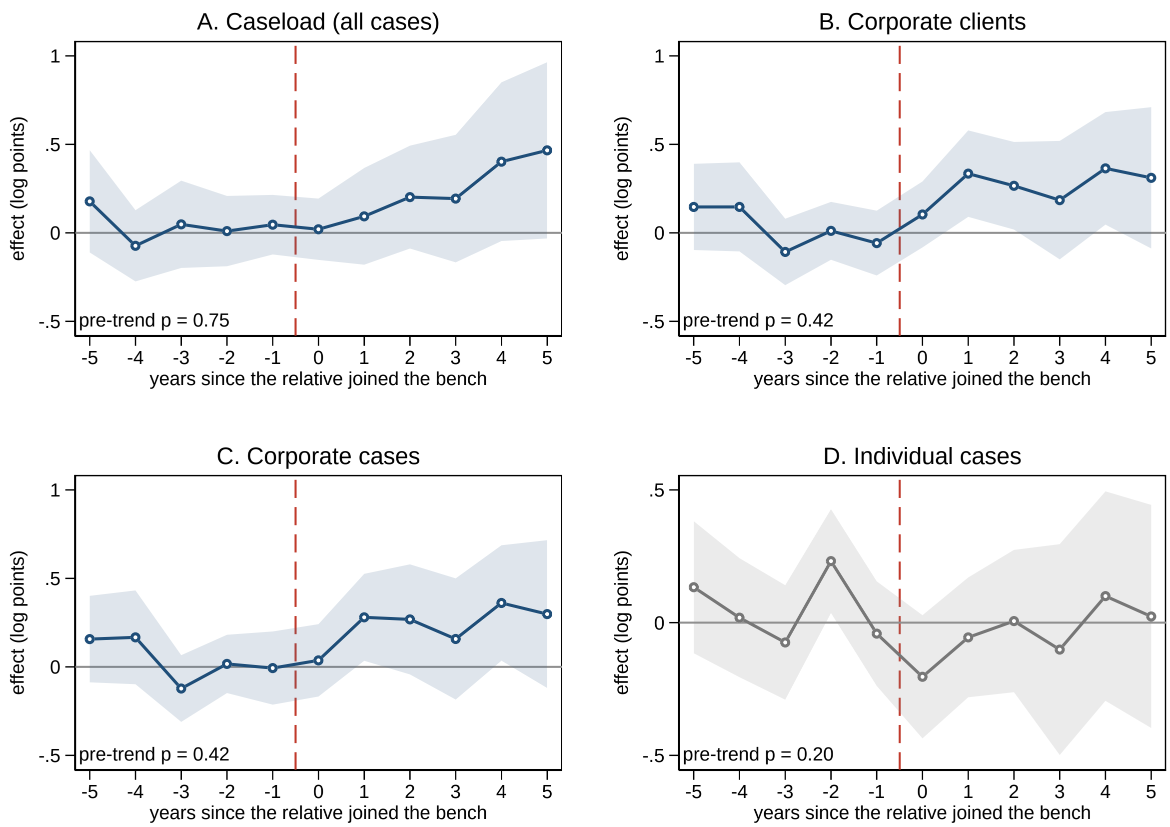
$$\text{won}_{\ell c} \;=\; \beta\,\text{Connected}_{\ell} \;+\; \lambda\,\text{SurnameOnly}_{\ell} \;+\; \alpha_{r(\ell,c)} \;+\; \gamma_{k(c)\times t(c)} \;+\; \delta_{p(c)} \;+\; \varepsilon_{\ell c}, \qquad (1)$$

where $\text{won}_{\ell c}$ equals one if the side represented by lawyer ℓ in case c prevails. Connected_ℓ equals one if ℓ is a matched partner-relative of an appellate judge other than the deciding judge, regardless of when that judge was appointed. Section 6 splits the indicator by the timing of the appointment. SurnameOnly_ℓ equals one for an unverified rare-surname match. The specification includes procedural-role fixed effects, $\alpha_{r(\ell,c)}$, distinguishing the appellant and appellee sides; chamber-by-year fixed effects, $\gamma_{k(c)\times t(c)}$; and case-class fixed effects, $\delta_{p(c)}$. Standard errors are clustered by chamber. I estimate Equation (1) separately for lottery-assigned cases, non-randomly assigned cases, and the full sample. An indicator for the deciding judge being lawyer ℓ's relative is omitted because it is zero in the estimation sample, consistent with the court's recusal requirement.

6. Results

6.1. Appointments increase the firm's business

Figure 2: Dynamic ATT: firm-level outcomes.



Notes: Callaway–Sant'Anna event-study estimates on the 50 connected firms that were connected at the appointment and treated in 2011–2024 (SI Section E); the comparison group is connected firms whose relative has not yet been appointed, so identification comes from the timing of appointments among firms that all have a family tie to a future judge. Outcomes are $\ln(1 + \text{count})$. Solid lines show the effect at each year relative to the appointment; shaded bands are 95% confidence intervals; the red dashed line marks the appointment (*posse*). Each panel reports the p-value of a joint test that the five pre-appointment coefficients are zero. All 50 treated firms (37 appointing judges) contribute at $e = 0$; the count falls to 22 firms (20 judges) at $e = +5$, because late cohorts are observed for short post-periods. Panels C and D split the caseload by client type: corporate cases are those whose client is a company; individual cases, those whose client is a person. SI Section C reports the same estimates under alternative estimators and under a matched never-treated comparison.

I define a case as a distinct TJSP case number appearing in the electronic gazette (DJE, 2008–2024) in which a registered partner of the firm appears as counsel. Each firm–case pair is counted only once per year. The client is the party represented by the firm's lawyer, as identified from the DJE's structured headers. I classify clients as corporate when the party name contains a legal-entity marker—such as *LTDA*, *S.A.*, *EIRELI*, *banco*, *seguradora*, *construtora*, or *participações*. Government entities and natural persons are classified separately. The corporate-client outcome counts distinct corporate client names within each firm-year and therefore captures client relationships rather than case volume.

Figure 2 reports Callaway and Sant'Anna event-study estimates for four firm-level outcomes over event years $e = -5, \ldots, +5$, using 50 treated firms. The comparison group consists of connected firms whose relative has not yet been appointed to the court. The design therefore identifies what changes when a relative reaches the bench, holding fixed the firm's preexisting family connection to a future judge. All outcomes are measured as $\ln(1 + \text{count})$. Shaded bands show 95% confidence intervals, and $e = 0$ denotes the year of appointment (*posse*).

Panel A shows that caseload increases gradually following appointment. The pretreatment coefficients are close to zero, and a joint test cannot reject parallel pretrends ($p = 0.75$). A linear trend fitted across the post-appointment estimates is 0.093 log points per year ($p = 0.051$), and the average effect is 0.377 log points larger in later than in earlier post-appointment years ($p = 0.041$). The effect reaches 0.466 log points by year five ($\text{SE} = 0.254$). However, the pooled post-treatment ATT is positive but imprecise at 0.287 log points ($\text{SE} = 0.191$, $p = 0.13$). The matched estimate is 0.163 log points ($\text{SE} = 0.153$, $p = 0.29$).

Panel B shows a sharper increase in the number of distinct corporate clients the firm represents in the appellate record. The pretreatment coefficients are close to zero, and a joint test cannot reject parallel pretrends ($p = 0.42$). The effect is 0.104 log points in the appointment year ($\text{SE} = 0.095$), rises to 0.335 one year later ($\text{SE} = 0.125$), and then remains flat. The pooled ATT is 0.341 log points ($\text{SE} = 0.147$, $p = 0.020$). This is equivalent to roughly 2.18 additional corporate clients per year (SE $= 0.881$, $p = 0.013$; 1.66 in the matched design, SE $= 0.678$), relative to a pre-appointment mean of 3.4 clients. A Poisson pseudo-maximum-likelihood (PPML) estimate of the proportional change is 19 percent (SE $= 0.113$ log points) and imprecise, because the count model weights the largest firms, where proportional growth is smaller. Overall, the appointment roughly doubles the corporate clientele of the typical connected firm—one serving about two corporate clients a year.[12] A randomization test that reassigns appointment years among firms whose judges were appointed in the same period and through the same track gives $p = 0.030$ (SI Section C).

To assess whether the increase in caseload reflects new demand rather than continued activity in existing cases, I classify each firm–case as new in the year it first appears in the gazette and as continuing in subsequent years. New and continuing cases sum to total caseload by construction; because the data begin in 2008, cases classified as new from 2010 onward have at least a two-year burn-in period. The estimate is 0.295 log points for new cases (SE $= 0.195$) and 0.245 for new cases from clients the firm had not previously represented (SE $= 0.175$). Neither estimate is statistically distinguishable from zero. The corresponding estimates for continuing cases are 0.203 log points in the timing design (SE $= 0.159$) and 0.219 in the matched design (SE $= 0.113$, $p = 0.054$).

Panels C and D show that the increase is driven by corporate—not individual—cases. For corporate cases, the joint pretrend test has a p-value of 0.42. The effect reaches 0.280 log points one year after appointment (SE $= 0.125$), and the pooled ATT is 0.309 log points (SE $= 0.156$, $p = 0.048$). This is equivalent to roughly 2 additional corporate cases per year (SE $= 0.965$; 1.40 in the matched design), relative to a pre-appointment mean of 3.6—that is, about one case per additional client. The matched estimate is 0.210 log points (SE $= 0.122$, $p = 0.085$).

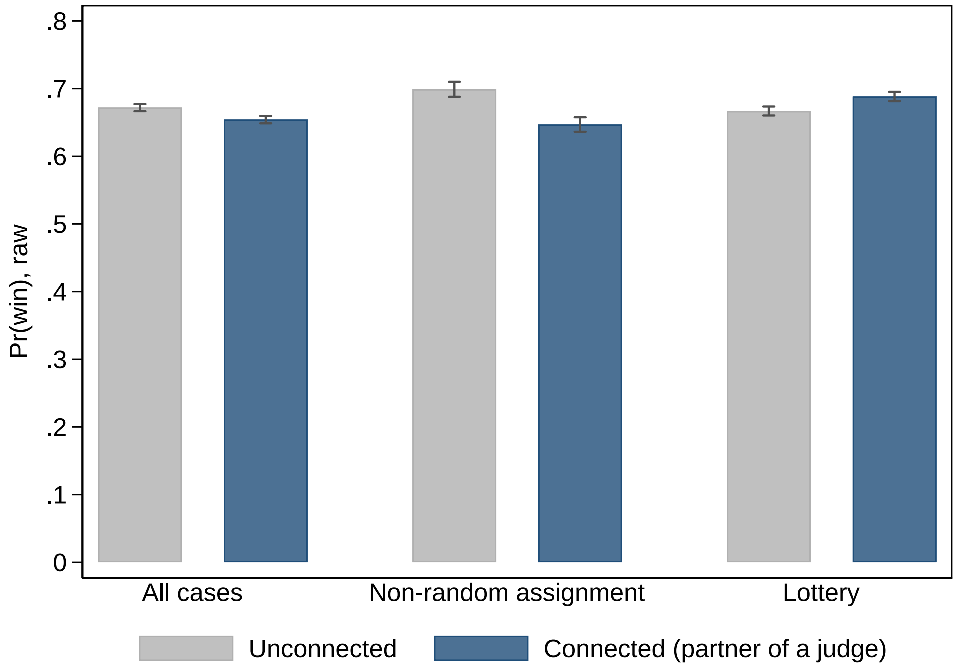
[12] A split by baseline size shows that among the 25 firms at or below the median of pre-appointment corporate clients, the appointment adds 1.86 clients per year (SE $= 0.69$, $p = 0.007$; 0.611 log points, $p = 0.001$), while among above-median firms the proportional effect is near zero (0.051 log points, SE $= 0.190$). The three magnitudes describe the same pattern: the average log effect (0.341), the average level effect (2.18 clients), and the size-weighted proportional effect (19 percent), which is small because the largest firms grow little in proportion.

6.2. Connected lawyers win more when judges are assigned by lottery

Figure 3 plots the raw share of cases won by the side represented by connected and unconnected counsel, without controls or model adjustment, for all cases and separately by assignment regime. Across all cases, connected counsel have a lower win rate than unconnected counsel (0.654 versus 0.672), a gap of 1.8 percentage points. Among cases assigned non-randomly, this gap widens to 5.2 percentage points (0.647 versus 0.699). The pattern reverses in cases assigned by lottery: connected counsel win 68.8% of cases, compared with 66.7% for unconnected counsel, a difference of 2.1 percentage points ($\text{SE} = 0.007$).

The contrast between assignment regimes suggests selection: connected firms may take harder, more complex cases. The negative raw gap likely reflects case portfolio, not unfavorable treatment of connected counsel. When the judge is drawn by lot, judge selection is removed from the comparison and connected counsel instead win approximately 2 percentage points more. This inversion also cuts against assignment steering: if connected firms benefited from manipulating judicial assignment, they should perform better precisely where assignment is discretionary, yet the opposite pattern appears.

Figure 3: Raw probability of winning: connected vs. unconnected counsel.



Notes: The sample comprises TJSP appellate decisions published in the electronic gazette (DJE, 2008–2024) and matched to e-SAJ case records, which report the assignment mechanism: 59,703 case–lawyer observations across 34,456 cases with a codable outcome. Bars show the raw share of cases won by the side the lawyer represents; whiskers are 95% confidence intervals. Connected = the lawyer is a matched partner-relative of a sitting TJSP judge other than the deciding judge; unconnected = lawyers with no matched partner tie and no surname match. Lottery = judge assigned by random draw (*sorteio*); non-random = the case is routed to a specific judge, either because a related case was already before them (*prevenção*) or by specialized subject-matter jurisdiction (*competência exclusiva*).

Table 2: Connection premium in randomly and non-randomly assigned cases.

	(1) Lottery	(2) Non-random	(3) All
Partner of a judge	0.0287***	-0.0452***	-0.0046
	(0.0071)	(0.0156)	(0.0069)
Observations	34,786	12,791	56,331
Chamber clusters	96	91	96

Notes: The dependent variable is an indicator equal to one if the side represented by the lawyer prevails. All specifications include role, chamber-by-year, and case-class fixed effects. Standard errors, clustered by chamber, in parentheses. Lottery = judge assigned by random draw (*sorteio*); non-random = the case is routed to a specific judge, either because a related case was already before them (*prevenção*) or by specialized subject-matter jurisdiction. All specifications include an indicator for lawyers with an unverified rare-surname match to a judge. Because connection is assigned at the lawyer level, inference must also allow for dependence within lawyers across chambers: clustering by lawyer gives a standard error of 0.013 for the lottery premium ($p = 0.028$), and two-way clustering by chamber and lawyer gives 0.013 ($p = 0.026$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports estimates of Equation (1). Among cases assigned by lottery, the side represented by a judge's partner-relative is 2.87 percentage points more likely to prevail ($p < 0.01$). In contrast, connected counsel are 4.52 percentage points less likely to prevail among non-randomly assigned cases ($p < 0.01$). This negative estimate is consistent with the raw pattern in Figure 3: routed cases appear to differ systematically in difficulty and composition.

The lottery estimate is the most credible comparison because random assignment removes endogenous judge selection. It provides no evidence that the connection operates through assignment steering; connected counsel perform better precisely when the deciding judge is drawn by lot. The estimate should be interpreted as a conditional connection premium, not an appointment effect.

6.3. Judicial appointments do not cause favoritism in case outcomes

I examine two mechanisms that could explain these patterns. The first is favoritism: firms grow because the connection gives their clients an advantage in court, such as favorable outcomes or faster proceedings. The second runs through third-party behavior: firms grow because the appointment changes how prospective clients act toward the firm, even if the connection creates no advantage in court.

The most direct form of favoritism—the relative adjudicating the firm's own case—does not occur. In the estimation sample, the deciding judge is the lawyer's relative in none of the 60,070 case–lawyer observations, including the 36,387 assigned by lottery. The recusal rule binds in practice: any favoritism must be indirect, operating through the relative's colleagues.

To test whether appointment creates such an indirect advantage in court, I re-estimate equation (1) with separate indicators for cases decided before and at or after the earliest appointment among a lawyer's judge-relatives. Lawyers for whom no appointment date is available enter through a separate indicator. All other terms and fixed effects remain as in equation (1). I cluster standard errors by chamber. I test whether $(\beta_{\text{pre}} = \beta_{\text{post}})$. Favoritism predicts that appointment increases the win-rate advantage, so $\beta_{\text{post}} > \beta_{\text{pre}}$. If the appointment instead changes third-party behavior without changing treatment in court, an advantage in case outcomes—if any—should exist before the appointment and should not increase afterward.

$$\begin{aligned} \text{won}_{\ell c} \;=\; & \beta_{\text{pre}}\,\text{Connected}_{\ell} \times \mathbf{1}\{t(c) < g_{\ell}\} \;+\; \beta_{\text{post}}\,\text{Connected}_{\ell} \times \mathbf{1}\{t(c) \geq g_{\ell}\} \\ & +\; \beta_{u}\,\text{Connected}_{\ell}^{\text{undated}} \;+\; \lambda\,\text{SurnameOnly}_{\ell} \;+\; \alpha_{r(\ell,c)} \;+\; \gamma_{k(c)\times t(c)} \;+\; \delta_{p(c)} \;+\; \varepsilon_{\ell c} \end{aligned} \tag{2}$$

Table 3 tests whether the probability of winning increases after a relative is appointed to the bench or whether the advantage predates the appointment. If having a relative judge creates favorable treatment, the winning premium should increase after the appointment. This is not the case: the observed win premium does not increase after a law firm partner's relative is appointed as a judge.[13] Additional outcome margins and robustness of this comparison appear in SI Section D. [14]

[13] One-sided tests reject an appointment-induced increase as large as half the pre-existing premium ($p = 0.003$) and as large as the full premium ($p < 0.001$).

[14] One margin does change at the appointment: partial relief rises by two percentage points on an 11 percent base. However, the change runs against the connected side. Connected counsel almost always represent the party defending a lower-court victory (98 percent of observations), and for that side partial relief means the opponent's appeal partially

Table 3: Win premium before and after the relative's appointment.

	(1) Lottery	(2) All
Connected × before appointment	0.0617***	0.0354*
	(0.0229)	(0.0196)
Connected × after appointment	0.0300***	-0.0062
	(0.0072)	(0.0069)
p: before = after	0.164	0.033
Observations	34,786	56,331
Chamber clusters	96	96

Notes: Lottery-assigned cases in column (1); all cases in column (2). The connected-lawyer indicator is split by whether the case was decided before or after the earliest appointment year among the lawyer's judge-relatives; connected lawyers whose relative's appointment date is unavailable enter through a separate indicator. Specifications and fixed effects as in Table 2; standard errors clustered by chamber. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.4. Mechanisms

Demand can increase because third parties change their behavior toward the firm even without direct favoritism. I present three pieces of evidence consistent with this interpretation. First, connected law firms already have a higher win rate before the judge's appointment.

Second, if increased caseload reflects a response to the appointment itself, it should rise shortly after it; if it instead reflects the firm's increased capacity, it should rise gradually.[15] Table 4 shows distinct timing patterns across outcomes. Corporate clients increase one year after the appointment and then plateau, consistent with an immediate response to the appointment. Overall caseload grows steadily, and repeat clients increase after two years. The early corporate response is consistent with sophisticated transactors moving first.

succeeds: among connected appellees, partial clawbacks of their clients' victories rise by 4.0 percentage points after the appointment ($p = 0.003$; Table D.2). Favoritism predicts the opposite. This pattern is more consistent with increased demand than with favoritism: after the appointment, the firm defends more contestable lower-court victories.

[15] The modest and marginal effect on number of partners also runs against the capacity interpretation.

Table 4: Timing of the demand response.

	Caseload	Corporate clients
Effect in the appointment year	0.021 (0.088)	0.104 (0.095)
Effect one year after	0.093 (0.139)	0.335*** (0.125)
Per-year change, years 1–5	0.093* (0.048)	-0.006 (0.044)

Notes: Each column reports linear combinations of the Callaway–Sant'Anna event-study coefficients from Figure 2 (comparison group: not-yet-treated connected firms; outcomes in $\ln(1 + \text{count})$); standard errors from the joint covariance of the event-study estimates in parentheses. e denotes years since the relative's appointment. The linear trend is the per-year slope fitted across the coefficients for $e = 1$ through $e = 5$; a positive value indicates that the effect continues to grow after the first year. Results are unchanged using a nonparametric comparison of the average effect in years 2–5 against years 0–1: caseload $+0.317$ ($p = 0.009$), corporate clients $+0.145$ ($p = 0.160$), repeat clients $+0.288$ ($p = 0.034$). The absence of a detectable trend for corporate clients does not establish that the path is exactly flat. The delayed response of repeat clients is partly mechanical: a client acquired at the appointment can be recorded as a repeat client only in a subsequent year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, I examine what happens when the connected judge leaves the bench (Figure 1, Panel B). Neither caseload nor corporate clients decline after the judge leaves (Figure 4; Table F.1). In the primary sample, the post-exit estimates are 0.121 log points for caseload (SE $= 0.191$) and -0.066 for corporate clients (SE $= 0.183$). In the broader sample, the corresponding estimates are -0.016 (SE $= 0.129$) and -0.117 (SE $= 0.143$). [16] The event-study estimates are flat before and after exit through year five, and none of the 20 estimates across five estimators is statistically significant. Joint tests generally do not reject parallel pretrends ($p = 0.65$ and $p = 0.30$). The exception is corporate clients in the primary sample ($p = 0.089$), for which I place more weight on the broader sample.[17]

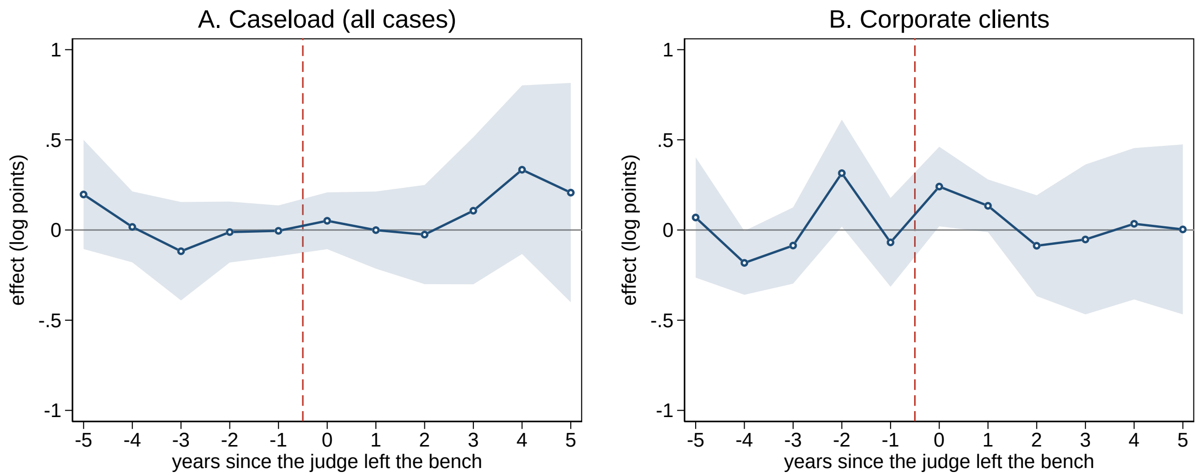
These results suggest that business accumulated under the tie remains after the judge leaves. Because these judges had served a median of fifteen years before exiting and the ties predate the panel, the design captures the stock of business accumulated under mature ties. However, it cannot

[16] One-sided tests against a decline the size of the appointment effect reject it for caseload ($p = 0.016$ in the primary sample, 0.018 in the broader one) but fall just short for corporate clients ($p = 0.067$ and 0.059).

[17] SI Section F reports results across alternative estimators, shifted exit dates, documented retirements only, and leave-one-judge-out tests.

determine whether attracting new demand requires an active connection (only six firms identify that comparison). This persistence is less consistent with clients buying only ongoing access to a sitting judge.

Figure 4: Business around the judge's exit.



Notes: Callaway–Sant'Anna event-time paths with 95 percent confidence intervals for the sample whose kinship tie predates the panel (24 treated, 24 comparison firms). The dashed line marks the judge's departure; event time 0 is the first full year without the judge on the bench.

7. Judges' relatives are disproportionately represented among law-firm partners

The firm-level results concern the 50 firms whose relative reached the bench inside the observation window, but the tie between judicial families and firm ownership extends beyond them. Applying the same surname rule to the full roster of 588,065 practising lawyers rather than to registered partners shows that judges' relatives are registered as law-firm partners at a rate of 9.0 percent, compared with 5.7 percent among other lawyers—a ratio of 1.58 that holds within every quintile of practice intensity and is widest among the most active lawyers (Table 5). This pattern is descriptive: relatives' partnership entries do not immediately follow appointments at any tested time horizon;

the timing is consistent with the next generation entering the legal profession. Thus, the same families produce judges and firm owners, suggesting elite reproduction among families that hold a competitive edge on both sides of the legal market.

Table 5: Judges' relatives are more likely to own the firms they work in.

	Share who are law-firm partners			Lawyers	
	Judges' relatives	Other lawyers	Ratio	Judges' relatives	Median cases
All lawyers	9.0%	5.7%	1.58	5,493	11
By quintile of career caseload					
Quintile 1	3.8%	2.7%	1.41	1,488	2
Quintile 2	5.6%	3.4%	1.66	750	5
Quintile 3	7.0%	4.4%	1.60	1,097	11
Quintile 4	7.2%	5.9%	1.22	1,052	44
Quintile 5	22.1%	12.6%	1.76	1,106	254

Notes: The population is the 588,065 lawyers who appear in the appellate gazette between 2010 and 2024. A lawyer counts as a judge's relative under the surname rule of SI Section A, applied to the full roster of practising lawyers rather than to registered partners; lawyers who are themselves the judge are excluded. Partnership is measured from the federal registry of firm partners, which records current partners only. Partners litigate more than other lawyers, so the lower panel holds practice intensity fixed by sorting lawyers into quintiles of the number of cases they handle across the period. A chi-squared test of independence between kinship and partnership rejects at $p = 1.5 \times 10^{-25}$. The table describes an association: it cannot distinguish relatives brought into ownership after an appointment from families who enter the profession and rise within it together.

8. Conclusion

This article studied whether social ties to public officials can generate private returns. By combining court records, a registry of law-firm partners, payroll data, and information about judges in the world's largest appellate court, I showed that the appointment of a partner's relative increases the firm's corporate business. Connected firms gain about two corporate clients per year—roughly doubling the typical firm's corporate clientele—and the number of corporate cases also rises. Overall caseload also increases, although the estimate is imprecise.

I provided evidence that this increase in demand is not driven by favoritism in case outcomes. In the estimation sample, the deciding judge is never the lawyer's relative. Connected lawyers win more often in randomly assigned cases even before their relative's appointment, but their win rate does not increase afterward; nor do case duration, dissent, or disposal change. I also found no evidence that business accumulated under mature ties declines after the judge leaves, which is inconsistent with clients buying only ongoing access to a sitting judge. Because the exit design studies mature ties, it cannot determine whether attracting new demand requires an active connection. Together, the results are consistent with an audience channel: the appointment changes the behavior of third parties, increasing demand for the firm, without conferring an advantage on the firm's clients in court.

These results reveal a limit of integrity rules: they regulate officials, not the audiences that value proximity to public office. Recusal can preserve impartial adjudication, but it cannot prevent clients from attaching value to a judge's family ties. Formal rules can therefore constrain favoritism without eliminating the private value of public connections.

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Supplementary Information

A Judge in the Family: The Value of Kinship Ties in Legal Markets

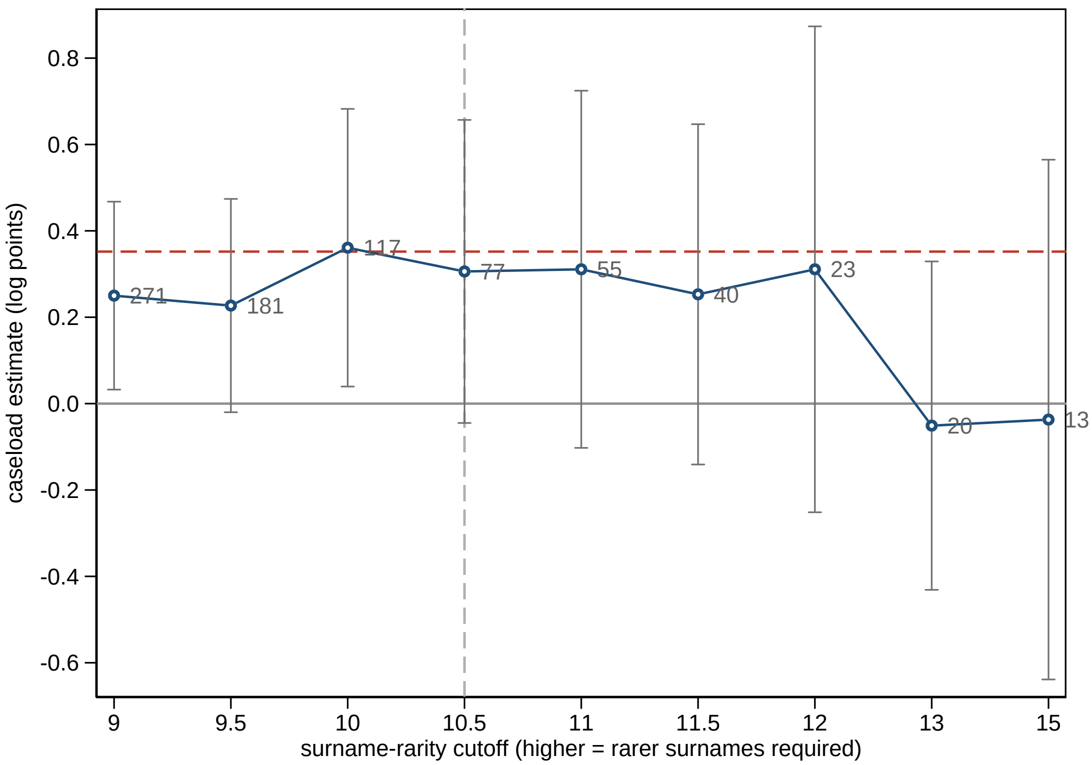
Contents

A Identifying kinship ties between judges and law firms . . . A2
B Additional firm-level results . . . A7
C Robustness of the firm-level event study . . . A10
D Additional case-level results . . . A15
E Connection at the time of appointment . . . A18
F When the judge leaves the bench . . . A21
G The appointment as a public event . . . A24
H Data construction and variable definitions . . . A25

A. Identifying kinship ties between judges and law firms

Matching rule. I identify kinship ties by comparing the 1,452 appellate judges against the 35,089 registered partners of São Paulo's 8,339 law firms—partners specifically, because a relative who co-owns the firm is visible in the tax registry while a salaried relative is not. Names are first decomposed into standardized surname units: Portuguese connectors are dropped, foreign prefixes stay attached, generational endings (*Filho, Neto, Júnior*) are set aside, and spelling is normalized so that variants such as Mattos and Matos remain comparable. Each unit receives a rarity score, $\ln(588{,}065/f)$, where f is the number of lawyers in the gazette roster carrying it. A partner is classified as a judge's relative if any of five rules holds: (1) identical full name up to a generational ending; (2) one shared surname carried by at most 16 lawyers (roughly one in 37,000); (3) two shared surnames with rarity scores summing to at least 11 (a joint coincidence probability below 2 in 100,000); (4) two shared surnames summing to at least 10.5; or (5) one shared surname carried by 17–22 lawyers that also appears in the firm's registered name. Firms where the judge is himself the registered partner (46 cases) are excluded. Rules 1–3 define the strict set (352 firms); adding rules 4–5 gives the preferred set of 410, of which 79 are connected to a judge appointed in 2011–2024. Those 79 are the candidate set this appendix audits; the event study uses the 50 of them that were connected at the appointment (SI Section E). A hand reading of the extracted surnames agreed with the automated extraction in 95.6 percent of cases, and the caseload estimate is flat (+0.25 to +0.36) across rarity cutoffs from 9 to 12, collapsing only above 13, where 13–20 firms remain (Figure A.1).

Figure A.1: The caseload estimate across surname-rarity cutoffs.



Notes: Each point re-runs the full matching procedure and the caseload event study with the surname-rarity threshold set to the value on the horizontal axis, from permissive (9) to demanding (15); the number beside each point is the count of treated firms that definition yields. Bars are 95% confidence intervals. The vertical dashed line marks the threshold used in the preferred definition; the horizontal dashed line marks the preferred-definition estimate ($+0.35$, 79 firms), which combines the five matching rules rather than a single threshold. Every point on this ladder, including that reference line, comes from the matched never-treated comparison, so the relevant benchmark is the matched estimate and not the larger timing-only estimate; the ladder varies the matching rule, and all its points are estimated on all treated firms a given threshold yields, before the partnership-date restriction of SI Section E. The estimate stays between $+0.23$ and $+0.36$ across the range 9–12; at 13 and above, 13–20 firms remain and the intervals widen accordingly. Matching-rule sensitivity for the analysis sample and the corporate-client outcome appears in the documentary-verification paragraph above: the strict rules alone and the drop-refuted sample leave that estimate at $+0.34$ and $+0.33$.

Documentary verification. Verification proceeds at the (firm, judge)-pair level. The 79 treated firms generate 122 pairs across all appointment eras; the audit covers 87 pairs whose judge was appointed in or near the study window. Two AI research assistants, queried independently under identical instructions, searched for published evidence of each claimed relationship; every verdict required a verbatim quotation and a working link, and the shared surname itself—or the firm bearing it—did not count as evidence. Their verdicts never conflicted. The search confirmed or found strong circumstantial evidence for 32 of 79 firms (41 percent), positively refuted 5, and could not settle 42. The refutations arose from given names mistaken for surnames, homonymous families, a surname acquired by marriage, and one judge born in 1902. Five proven errors out of 79 firms establish a floor of 6.3 percent; among the 37 cases the records could settle, the false-positive rate is 13.5 percent. The audit covers the candidate universe that the matching rule defines—79 firms—of which the analysis sample retains 50 (SI Section E); among those 50, the verdicts are 5 confirmed, 13 with strong circumstantial evidence, 30 unsettled, and 2 refuted. Removing the two refuted firms changes the corporate-client estimate from $+0.341$ to $+0.328$ ($p = 0.030$) and the caseload estimate from $+0.287$ to $+0.302$; restricting to the conservative matching rules alone (40 of the 50 firms) leaves the corporate-client estimate at $+0.339$ ($p = 0.042$). Table A.1 traces the counts from the judicial roster to the 50-firm estimation sample, and Table A.2 traces the case counts from the gazette panel to the case-level estimation samples.

Table A.1: From the judicial roster to the estimation sample.

	Count
Appellate judges compared against the partner registry	1,452
Registered partners of São Paulo law firms	35,089
Law firms in the registry	8,339
Matching	
Firms matched to at least one judge	410
Appointment dates	
Matched firms with a usable appointment date	142
Judge appointed before 2011 (always treated, dropped)	63
Treated firms (appointment 2011–2024)	79
with one in-window judge	73
with two or three in-window judges	6
their firm–judge pairs, counting judges of every era	122
matched to more than one judge, any era	28
Documentary verification (firm–judge pairs)	
Pairs searched	87
Firms with confirming or strong circumstantial evidence	32
Firms positively refuted	5
Firms the records could not settle	42
Connection at the appointment	
Relative was a partner at the appointment	52
Relative became a partner only afterward (excluded)	27
Another relative already observed deciding cases (excluded)	2
Analysis sample	50

Notes: Each block reports the count entering the next stage. Firm–judge pairs are counted for the 79 treated firms across all appointment eras, so a treated firm matched to a judge appointed in 1995 and another appointed in 2016 contributes two pairs; the verification stage searched the pairs whose judge was appointed in or near the study window, which is why its count (87) is smaller than the all-era total (122) and larger than the 86 pairs implied by the in-window counts alone—one verified pair involves a judge appointed in 2025, after the panel ends. Verification verdicts are recorded at the firm level: a firm counts as refuted only when every one of its pairs is refuted. The final block applies the partnership-date test of SI Section E.

Table A.2: From the gazette panel to the case-level estimation samples.

	Observations
Case–lawyer observations in the gazette panel	8,849,474
Cases sent to the e-SAJ record lookup	37,643
retrieved	34,950
court-restricted, permanently unavailable	2,692
lookup unresolved	1
Assignment-route sample	
Case–lawyer observations matched to an e-SAJ record	60,070
distinct cases	34,677
Assigned by lottery	36,387
Routed by prevention or exclusive competence	14,292
Route not parsable from the record	9,391
Estimation samples	
Raw win-rate comparison (Figure 2)	59,703
Premium by assignment route (Table 2)	56,331
Lottery premium, before and after (Table 3)	34,786

Notes: The gazette panel covers every case–lawyer observation with a coded disposition, 2008–2024. The assignment route is recorded only in the e-SAJ case record, which must be retrieved case by case; cases the court restricts cannot be retrieved at all, and a further group of records does not state the route in a parsable form. The estimation samples differ from the merged total because the raw comparison requires a codable outcome, the route regression drops chamber-by-year cells with no variation, and the before-and-after specification requires a dated appointment for the connected lawyer's judge-relative. The baseline before-and-after sample is not restricted to lawyers observed on both sides of the appointment—36 connected lawyers appear before it and 263 after (SI Section D); the restriction to the 28 lawyers observed on both sides is a robustness specification reported there. The three lottery counts sum to the merged total by construction.

Behavioral validation. Recusal itself provides a test that requires no documents and covers every pair: the obligation to withdraw binds only if the tie is real. Matched relative-partners made 33,320 appellate appearances. For the 22,518 appearances with an identified chamber-year risk set, if every match were spurious, assignment would have placed 28.6 appearances before the matched judge using empirical assignment shares, or 44.4 under equal assignment within the cell. Only three occurred. These benchmarks imply false-match shares of 10.5 percent (95 percent confidence interval: 2.2 to 30.6) and 6.8 percent (1.4 to 19.8), respectively, consistent with the documentary audit. Table A.3 reports the comparison. The three appearances occur in the full gazette panel; none carries a coded assignment route, so the estimation sample of Equation (1) contains no case decided by the lawyer's own relative.

Table A.3: Appearances before the matched judge against the assignment risk set.

	Empirical assignment shares	Uniform among eligible judges
Expected appearances before the matched judge	28.6	44.4
Observed	3	3
Implied share of matches that are false	10.5%	6.8%
95% confidence interval	[2.2, 30.6]	[1.4, 19.8]

Notes: Cases are assigned within a chamber and year, so the expected number of appearances before the matched judge is computed from that judge's share of the decisions issued in each case's own chamber-year rather than from the court-wide judge count. The first column uses those empirical shares; the second divides equally among the judges deciding in the cell, which is more generous to the null. Both are computed over the 22,518 appearances whose chamber-year cell has an identified deciding judge. The implied false-match share is the observed count divided by the expected count, with an exact Poisson interval. Under the hypothesis that every match is spurious, the probabilities of observing three or fewer appearances are 1.6×10^{-9} and 8.3×10^{-16}, respectively.

Missed relatives. Surname matching is blind by construction to relatives who carry a different name—a son-in-law, or a daughter who took her husband's name. I therefore inverted the search: for a random sample of 80 of the 223 in-window judges matched to no firm, official swearing-in announcements and association death notices—which name family members regardless of surname—yielded documented families for 38 judges, naming 146 relatives, 45 of them by marriage. None of the 111 checkable names is a partner in any São Paulo law firm. (The pipeline recovers all 200 known relative-partners when tested against them, so the zero is informative.) Zero misses among the 38 judges with recoverable family records bounds their undetected-relative rate below roughly 8 percent; the bound extends to the remaining unmatched judges only insofar as judges with recoverable records resemble those without, and 111 of the 146 named relatives could be checked. Since a missed firm sits in the comparison group, any such misses attenuate the estimates toward zero.

B. Additional firm-level results

Table B.1 reports descriptive statistics for the treated firms, their matched controls, and the unconnected firm universe. Table B.2 tracks the share of the firms' cases on which partners of other firms appear as co-counsel. Figure B.1 reports event studies decomposing the caseload effect into new and continuing cases, and Figure B.2 reports event studies for partner count, repeat clients, and employment outcomes.

Table B.1: Descriptive statistics.

	Treated (pre-appointment)	Matched controls (pre-appointment)	Unconnected firms (2010–2024)
Annual caseload	153.8	120.7	111.9
	[72.4]	[67.6]	[48.5]
Registered partners	16.5	8.2	3.4
	[4]	[2]	[2]
Firm age in 2024 (years)	20.4	20.3	15.9
	[22]	[21]	[15]
Corporate clients per year	3.7	4.0	—
	[1.8]	[1.6]	
Individual clients per year	8.1	7.8	—
	[3.2]	[2.7]	
Firms	50	229	7,894

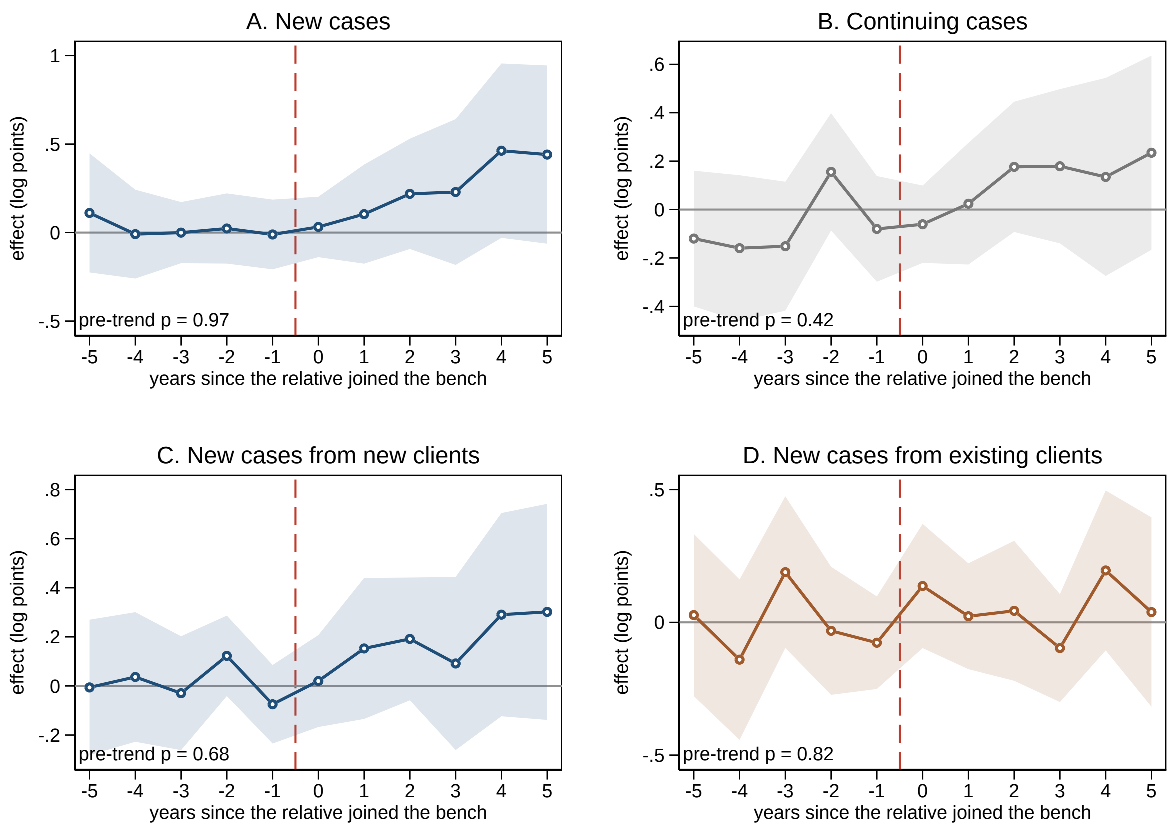
Notes: Firm-level means; medians in brackets. The treated and matched-control columns cover the five years before the (matched) firm's appointment—the matching window; the unconnected column covers all 2010–2024 firm-years outside the connected set and the surname-overlap exclusions. Caseload is the firm's mean distinct appellate cases per year. Partner counts and firm age (years since the earliest entry, as of 2024) come from the partner registry and are defined identically in every column. Client counts require the case-header client link, built only for treated and matched-control firms.

Table B.2: Share of the firm's cases on which another firm's partner appears as co-counsel.

	Partner of another firm on the firm's side	Any non-partner lawyer on the firm's side
Post-appointment ATT (Callaway–Sant'Anna)	0.0053 (0.0166)	0.0164 (0.0206)
Post-appointment, two-way fixed effects	-0.0090 (0.0082)	-0.0110 (0.0157)
Pre-appointment mean of the share	0.032	0.062
Joint test of pre-appointment coefficients (p)	0.491	0.573
Firm-years	1,014	

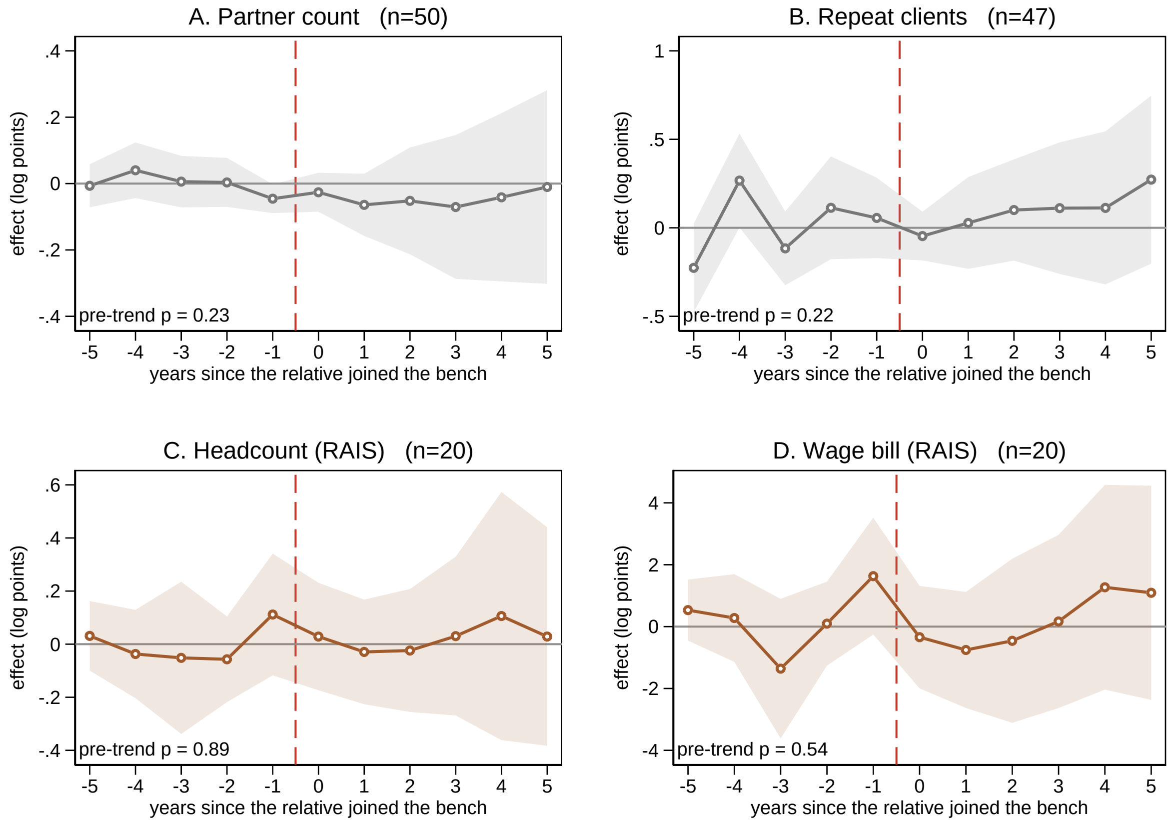
Notes: For each case-side on which a partner of a connected firm appears in the gazette, the other lawyers on that side are classified against the partner registry: partners of a different registered firm (column 1) and lawyers who are not partners of the firm (column 2; the firm's own associates fall in this column). Outcomes are firm-year shares of the firm's cases, in levels. The first row reports pooled post-appointment ATTs from the specification of Figure 2; the second, two-way fixed-effects estimates with standard errors clustered by firm. This measure detects a referral only when the referring firm's partner also enters an appearance on the case, so it bounds the visible-co-counsel form of referral and cannot rule out referrals that leave no trace in the record. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.1: Event studies for new and continuing cases.



Notes: Callaway–Sant'Anna event studies under the timing-only design for new cases, continuing cases, new cases from new clients, and new cases from existing clients, in $\ln(1 + \text{count})$; shaded bands are 95% confidence intervals; the red dashed line marks the appointment; each panel reports the joint test of the five pre-appointment coefficients. The delayed rise of continuing cases reflects new cases aging into continuing ones.

Figure B.2: Additional firm-level outcomes.

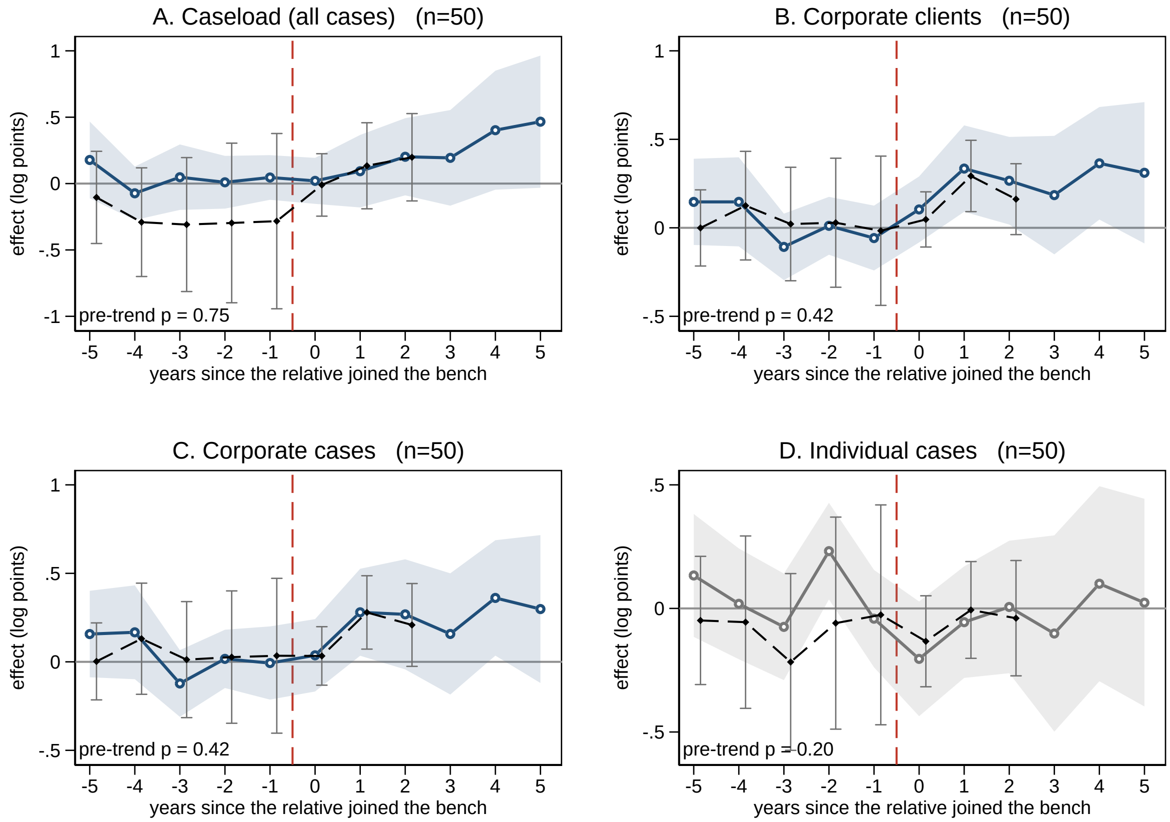


Notes: Callaway–Sant'Anna ATT by event time; shaded bands are 95% confidence intervals; the red dashed line marks the appointment. Comparison group: not-yet-treated connected firms, as in Figure 2. Repeat clients are distinct clients the firm had represented in an earlier year. Employment outcomes cover roughly 20 of the 50 treated firms, because the employer–employee census ends in 2019, and are underpowered by construction.

C. Robustness of the firm-level event study

This SI section probes the robustness of the firm-level estimates. Figure C.1 re-estimates the event studies under an alternative estimator. Table C.1 varies the estimator, treatment dating, sample, and comparison group; its note reports the judge-clustered and randomization-inference results. Table C.2 reports covariate balance in the matched comparison; Figure C.2 overlays the event studies from the two designs, and Figure C.3 varies the pairing.

Figure C.1: Event studies under an alternative estimator.



Notes: The four panels are the outcomes of Figure 2: caseload, corporate clients, corporate cases, and individual cases. Solid lines with shaded bands: Callaway–Sant'Anna estimates with 95% confidence intervals, as in Figure 2. Black dashed lines with capped bars: Borusyak–Jaravel–Spiess imputation estimates with 95% confidence intervals. The imputation estimator is identified only through the second year after the appointment on this sample: with 50 treated firms it reports an insufficient effective sample size at longer horizons, and those horizons are left blank rather than plotted at zero. Where both estimators are identified they agree closely. Comparison group: not-yet-treated connected firms. The red dashed line marks the appointment.

Table C.1: Robustness of the firm-level effects.

	Caseload	Corporate clients	Corporate cases
Panel A: effect five years after the appointment, by estimator			
Callaway–Sant'Anna	0.466* (0.254)	0.311 (0.204)	0.298 (0.213)
Borusyak–Jaravel–Spiess imputation	—	—	—
de Chaisemartin–D'Haultfœuille	0.402* (0.225)	0.365** (0.172)	0.361** (0.175)
Panel B: pooled ATT under alternative specifications			
Benchmark	0.287 (0.191)	0.341** (0.147)	0.309** (0.156)
Appointment shifted back one year	0.325* (0.186)	0.177 (0.137)	0.201 (0.142)
Appointment shifted back two years	0.397* (0.238)	0.090 (0.122)	0.113 (0.125)
First full calendar year as treatment	0.320** (0.157)	0.222* (0.124)	0.212 (0.133)
Firms with a single connected judge	0.282 (0.198)	0.310** (0.148)	0.276* (0.158)
Career-track judges only	0.201 (0.169)	0.291* (0.175)	0.270 (0.183)
Matched never-treated comparison	0.163 (0.153)	0.244** (0.118)	0.210* (0.122)
excluding largest firms	0.151 (0.161)	0.237* (0.125)	0.203 (0.129)
excluding gazette-reinstated firms	0.254 (0.206)	0.236* (0.142)	0.200 (0.152)

Notes: Panel A reports the effect five years after the appointment under three estimators, on the connected-firm panel of Figure 2; the de Chaisemartin–D'Haultfœuille estimate is unchanged when the set of treated firms is held fixed across horizons. The Borusyak–Jaravel–Spiess row is empty because the imputation estimator is identified only through the second post-appointment year on this sample (Figure C.1). Panel B reports pooled post-appointment ATTs from the Callaway–Sant'Anna specification. Shifting the appointment back one or two years reassigns treatment to placebo dates preceding the true appointment; on this sample the shifted caseload estimates are larger than the benchmark rather than attenuated, which the smaller sample and the growing effect path can both produce, so these rows bound anticipation loosely at best. The first-full-calendar-year row codes treatment as beginning in the first complete calendar year after the swearing-in. Firms with a single connected judge (49 of 50) exclude firms whose treatment bundles more than one elevation. Career-track judges only (37 firms) uses the appointment track recorded in the seniority roster, which is stricter than the Curriculum-based coding of Section 3 (41 of 50 firms) because it leaves judges absent from the current roster unclassified. The matched never-treated comparison replaces the timing-only design with the 229 unconnected controls matched to these treated firms; the excluding-largest-firms row drops the largest decile of treated firms by baseline partners. The gazette-reinstated row drops the three firms that enter the sample through the pre-appointment co-counsel check rather than a registry partnership predating the swearing-in (SI Section E). The check reinstates a firm at three shared pre-appointment cases; for these three the relative appears on 24, 31, and 46 of the firm's cases before the appointment. The row tightens the partnership definition, not the evidence of a tie. Standard errors, in parentheses, are clustered by firm; clustering the benchmark estimates by the 37 appointing judges instead leaves them unchanged (corporate clients: 0.147 under both). No single judge drives the corporate estimate: dropping each judge and its firms in turn yields estimates between 0.281 and 0.390, 36 of 37 significant at the 5 percent level; aggregating firms to their appointing judges yields similar magnitudes with wider intervals. A randomization test that reassigns appointment years among firms whose judges were appointed in the same period (2011–2017 or 2019–2024) and through the same track, re-estimating the Callaway–Sant'Anna effect in each of 1,000 replications, gives $p = 0.030$ for corporate clients and $p = 0.072$ for corporate cases; the same test does not reject for total caseload ($p = 0.68$), whose gradual path is not identified by appointment timing. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The matched never-treated comparison. The alternative to the timing-only design compares connected firms with unconnected São Paulo law firms selected as follows. The donor pool starts from the 8,339 registered law firms, removes every firm connected to a judge under any tier of the matching rule, and additionally removes firms with any surname overlap with a judge, so that no plausibly connected firm can serve as a control. Each treated firm is then matched to its five nearest neighbors on four pre-appointment covariates—mean log caseload, caseload trend, partner count, and firm age—measured over the five years preceding its own appointment year, with a penalty for mismatched size categories and a caliper discarding the worst decile of matches. Each control serves a single treated firm, so no control is duplicated or reweighted. Table C.2 reports covariate balance: the pre-appointment caseload level and trend are closely balanced; treated firms have more partners; all 50 treated firms lie inside the controls' joint covariate support. Figure C.2 overlays the event studies from the two designs.

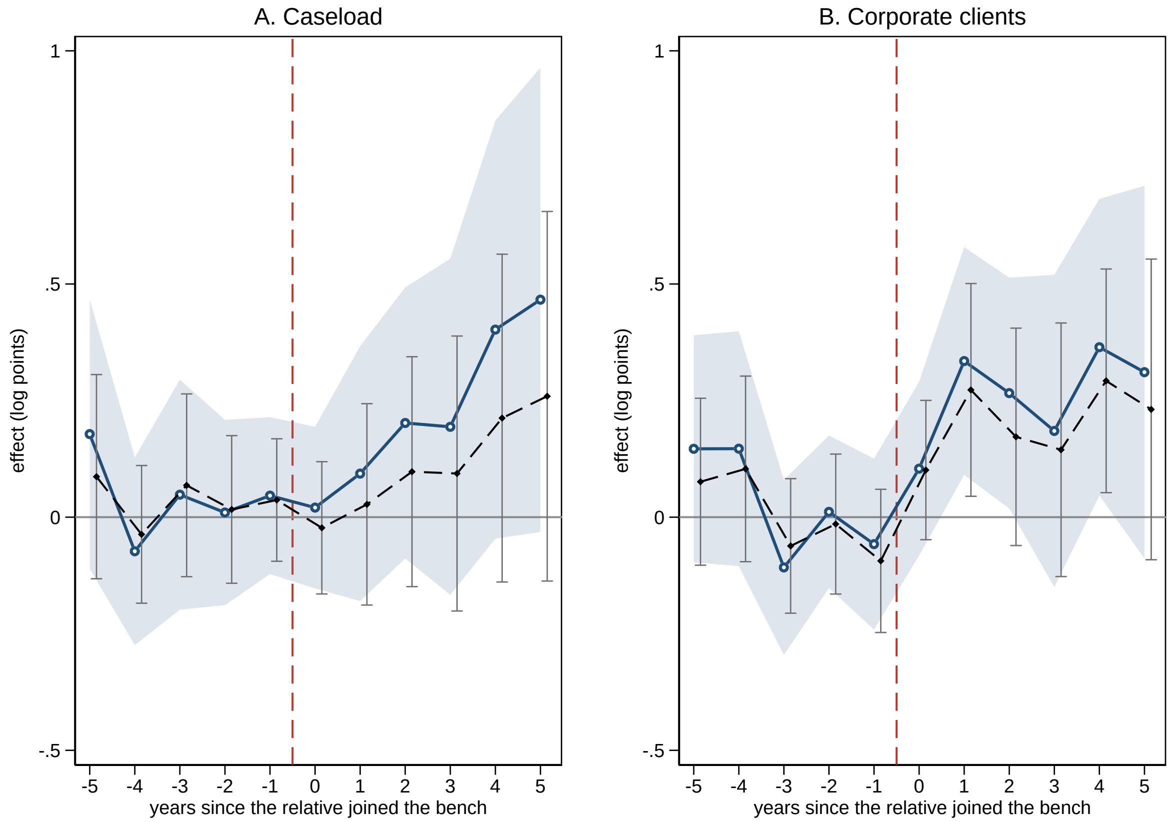
Table C.2: Covariate balance in the matched comparison.

	Treated	Control	Std. diff.
Pre-appointment log caseload	3.80	3.59	+0.13
Pre-appointment caseload trend	0.29	0.31	-0.03
Registered partners	16.48	8.25	+0.35
Firm age	20.42	20.28	+0.02
Firms	50	229	

Notes: Means of the four matching covariates, each measured over the five years before a firm's own appointment year, for the 50 treated firms and their 229 matched controls. The standardized difference is the treated–control difference divided by the pooled standard deviation. The caseload level and trend are well balanced; partner count is the covariate on which treated firms differ most. Re-running the matching procedure from scratch does not close that gap: the rematch replaces eight of the 229 controls, leaves the standardized difference in partner count at $+0.35$, and changes the estimates little (caseload 0.165, corporate clients 0.242). The gap reflects the donor pool rather than the procedure. Treated firms hold 16.5 partners on average against 3.4 for the 7,894 unconnected São Paulo law firms; only 2.8 percent of that pool has seventeen or more partners, and six of the 50 treated firms are larger than its 99th percentile. No matching procedure on this donor universe can balance partner count. For this reason, I place more weight on the specification that excludes the largest treated firms.

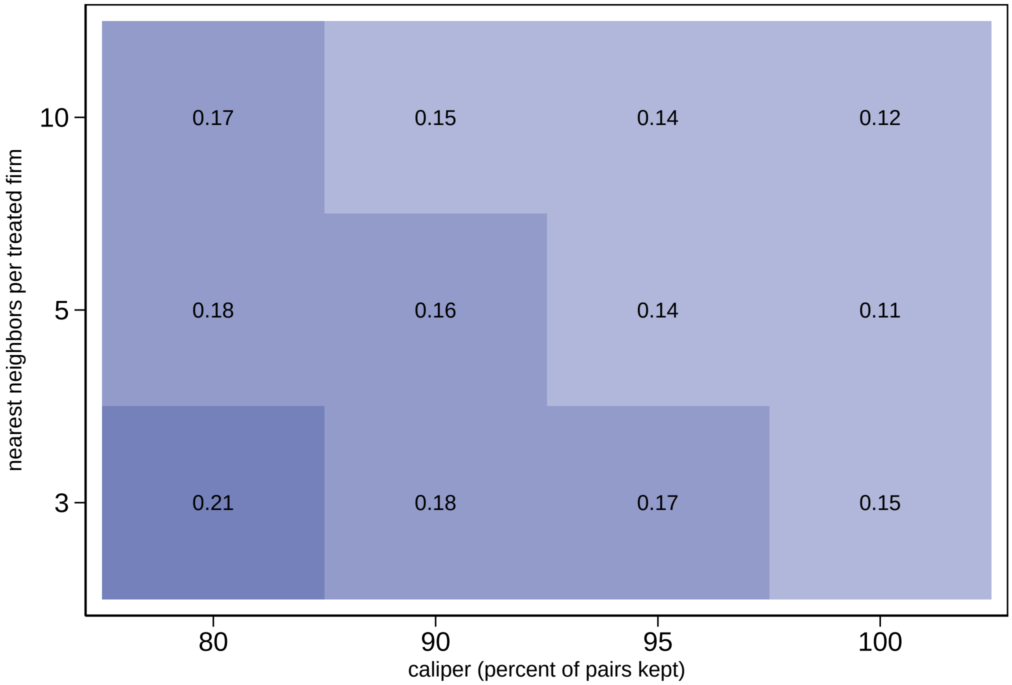
That specification keeps the matched comparison and drops only the largest treated firms. Among the 46 remaining treated firms and their 226 matched controls, the standardized difference in partner count falls to $+0.13$, and the other three covariates balance within ± 0.04.

Figure C.2: The timing-only and matched designs overlaid.



Notes: Solid lines with shaded bands: the timing-only design of Figure 2 (comparison group: not-yet-treated connected firms). Black dashed lines with capped bars: the matched never-treated comparison described above. 95% confidence intervals; the red dashed line marks the appointment. Both designs show flat pre-appointment coefficients and a post-appointment rise; the matched comparison yields somewhat smaller caseload estimates, concentrated in the largest firms (Table C.1).

Figure C.3: The matched estimate is stable across pairing choices.



Notes: Each cell re-runs the matched never-treated caseload event study on the analysis sample, varying the number of matched neighbors per treated firm (rows) and the caliper (columns: the percent of match pairs retained after discarding the worst-matched). Panels come from the matching grid evaluated on the analysis firms and their matched controls. Cells report the pooled post-appointment ATT; estimates range from 0.11 to 0.21 across all twelve pairings, against the benchmark matched estimate of 0.163, and none is statistically significant—the matched caseload estimate does not depend on how the pairing is tuned.

D. Additional case-level results

Table D.1 extends the before/after comparison to further outcome margins on lottery-assigned cases: partial relief, procedural disposal, panel split, and time to decision. Partial relief is the only margin that changes at the appointment; the slower resolution of connected counsel's cases predates it. Table D.2 splits each margin's change by the connected lawyer's procedural side; no margin moves in the connected side's favor. Table D.3 tests the robustness of the before-appointment premium under alternative clustering and specifications.

Table D.1: Outcome margins on lottery-assigned cases, before and after the appointment.

	Before	After	p: before = after	N
Final win	0.0630*** (0.0236)	0.0311*** (0.0072)	0.171	34,208
Partial relief	-0.0163 (0.0135)	0.0210*** (0.0057)	0.006	34,208
Procedural disposal	0.0185* (0.0107)	0.0057 (0.0040)	0.223	34,208
Time to decision (log days)	0.1305*** (0.0387)	0.1633*** (0.0161)	0.392	33,283
Panel split (non-unanimous)	-0.0023 (0.0069)	0.0017 (0.0028)	0.604	32,991

Notes: Each row reports the coefficients on connected counsel before and after the related judge's appointment, from the specification of Table 2 on lottery-assigned cases: fixed effects for procedural role, chamber $\times$ year, and case class; standard errors clustered by chamber (96 clusters). Partial relief indicates an appeal granted in part; procedural disposal indicates dismissal without reaching the merits; panel split indicates a non-unanimous vote, defined where the ruling records the vote. The partial-relief and procedural indicators are coded from the disposition text and validated on a stratified sample of 150 rulings coded independently by a large language model with a verbatim justification quote per ruling and an author-audited subsample: agreement is 149 of 150 for partial relief (the residual case is a documented coding-convention difference over partial cognizance) and 150 of 150 for procedural disposal; two systematic coding errors found in the validation were corrected before estimation. Partial relief is largely invisible to the win/lose coding—partial-relief cases are coded as wins 15 percent of the time, against 74 percent otherwise—and its base rate is 11.2 percent, so the post-appointment coefficient is roughly a 19 percent relative increase. That coefficient is essentially unchanged by client-type $\times$ year fixed effects (0.0209) and is nearly identical for corporate (0.0229) and non-corporate (0.0233) clients, so client composition does not generate it; it is the only margin of the five that changes at the appointment, and it survives a Bonferroni correction for the five tests. Table D.2 splits each change by the connected lawyer's procedural side. The time-to-decision coefficients are positive—connected counsel's cases resolve more slowly—and present before the appointment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: Change at the appointment, by the connected lawyer's procedural side.

	Connected lawyer for the appellee (after − before)	Connected lawyer for the appellant (after − before)	N
Final win	-0.0336 (0.0236)	0.0311 (0.1248)	34,208
Partial relief	0.0402*** (0.0131)	-0.1052 (0.1246)	34,208
Procedural disposal	-0.0132 (0.0106)	-0.0001 (0.0195)	34,208
Time to decision (log days)	0.0321 (0.0382)	0.0708 (0.2180)	33,283
Panel split (non-unanimous)	0.0062 (0.0074)	-0.1056 (0.0858)	32,991

Notes: Each cell is the after-minus-before difference in the connected-counsel coefficient from the specification of Table D.1, with the pre- and post-appointment indicators interacted with the connected lawyer's procedural side: the first column covers cases in which the connected lawyer represents the appellee, the second those in which the connected lawyer represents the appellant. The appellee side (*apelado*, *agravado*, *embargado*, *recorrido*; the terms are used generically for the party defending the lower-court decision in any appeal type) accounts for 98 percent of connected observations (15,219; the appellant side, 285). The sign matters: partial relief grants the appellant's appeal in part, so for the appellee it is a partial reversal of the client's lower-court victory, and a procedural disposal dismisses the appellant's appeal, which favors the appellee. The only significant change is the rise in partial reversals of connected clients' victories; no margin moves in the connected side's favor. All cells come from one interacted regression per outcome, not from split samples; each column reports the within-side after-minus-before contrast. Standard errors clustered by chamber. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Robustness of the before/after comparison.

	Before	After	p: before = after
Baseline (chamber clusters)	0.0617*** (0.0229)	0.0300*** (0.0072)	0.164
Clustered by lawyer	0.0617* (0.0357)	0.0300** (0.0137)	0.398
Two-way: chamber, lawyer	0.0617* (0.0360)	0.0300** (0.0132)	0.398
Excluding top-3 pre-appointment lawyers	0.0960*** (0.0298)	0.0302*** (0.0072)	0.026
Lawyers at the 50 analysis firms only	0.0585** (0.0246)	0.0601*** (0.0192)	0.963
Partnership entry predates the posse	0.0591** (0.0247)	0.0485*** (0.0085)	0.675
Lawyers observed on both sides only	0.0511** (0.0246)	0.0680*** (0.0200)	0.622
Dropping the posse calendar year	0.0621*** (0.0229)	0.0300*** (0.0073)	0.159
Case-class × year fixed effects	0.0584** (0.0227)	0.0295*** (0.0071)	0.194

Notes: The before-appointment premium in Table 3 rests on 36 distinct connected lawyers observed in 1,358 case appearances before their relative's appointment (263 lawyers appear after it), because the case data begin in 2008 and left-censor earlier careers. This table stress-tests that cell on the exact Table 3 lottery sample ($N = 34{,}786$; the baseline row reproduces it to the fourth decimal). Clustering by lawyer, or two-way by chamber and lawyer, widens the before-appointment interval ($p = 0.084$ and 0.090) without changing the conclusion that the premium does not increase after the appointment. The three highest-volume pre-appointment lawyers hold 49.3 percent of the pre-appointment appearances; excluding them raises the before coefficient to 0.096. Three rows restrict the connected set: to lawyers at the 50 firms of the analysis sample (the same firms as the firm-level design); to lawyers whose registry entry into the partnership predates the earliest appointment among their judge-relatives; and to the 28 lawyers observed on both sides of the appointment, so the before and after cells compare the same individuals. Under all three restrictions the premium is flat across the appointment ($p = 0.96$, 0.68, and 0.62). A within-lawyer specification on the both-sides subsample, which adds lawyer fixed effects and identifies only the change at the appointment, gives a change of -0.021 (SE $= 0.080$). The last two rows drop the posse calendar year, whose cases the annual coding cannot place before or after the swearing-in date, and replace case-class fixed effects with case-class-by-year fixed effects, which absorb class-specific shifts in the case portfolio; the estimates are unchanged. One-sided equivalence tests on the baseline reject an appointment-induced increase in the premium as large as half the before-appointment premium ($p = 0.003$) and as large as the full premium ($p < 0.001$). The baseline after-coefficient pools two groups: lawyers at the 50 analysis firms, and lawyers at firms whose relative was appointed before the panel begins. The estimated premium is smaller for the second group, which pulls the pooled coefficient down. The restricted rows drop that group, which is why their after coefficients are higher. Leave-one-lawyer-out estimates over the 36 pre-appointment lawyers range from 0.041 to 0.091 (mean 0.062), and 35 of 36 retain a 95 percent confidence interval above zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E. Connection at the time of appointment

A firm enters the analysis sample only if the kinship tie predates the appointment. Table E.1 shows that such ties are the typical case: when the registry can observe the pre-appointment state, 65 to 80 percent of relatives were already partners when the judge took office. Table E.2 classifies the treated firms by when the relative became a partner, and Figure E.1 plots the distribution of partnership entries around the appointment.

Table E.1: When the partnership began, relative to the appointment.

Appointment	Judges with a matched relative	Relative was already a partner at the appointment	Share
before 2001	102	7	7%
2001–2010	122	42	34%
2011–2020	40	26	65%
after 2020	30	24	80%
All	294	99	34%

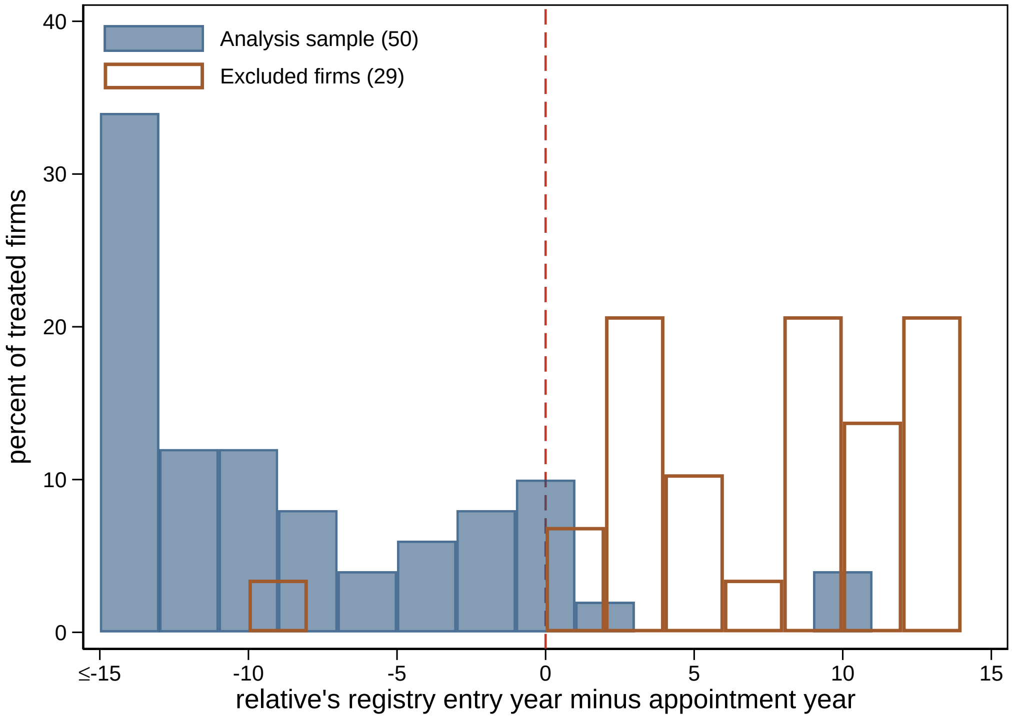
Notes: For every judge with a matched partner-relative whose partnership entry date and swearing-in date are both recorded, the table reports how often the relative was already a registered partner when the judge took office. The registry is a current snapshot and records no exit dates, so a relative who was a partner in 1995 and has since left the firm cannot be observed: the earlier the appointment, the more the measure is censored, and the first two rows understate the share for that reason rather than describing a different era. The rows for appointments since 2011, where the registry can observe the pre-appointment state, are the informative ones, and they match the partnership-date audit of the treated firms in Table E.2. The prevalence counts in Table 1 therefore combine ties that predate the appointment with relatives who joined a firm afterward.

Table E.2: Classifying treated firms by when the relative became a partner.

	Firms
Treated firms with an in-window appointment	79
Relative's registry entry relative to the appointment	
Partner before the appointment year	45
Entered in the appointment year	5
Entered after the appointment year	29
Gazette check on firms failing the registry test	
Relative appears in the gazette before the appointment	8
Shares pre-appointment cases with a firm partner	5
Reinstated (three or more shared cases)	4
Connected at the appointment	52
Another relative already observed deciding cases	2
Analysis sample	50
Excluded	29

Notes: The partner registry records the date each partner entered the firm. A treated firm is connected at the appointment if the matched relative was already a registered partner in an earlier year, or entered in the appointment year before the swearing-in date. Firms failing that test are checked against the full gazette: a relative who appears on the firm's cases alongside its partners before the appointment was affiliated with the firm even though the registry records the partnership later, and is reinstated at a threshold of three shared cases. The registry records equity partners only, so a relative employed as an associate is not visible in it; the gazette check is the correction for that limitation. The reinstated firms satisfy documented pre-appointment affiliation rather than registered equity partnership, so the sample definition is connection at the appointment—partnership or documented affiliation; Table C.1 reports the estimates excluding the three reinstated firms in the analysis sample. Median time as a partner before the appointment among firms passing the registry test is three years. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure E.1: When the relative became a partner, relative to the appointment.



Notes: Distribution of the matched relative's registry entry year minus the judge's appointment year, for the two groups of treated firms. Negative values mean the relative was already a partner; the leftmost bin collects gaps of fifteen years or more. Four firms appear to the right of zero among those counted as connected at the appointment: their registry entry postdates the appointment, but the relative appears on the firm's cases alongside its partners beforehand, so the gazette places them at the firm earlier than the registry does. The excluded firms cluster far to the right—their relatives entered eight to fourteen years after the appointment, in 2023–2026.

F. When the judge leaves the bench

This SI section documents the exit design. Table F.1 reports the pooled post-exit estimates. Table F.2 traces the sample construction. Table F.3 splits the outcomes by client type. Table F.4 and Figure F.1 show that the estimates agree across five estimators. Table F.5 varies the exit dating, the sample, and the comparison group.

Table F.1: The stock of business after the judge's exit.

	Caseload	Corporate clients
Kinship tie predates the panel (24 treated, 24 comparison)	0.121 (0.191)	-0.066 (0.183)
Kinship tie predates the exit (42 treated, 58 comparison)	-0.016 (0.129)	-0.117 (0.143)

Notes: Pooled post-exit average treatment effects from Callaway–Sant'Anna staggered difference-in-differences on connected firms whose judge was appointed before the observation window, so the exit is the only event in the panel. A firm is treated when its judge leaves the bench (last year with at least fifty decisions as reporting judge, 2011–2022; every exit that can be checked against the court's biographical records agrees within one year); comparison firms are identically constructed connected firms whose judge is still deciding cases in 2023–24. The first row requires the kinship tie to predate the panel; the second requires it to predate the exit. Standard errors clustered by firm. The corporate pre-trend is marginal in the first row ($p = 0.089$); pre-trends are unproblematic elsewhere. One-sided tests against a decline the size of the appointment effect reject it for caseload ($p = 0.016$ and 0.018 in the two samples) and fall just short for corporate clients ($p = 0.067$ and 0.059). This SI section reports the sample construction, agreement across five estimators, and robustness to exit dating, documented retirements only, and leave-one-judge-out. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.2: Exit design: sample construction.

	Firms
Connected firms with a judge active in the gazette era and an observed exit, 2011–2022	99
outside the appointment design (always-treated)	83
with gazette caseload (all with a registry-dated kinship tie)	71
relative was a registered partner before the exit	42
relative was a registered partner before the panel (entry ≤ 2009)	24
Comparison firms (judge still active in 2023–24): before-panel / before-exit definition	24 / 58

Notes: A judge's exit year is the last year with at least fifty decisions as reporting judge; judges still above that threshold in 2023–24 are censored and serve as comparisons. All fifteen exits that can be checked against the court's 2018 biographical compendium agree with the documented retirement year within one year. Treated exits are spread across ten cohorts (2011–2022, one to five per year). The registry records current partners only, so ties whose holder left the firm before 2025 are invisible. Because the registry snapshot postdates the exits, the estimates apply to historically connected firms whose ties remain observable in the current registry; firms that dissolved or lost the partner after the exit are not in the sample. One-sided tests against a decline the size of the appointment effect reject it for caseload ($p = 0.016$ and 0.018) and fall just short for corporate clients ($p = 0.067$ and 0.059), so the evidence of persistence is stronger for caseload than for corporate clients.

Table F.3: Client composition around the exit.

	Corporate clients	Corporate cases	Individual cases
Kinship tie predates the panel	-0.066 (0.183)	-0.101 (0.198)	-0.186 (0.160)
Kinship tie predates the exit	-0.117 (0.143)	-0.118 (0.158)	-0.215 (0.139)

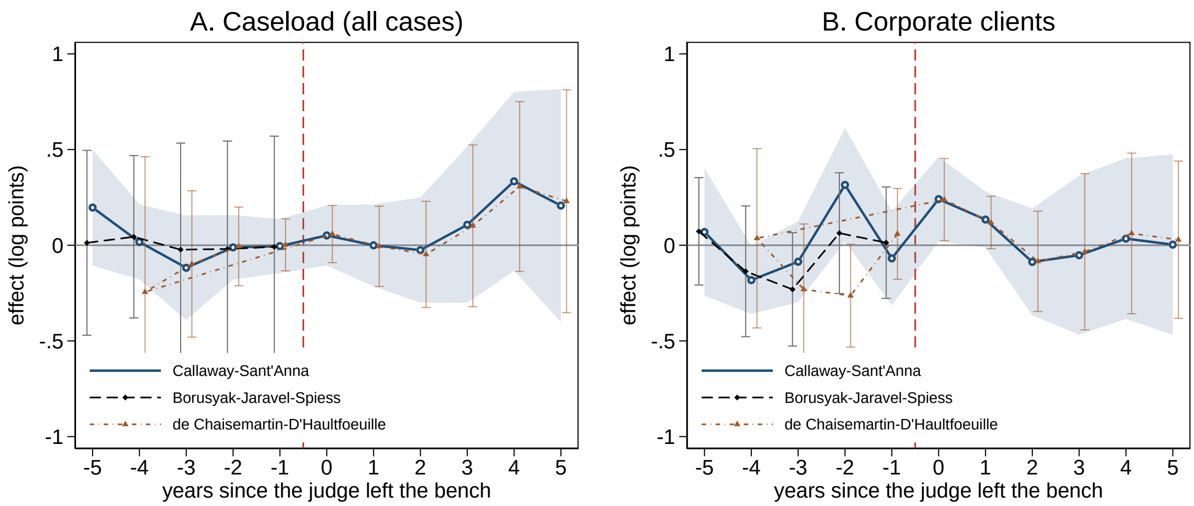
Notes: Same design as Table F.1; outcomes are $\log(1+x)$ of distinct corporate clients, corporate cases, and individual cases per firm-year, constructed by the party-name classification of SI Section H. Joint pre-trend tests are unproblematic in the before-exit sample ($p = 0.46$, 0.35, 0.23) and marginal for the corporate outcomes in the smaller before-panel sample ($p = 0.089$ and 0.066), so the corporate conclusion rests on the larger sample. Standard errors clustered by firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.4: The exit estimates are estimator-invariant.

	Caseload		Corporate clients	
	Tie predates panel	Tie predates exit	Tie predates panel	Tie predates exit
Callaway–Sant'Anna (pooled)	0.121 (0.191)	-0.016 (0.129)	-0.066 (0.183)	-0.117 (0.143)
Borusyak–Jaravel–Spiess	0.156 (0.171)	-0.111 (0.128)	0.088 (0.118)	-0.096 (0.100)
de Chaisemartin–D'Haultfœuille	0.083 (0.114)	0.089 (0.088)	0.060 (0.121)	0.045 (0.092)
same switchers	0.124 (0.124)	0.118 (0.100)	0.016 (0.142)	-0.015 (0.121)
Two-way fixed effects	0.118 (0.155)	-0.064 (0.121)	0.162 (0.111)	-0.024 (0.101)

Notes: Pooled post-exit effects under five estimators: Callaway–Sant'Anna (doubly robust), Borusyak–Jaravel–Spiess imputation, de Chaisemartin–D'Haultfœuille with and without the balanced same-switchers restriction (average of effects one through five), and two-way fixed effects. All twenty estimates lie between -0.12 and $+0.16$ log points. None of the twenty estimates is statistically significant. Standard errors clustered by firm; clustering the two-way fixed-effects specification by judge instead (38 and 66 clusters) gives $+0.118$ (se 0.154) and -0.064 (se 0.127) for caseload. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure F.1: Event-study paths agree across estimators.



Notes: Callaway–Sant'Anna (solid, with 95 percent band), Borusyak–Jaravel–Spiess (dashed), and de Chaisemartin–D'Haultfœuille (dash-dot, offset) event-time paths for the before-panel sample, both outcomes.

Table F.5: Exit-date, subset, and comparison-group robustness.

	Caseload		Corporate clients	
	Tie predates panel	Tie predates exit	Tie predates panel	Tie predates exit
Benchmark (treatment at exit +1)	0.121 (0.191)	-0.016 (0.129)	-0.066 (0.183)	-0.117 (0.143)
Treatment at the exit year	0.128 (0.174)	-0.098 (0.113)	-0.139 (0.211)	-0.130 (0.158)
Treatment at exit +2	-0.002 (0.198)	-0.109 (0.138)	-0.297* (0.179)	-0.101 (0.157)
Documented retirements only (15 firms)	0.250 (0.225)	0.067 (0.187)	-0.174 (0.251)	-0.234 (0.216)
Exiting firms only (no never-exited)	0.223 (0.224)	0.198 (0.194)	-0.054 (0.202)	-0.065 (0.155)
Leave-one-judge-out range (set B)	[-0.086, 0.022]		[-0.162, -0.016]	

Notes: Pooled post-exit effects, Callaway–Sant'Anna, standard errors clustered by firm. The documented-retirement subset comprises the fifteen treated firms whose judge's retirement appears in the 2018 biographical compendium within one year of the activity-based date; the compendium does not cover later exits, which is why the subset is small. The leave-one-judge-out row reports the range of the pooled effect across re-estimations dropping each treated judge in turn. One cell of twenty reaches $p < 0.10$ (corporate clients, before-panel sample, treatment at exit +2); it arises in the sample whose corporate pre-trend already drifts, disappears under the identical shift in the larger sample, and is consistent with chance across twenty specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G. The appointment as a public event

The court announces every appointment on its public news portal. Of the 37 appointing judges in the analysis sample, 35 have a dated announcement page for their swearing-in (*posse*); 34 of the 35 pages mention the incoming judge's family. A systematic search of every archived page for the matched partners' names finds the partner-relative named on four judges' pages—three by full name, one by first name with the identity confirmed by the mother's surname. Naming in the announcement is therefore one channel of visibility, not the main one: the shared rare surname is public in every gazette filing, and the appointment itself is the public event. The pages contain the ceremony account, the judge's biography, and a professional photo gallery, and the court circulates them on social media. Figure G.1 reproduces one announcement.

Figure G.1: The court's announcement names the partner-relative.


Tribunal de Justiça
Estado de São Paulo
A Justiça próxima do cidadão

Mais quatro novos desembargadores são empossados no TJSP

26/11/2013

Em sessão solene, realizada hoje (26) no Salão do Júri do Tribunal de Justiça de São Paulo, sob o comando do presidente Ivan Sartori, tomaram posse quatro novos desembargadores: Roberto Caruso Costabile e Solimene, Henrique Rodrigueiro Clavisio, Maria Cristina Cotrofe Biasi e Maury Angelo Bottesini. Os magistrados assinaram o termo de posse, prestaram compromisso e receberam o Colar de Mérito Judiciário.

Em nome do TJSP falou o desembargador Otacílio Ferraz Felisardo, que enalteceu a dedicação dos colegas. "São devotados heroicamente ao trabalho, com espírito de sacrifício, coragem e serenidade que a função

Para Maria Cristina Cotorofe Biasi, a magistratura sempre significou a corporificação da Justiça. "Fui apresentada à Têmis (referindo-se à deusa da Justiça), na infância, através das mãos de meu pai Walter Cotrofe, advogado militante, que exerceu a judicatura trabalhista, e da minha mãe Maria José. Ambos me ensinaram a trilhar o caminho da ética e do justo". A desembargadora contou que olhava, ainda menina, a estátua da deusa enigmática, que acompanhou a carreira de meu pai, e ficava encantada com a venda nos olhos, a espada e a balança. "A solenidade representa um ato de amor. Amor à Magistratura, amor a este Tribunal, amor à Justiça", afirmou.

Notes: The TJSP news portal's announcement of the November 26, 2013 swearing-in, as archived from https://www.tjsp.jus.br/Noticias/Noticia?codigoNoticia=20921. The boxed paragraph quotes the incoming judge's ceremony speech: she was introduced to Themis in childhood "through the hands of my father Walter Cotrofe, a practicing lawyer" (*advogado militante*). Walter Cotrofe is the matched partner-relative at the connected firm, and the court's own press release identifies his profession. Announcement pages for the sample's appointments are archived in the replication materials.

H. Data construction and variable definitions

Tables H.1 and H.2 define every variable in the firm-year and case-by-lawyer panels.

Table H.1: Firm-year panel: variable definitions.

Variable	Definition	Source
Caseload	Distinct appellate cases in year t in which any registered partner of the firm appears as counsel; used as $\ln(1+x)$. Partners are taken from the current registry snapshot, so a partner's cases count toward the firm in every panel year, including years before the partner's recorded entry; attributing a later entrant's earlier cases to the firm raises pre-appointment counts and so, if anything, attenuates the estimates.	DJE $\times$ Receita Sócios
Corporate clients	Distinct corporate parties the firm represents in year t. A party is government if its name matches public-entity patterns (município, fazenda, INSS, and similar), corporate if it matches company patterns (Ltda., S.A., banks, and similar), and individual otherwise; $\ln(1+x)$.	DJE case headers
Individual clients	Same construction, individual parties; $\ln(1+x)$.	DJE case headers
Corporate cases; individual cases	Cases in year t whose client—the party the firm's lawyer represents—is corporate (respectively individual) under the same party-name classification; $\ln(1+x)$.	DJE case headers
New clients	Distinct clients the firm represents in year t and in no earlier year; $\ln(1+x)$.	DJE case headers
Repeat clients	Distinct clients the firm represents in year t and in at least one earlier year; $\ln(1+x)$.	DJE case headers
Partners	Registered partners in year t, from partner entry dates; $\ln(1+x)$.	Receita Sócios
Employees; payroll	Formal employees and total payroll of the firm in year t.	RAIS
Treatment year	Year of the earliest verified appointment (*posse*) of a partner-relative to the appellate bench.	Seniority roster; Curriculum; DJE
Judge's exit year	Last year with at least fifty decisions as reporting judge in the case-level panel (2008–2024); judges above the threshold in 2023–24 are censored as still active. Every exit checkable against the court's 2018 biographical compendium (fifteen) agrees with the documented retirement year within one year.	DJE relator counts
Exit-design kinship tie	The relative's registry entry as a partner predates the panel (entry $\leq$ 2009; primary sample) or predates the exit year (robustness sample).	Receita Sócios

Table H.2: Case-lawyer panel: variable definitions.

Variable	Definition	Source
Won	Equals one if the lawyer's side prevails: appeal granted (*provido*) counts for the appellant; appeal denied (*improvido*) or not heard (*não conhecido*) counts for the appellee. Partial grants and roles that do not identify a side are set to missing.	DJE dispositions
Connected	Matched partner-relative of a sitting judge other than the judge deciding the case.	Matching (SI Section A)
Surname-only match	Rare-surname match not confirmed as a partner-relative; controlled separately in every specification.	Matching (SI Section A)
Before/after appointment	Case decided before versus at or after the earliest related judge's *posse* year.	DJE; posse records
Assignment method	From the e-SAJ distribution record: lottery (*sorteio*), routing to the judge already seized of a related case (*prevenção*), or specialized subject-matter jurisdiction (*competência exclusiva*).	e-SAJ
Chamber	Panel number and section from the gazette; enters as chamber $\times$ year fixed effects and as the clustering unit.	DJE
Procedural role	The side the lawyer represents (appellant, appellee, and variants); fixed effects.	DJE
Case class	Procedural class of the case; fixed effects.	DJE
Partial relief	Disposition grants the appeal in part ("em parte," "parcial" in the disposition text).	e-SAJ rulings
Procedural disposal	Case resolved without reaching the merits (not heard, *prejudicado*, *seguimento negado*, extinction).	e-SAJ rulings
Panel split	Non-unanimous vote; defined where the ruling records the vote.	e-SAJ rulings
Time to decision	Days from the e-SAJ distribution date to publication of the ruling in the DJE, logged; values outside 0–3,650 days are set to missing.	e-SAJ; DJE