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JUDICIAL PERFORMANCE, BANK LENDING AND FIRMS' INVESTMENTS: EVIDENCE FROM COURT LEADERSHIP TURNOVER

by Franco Fiordelisi*, Davide Moretti*, Nazzareno Ruggiero* and Konstantinos Baltas*

Abstract

We exploit plausibly exogenous variation in court-president appointments in Italy and show that leadership turnover slows the growth in the duration of bankruptcy proceedings. These effects are larger when incoming presidents are younger, have prior managerial experience, or are locally rooted in the court's jurisdiction, suggesting that career concerns and organizational learning play an important part. Improved judicial efficiency strengthens creditor rights through the credit channel: bank lending expands, late payments decline, and larger firms reallocate investment towards intangible assets.

JEL Classification: E22, G21, G28, G33, L51, L53, O43.

Keywords: judicial efficiency, credit markets, firms' investments.

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1 Introduction¹

A sound institutional environment is a cornerstone of economic development (North 1991, 1994, La Porta et al. 1998). The legal system’s capacity to protect property rights and enforce contracts shapes firm investment, credit availability, and bank lending (Acemoglu & Johnson 2005, Laeven & Woodruff 2007, Qian & Strahan 2007, Rodano 2021, Djankov et al. 2025, Ramos-Maqueda & Chen 2026). Legal reforms that improve judicial efficiency generate measurable gains in credit markets (Djankov et al. 2007, Gopalan et al. 2016, Ponticelli & Alencar 2016, Fedaseyeu 2020, D’Apice et al. 2024) and real economic outcomes (Pezone 2023, Rodano et al. 2016, Ponticelli & Alencar 2016, Agrawal et al. 2022).

A key unresolved question is why substantial heterogeneity in court performance persists even under a common regulatory framework and standardized funding. Giacomelli et al. (2018) and Cugno et al. (2022) document wide variation in trial duration after accounting for caseload composition, staffing, and institutional factors. This residual heterogeneity points to internal organizational drivers—managerial effort, case allocation, and administrative coordination—that operate largely outside formal rules. Yet the role of court leadership in shaping these practices has received virtually no empirical attention.

We examine a neglected dimension of judicial performance: whether changes in court leadership, specifically president turnover, improve judicial performance and whether these gains propagate to credit markets and firm investment. Court presidents perform central managerial functions: assigning cases, supervising judges and staff, setting organizational priorities, and coordinating administrative resources. Whether routine leadership transitions yield measurable performance gains, which characteristics of incoming presidents amplify these effects, and whether organizational improvements translate into broader economic outcomes remain open questions. We provide the first empirical evidence on all these questions.

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Italy offers a compelling laboratory for studying judicial inefficiency. Despite multiple reform efforts and increased public investment, the length of proceedings, especially in bankruptcy cases, remains a prominent and persistent policy concern and has attracted substantial empirical attention (Giacomelli et al. 2018, Pezone 2023, Canzian & Ferrara 2025, Cannella et al. 2024). Within-region variation in court outcomes is striking. In 2022, for example, average bankruptcy durations within the same region ranged from 1,710 to 3,984 days in Liguria, from 1,684 to 2,713 days in Lombardy, from 1,923 to 4,851 days in Tuscany, from 1,949 to 4,211 days in Latium, from 2,709 to 6,640 days in Sicily, and from 1,890 to 6,257 days in Calabria.² This pattern of idiosyncratic dispersion mirrors evidence on within-region productivity heterogeneity across Italian public institutions more broadly (Fenizia 2024). This idiosyncratic variation, which persists after accounting for resources and formal rules, motivates a focus on internal organizational drivers. Consistent with this view, Rodano et al. (2016) document that observable differences in judge quality or effort do not explain the heterogeneous organizational and administrative performance of Italian courts, effectively generating quasi-random variation in judicial efficiency across courts. To investigate these determinants, we construct a novel court-level panel for 2015–2022 that links judicial outcomes to leadership transitions and downstream economic activity. The dataset draws on five main sources: (i) administrative records on case-processing times and court characteristics from the Italian Ministry of Justice; (ii) detailed records on the timing and nature of court-president turnovers from the High Council of the Judiciary (*Consiglio Superiore della Magistratura* — CSM); (iii) bank–firm credit exposures from the Central Credit Register (*Centrale dei Rischi* — CR); (iv) firm balance-sheet data from *Cerved Group*; and (v) regional economic statistics from the Italian National Institute of Statistics (ISTAT).

Our empirical strategy exploits plausibly exogenous variation in turnover events (mandatory retirements, deaths, and resignations) and implements a staggered difference-in-differences design (Sun & Abraham 2021). Comparing courts that experience court-president turnover

²Source: Italian Ministry of Justice.

with those that do not, while controlling for court fixed effects and time-varying confounders, yields credible identification of the managerial contribution to judicial performance. We examine heterogeneity by incoming presidents’ characteristics and subject our estimates to extensive robustness checks. Our main findings are: i) appointing a new court president reduces the growth in the duration of bankruptcy proceedings; ii) effects are heterogeneous in ways that illuminate the underlying mechanism: gains are largest when incoming presidents are younger than their predecessors (consistent with stronger career incentives), have prior managerial experience, or are locally rooted in the court’s jurisdiction. These gains persist over time, ruling out transient attention effects and pointing to durable changes in organizational practice. Leadership-induced improvements in judicial performance propagate to the real economy: faster and more predictable case resolution reduces late payments on outstanding loans, expands credit supply on both the intensive and extensive margins, and induces larger firms to reallocate investment toward intangible assets.

Our findings contribute to three strands of literature. First, we add to research on how legal institutions shape economic outcomes ([Chemin 2020](#), [Visaria 2009](#), [Pezone 2023](#)) by showing that enforcement quality can improve through internally generated organizational change. Our identification strategy circumvents the endogeneity concerns that complicate inference in reform-based studies. Second, we extend the literature on managerial effects in organizations ([Bloom et al. 2020](#)) to public institutions, showing that individual court presidents generate measurable effects on judicial performance and downstream economic outcomes. Third, we contribute to the literature on career incentives and internal labor markets in the judiciary ([Melcarne 2017](#)) by establishing that managerial characteristics could predict performance gains. Together, the evidence shows that leadership transitions constitute a meaningful, internally generated source of variation in enforcement quality and an underutilized lever for improving judicial performance.

The remainder of the paper is organized as follows. [Section 2](#) reviews the related literature and develops our hypotheses. [Section 3](#) describes the institutional context and the functioning

of bankruptcy proceedings in Italy. [Section 4](#) describes the dataset construction. [Section 5](#) outlines the empirical strategy. [Section 6](#) discusses the main results, and [Section 7](#) offers robustness checks. [Section 8](#) concludes.

2 Literature and Hypotheses

A growing literature shows that judicial reforms and institutional changes can shorten case-processing times, improve credit markets, lower intermediation costs, and generate measurable real effects ([Djankov et al. 2007](#), [Gopalan et al. 2016](#), [Ponticelli & Alencar 2016](#), [Rodano et al. 2016](#), [Chemin 2020](#), [Li & Ponticelli 2022](#), [Pezone 2023](#), [D’Apice et al. 2024](#)). A common empirical regularity across these papers is substantial heterogeneity in court performance even within countries that operate under uniform procedural rules and funding regimes. This heterogeneity does not stem from inadequate rules, which are standard across all courts and can be improved through legal reforms (as discussed in [D’Apice et al. 2024](#), [Pezone 2023](#), [Rodano 2021](#)), but rather from the court’s internal organization. A few papers ([Giacomelli et al. 2018](#), [Cugno et al. 2022](#)) suggest that both observable and non-observable court-specific characteristics significantly affect the duration of real estate execution and civil legal proceedings, respectively. While these papers suggest organizational heterogeneity, they remain largely silent on the mechanisms through which such inefficiencies arise and, crucially, on whether changes in internal judicial governance can mitigate them.

The legal literature emphasizes the central role of court presidents in shaping judicial productivity. By exercising authority over case allocation, administrative oversight, and performance monitoring, court presidents determine the organizational and cultural conditions under which judges operate ([Benvenuti & Paris 2018](#), [Blisa & Kosař 2018](#)). Leadership transitions, therefore, represent discrete organizational shocks that may alter court functioning without changing formal legal rules, consistent with viewing the judiciary as an internal labor market in which career-motivated judges signal ability through higher output ([Melcarne](#)

2017).

Leadership turnover can affect court performance through two opposing channels. On the one hand, a “new broom” effect predicts that incoming presidents exert heightened effort to signal competence, consistent with career-concern models of public-sector leadership (Holmström 1999, Dal Bó et al. 2013). This effect can manifest across different horizons: in the short run, an incoming president may prioritize closing the oldest pending cases, mechanically compressing the mean duration; in the medium run, procedural reorganization, stricter monitoring, and improved coordination can generate more durable efficiency gains (Bandiera et al. 2020). On the other hand, managerial transitions entail adjustment costs: incoming presidents must acquire court-specific knowledge, master local practices, and restructure delegation networks, temporarily depressing productivity (Eisfeldt & Kuhnen 2013, Jenter & Lewellen 2020). Because these forces operate over different horizons and in opposite directions, the net effect is theoretically ambiguous. Whether leadership turnover improves judicial performance, and whether any such effect is transitory or persistent, are ultimately empirical questions. Our first hypothesis is, accordingly, that court-president turnover affects the evolution of judicial proceedings’ duration (H_1). While prior work on judicial reform has either treated all civil proceedings as a single efficiency measure or focused on a specific subsection (Table 1), we adopt a more comprehensive approach to examine the effect of court-president turnover on the judicial system by focusing first on the aggregate *Civil Caseload* and then on specific categories of proceedings (e.g. bankruptcy, labor, etc.). Our choice is driven by two main factors. First, court presidents hold organizational responsibility for all types of proceedings, so making an *ex-ante* decision to concentrate on a single category could be misleading. Second, as shown later in Section 3, case length varies considerably across matter types, implying that leadership turnover may have heterogeneous effects across categories.

We next examine whether the effects of leadership turnover are heterogeneous, and whether they are transitory or persistent—both of which speak to the underlying mechanism.

Transitory gains are consistent with short-lived attention effects or reputation building (Jovanovic & Prat 2021), whereas persistence points to durable organizational change through sustained managerial practices (Bloom et al. 2020). If leadership effects operate primarily through incentives and managerial effort, performance gains should vary systematically with observable characteristics of incoming presidents.

Three characteristics of incoming presidents are particularly salient: age, holding managerial experience, and local ties. Age captures both energy and career incentives. Although younger leaders may have less experience (Iverson et al. 2023), they face stronger incentives to signal ability and build reputational capital (Ash & MacLeod 2024); consistent with prior evidence (Fenizia 2022), we expect larger performance gains when incoming presidents are younger than their predecessors. Prior managerial experience equips incoming presidents with organizational skills and institutional knowledge that facilitate adopting best practices (Graham et al. 2013, Custódio & Metzger 2014, Benmelech & Frydman 2015, Coraggio et al. 2025). Local ties operate through competing forces: familiarity with the local institutional environment fosters cooperation and reduces coordination frictions, while greater geographic distance may disrupt entrenched norms and limit local capture. The net effect of local ties on judicial performance is therefore an empirical question (Ren et al. 2021, Bedendo et al. 2023). Our second research hypothesis is that the effects of leadership turnover on the evolution of judicial-proceeding duration are amplified when the incoming court president is younger than the outgoing president (H_{2A}), has prior managerial experience (H_{2B}), or is locally rooted in the court’s jurisdiction (H_{2C}).

Next, we examine the downstream economic consequences of leadership-induced improvements in judicial efficiency. More predictable and timely bankruptcy proceedings reduce creditor recovery risk, strengthen enforcement rights, and sharpen borrower repayment incentives. Prior work shows that judicial reforms lower default rates (D’Apice et al. 2024), reduce intermediation costs (Müller 2022), and expand credit supply by curbing moral hazard (Djankov et al. 2007, Ponticelli & Alencar 2016, Fedaseyeu 2020, Schiantarelli et al. 2020).

Beyond credit markets, judicial effectiveness shapes the level and composition of real investment: reforms raise firm investment and productivity (Chemin 2020, Pezone 2023), and stronger enforcement encourages investment in intangible assets — collateral-poor by nature and therefore especially sensitive to creditor protection (Claessens & Laeven 2003, Crouzet et al. 2022). We extend this literature along two dimensions. First, rather than exploiting statutory reforms, we identify variation in judicial performance through leadership transitions, which improve enforcement quality without altering the formal legal framework. Second, we examine both the extensive and intensive margins of credit supply and trace the downstream effects on firm investment and its composition, providing a more complete picture of the real and financial consequences of judicial efficiency than prior single-outcome studies. Our third hypothesis is that leadership turnover among court presidents mitigates borrower moral hazard (H_{3A}), expands lending to both incumbent (H_{3B}) and new borrowers (H_{3C}), and induces firms to increase investment, with reallocation toward more enforcement-sensitive forms of capital (H_{3D}).

3 Institutional Background

Italy provides a compelling laboratory for studying the organizational determinants of judicial performance (Pezone 2023, Schiantarelli et al. 2020). After repeated reforms to its bankruptcy framework, Italy’s judicial system continues to lag behind international benchmarks (André & Demmou 2022, World Bank 2025). Persistent variation across courts, even located in the same Italian region or judicial district, points to internal organizational drivers that the regulatory framework alone cannot fully explain. This section describes the structure of Italian courts, the managerial role of court presidents, and the institutional determinants of presidential turnover.

The Italian judicial system comprises 140 courts (Figure 1): 50 in the north, 28 in the center, and 62 in the south. Italian courts are organized into sections by matter type,

Table 1: The Italian Judicial System: Related Literature

The table classifies papers by the shock or reform exploited, identification strategy, type of justice, main outcome(s), sample period, and whether the bankruptcy-proceeding duration is a key variable (outcome or identification variable).

Study	Shock	Identification	Justice Type	Main Outcome(s)	Period
Pinotti (2015)	Mafia infiltration in Puglia & Basilicata (1970s)	Synthetic control	Criminal	GDP per capita	1951–2007
Giacomelli & Menon (2017)	—	Spatial RD on court jurisdiction borders	Civil	Firm size	2002–2007; 2008–2009
Giacomelli et al. (2018)	Mortgage foreclosure reforms 2015–16	Linear probability models	Civil (mortgage foreclosure)	Foreclosure proc. duration; pre-sale and sale phases	2003–2018
Schiantarelli et al. (2020)	—	Firm-quarter fixed effects	Property Execution	Loan repayment delays	2008–2013
Rodano (2021)	—	Spatial RD on court jurisdiction borders	Civil / Bankruptcy	Bank credit cost and availability, firm investment	2015–2016
Cugno et al. (2022)	—	Descriptive, cross-sectional regressions	Civil	Proceeding length; court productivity	2010–2020
Pezzone (2023)	Judicial reform 2012 (court mergers)	Event study, spatial discontinuity design	Civil	Firm employment	2011–2016
Cannella et al. (2024)	<i>Ufficio per il processo</i> (PNRR, 2021)	DiD	Civil	Resolved proceedings; backlog	2018–2023
Canzian & Ferrara (2025)	Judicial reform 2012 (court mergers)	DiD, event study	All subject matters	NPL ratio; judicial efficiency	2010–2017
<i>This paper</i>	Court-president turnover (exogenous variation)	Staggered DiD	Civil / Bankruptcy	Bankruptcy proceedings duration; credit supply; investment	2015–2022



Figure 1: Geographical Distribution of Italian Courts

The figure shows the geographical distribution of Italian courts during the 2015–2022 period. The 140 courts are organized into 26 judicial districts and are distributed geographically across all 20 Italian regions. Data Sources: *Italian Ministry of Justice*.

covering civil litigation, labor disputes, and insolvency proceedings, each characterized by markedly different levels of procedural complexity and case duration. Figure 2 documents the substantial heterogeneity in proceeding duration across subject matters. Panel (a) plots the mean duration of the civil caseload across all matter types, which follows a broadly declining trend, falling from 939 days in 2015 to 858 days in 2022. Panels (b) through (i) decompose this aggregate into eight substantive categories.³ The cross-sectional dispersion is striking. At one extreme, special proceedings resolve in under 90 days throughout the sample period, followed by non-contentious matters (under 100 days, increasing), bankruptcy declarations (under 190 days, declining), enforcement against other assets (under 320 days, increasing), employment disputes (under 400 days, declining), and social-security claims (under 680 days, declining). At the other extreme, real estate enforcement exceeds 1,600 days and trends upward, while bankruptcy liquidation is by far the most protracted category, averaging 3,197 days in 2016 and 3,179 days in 2022, roughly three and a half times the mean civil case duration. Of these categories, bankruptcy liquidation is the most directly relevant to credit markets: its duration determines how quickly banks recover defaulted collateral, shaping both credit risk premia and borrower repayment incentives.⁴

This persistent variation in case duration, which remains after controlling for caseload volume and staffing levels, motivates a focus on internal organizational drivers—specifically, the managerial role of court presidents. We focus on court-president turnover because presidents hold extensive organizational authority over judicial administration, with managerial rather than adjudicative powers. Specifically, court presidents allocate cases among judges, supervise

³The eight categories are: bankruptcy declaration (*liquidazione dichiarativa*), covering the formal opening of insolvency proceedings; bankruptcy liquidation (*liquidazione esecutiva*), covering the asset-realization and distribution phase; enforcement against real property (*esecuzione immobiliare*) and against other assets, such as financial instruments and receivables (*esecuzione mobiliare*); employment disputes (*lavoro*); social-security and pension claims (*previdenza*); summary and special proceedings (*procedimenti speciali*), which encompasses expedited civil procedures including payment injunctions and urgent interim relief; and non-contentious jurisdiction (*volontaria giurisdizione*), covering court supervision of private legal arrangements (such as guardianships and succession matters) where no adversarial dispute exists.

⁴In 2020–2021, court activity was severely disrupted by temporary closures and operational constraints imposed in response to the COVID-19 pandemic, making observations in these two years not fully comparable with the rest of the sample.

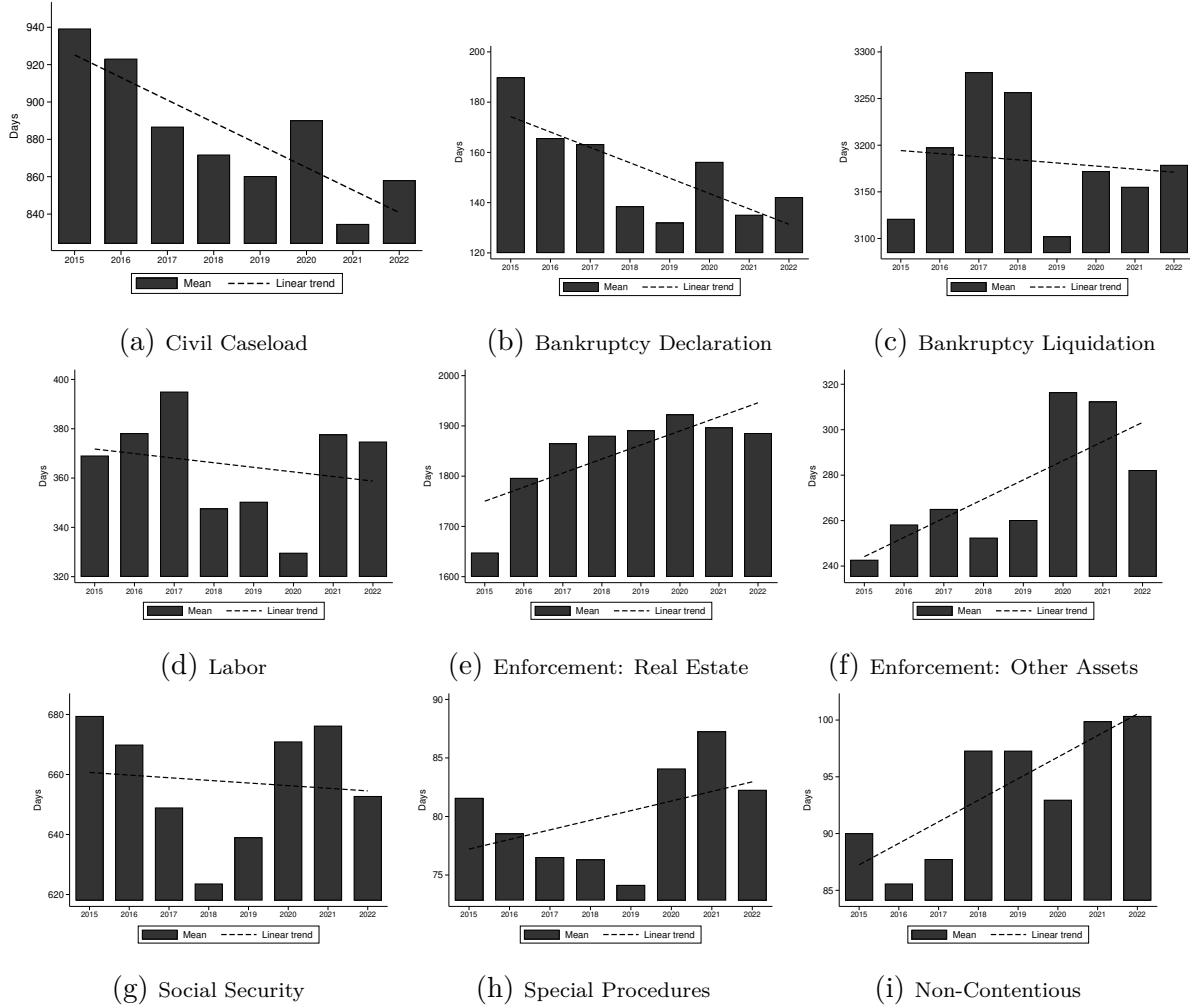


Figure 2: Judicial Performance: Civil vs Bankruptcy Proceedings

The figure plots the evolution of judicial performance in Italy over the 2015–2022 sample period. Panel (a) reports the mean duration in days of civil caseload. Panels (b) and (c) report the mean duration in days of bankruptcy proceedings (declaration and execution phases, respectively). Panel (d) reports the mean duration in days of labor proceedings, while panels (e) and (f) report the mean duration in days of enforcement proceedings (real estate and other assets, respectively). Panel (g) reports the mean duration in days of social security proceedings. Panels (h) and (i) report the mean duration in days of special and non-contentious proceedings, respectively. Each panel reports its respective trend. *Data source:* Italian Ministry of Justice.

judicial and administrative staff, are required to report judicial vacancies to the CSM every six months, issue annual decrees setting individual performance targets for judges, and jointly with the chief prosecutor determine performance-based compensation. These prerogatives of court presidents shape the organizational conditions under which judges operate (Blisa & Kosař 2018, Benvenuti & Paris 2018).

Table 2: Reasons for the Turnover of a Court President

The table reports court-president turnover events in Italy from 2015 to 2022, classified by the underlying reason. Retirement, death, and resignation are never the result of a *Consiglio Superiore della Magistratura* (CSM) decision. Transfers to another office are labeled *hybrid* because they arise from two legally distinct processes: in 9 cases, transfer follows completion of a first mandate and reflects a discretionary CSM decision; in 18 cases, transfer follows completion of a second mandate and is automatic under the statutory prohibition on serving more than two mandates. Data Sources: CSM.

Turnover Reason	Frequency	%	Decision Agency	Explanation
Death	3	2.19	No-CSM	Demise
Early Retirement	8	5.84	No-CSM	Voluntary early exit
Mandate Expiry	4	2.92	No-CSM	Statutory term limit
Resignation	27	19.71	No-CSM	Acceptance of resignation
Retirement Age Limits	68	49.64	No-CSM	Attainment of age limits
Transfer to Another Office	27	19.71	Hybrid	Relocation to new jurisdiction
Total	137	100		

Table 2 summarizes the reasons for all court-president turnovers in Italy between 2015 and 2022. In about half of cases, mandatory retirement age limits trigger replacement, and neither the incumbent nor the CSM can postpone, renegotiate, or accelerate them. Additional non-discretionary causes—death, early retirement, resignation, or expiration of the fixed-term mandate—account for around 31% of cases. The remaining 19.7% involve the transfer of the incumbent to another office; these cases are labeled *hybrid* in Table 2 because they arise from two legally distinct processes. In 18 cases (13.1%), transfer follows the completion of a second consecutive mandate: because Italian regulations cap presidential tenure at two consecutive four-year terms, the eight-year cumulative limit triggers an automatic departure that the CSM has no discretion to defer. In the remaining 9 cases (6.6%), transfer follows the completion of a first mandate and reflects a discretionary CSM judgment: whether to

renew the incumbent conditional on a favorable performance assessment or to appoint a successor. Taken together, 93.4% of all turnovers are therefore triggered by factors outside the CSM’s control—mandatory retirement, death, resignation, fixed-term expiry, or statutory two-term limits—providing a plausibly exogenous source of variation in court leadership. This feature substantially mitigates concerns that judicial outcomes drive leadership turnover (see [Section 6.1](#)).

4 Data Sources and Variables

We construct a panel dataset for Italy over 2015–2022 by combining five data sources: administrative records on court performance from the Italian Ministry of Justice, court-president information from the High Council of the Judiciary (*CSM*), the Central Credit Register (*CR*) maintained by the Banca d’Italia, the *Cerved Group* balance-sheet database, and regional economic statistics from the Italian National Institute of Statistics (ISTAT). Appendix A provides detailed variable definitions.

Our first data source is the Italian Ministry of Justice, which provides annual administrative data on court performance. We measure judicial performance as proceeding duration, the actual time elapsed from filing to closure, rather than the two standard alternatives. Disposition time (pending-to-resolved cases $\times$ 365) assumes constant case-disposal capacity ([Italian Ministry of Justice 2021](#)), an assumption that fails for bankruptcy proceedings, where court-level mean duration varies by over 1,300 days in our sample; [European Commission for the Efficiency of Justice \(2025\)](#) similarly cautions against treating it as a substitute for actual proceeding duration. The clearance rate (resolved-to-incoming cases) is distorted by cohort mismatch: when proceedings are lengthy, cases closed in a given year largely reflect filings from years earlier, so a high clearance rate may simply signal the liquidation of an old backlog rather than genuine efficiency gains, a reason the Italian Ministry of Justice explicitly advises against using this metric to evaluate insolvency courts ([Italian Ministry](#)

of Justice 2021). Following Rodano et al. (2016), we construct proceeding duration both as a level and as an annual growth rate, with the latter as our primary dependent variable. Figure 3 illustrates these conceptual differences for bankruptcy proceedings, the case with the greatest heterogeneity in judicial performance and the most relevant for our study. Comparing the time-series dynamics of proceeding duration and disposition time over 2015–2022, the two indicators diverge markedly: disposition time declines substantially more steeply than actual proceeding duration, confirming that it overstates improvements in court efficiency and validating our choice of proceeding duration as the more reliable measure. We merge court performance data with administrative records from the *CSM*, which cover the universe of 140 Italian courts and provide information on court presidents’ appointments, tenure histories, turnover dates, and the institutional reason underlying each transition.

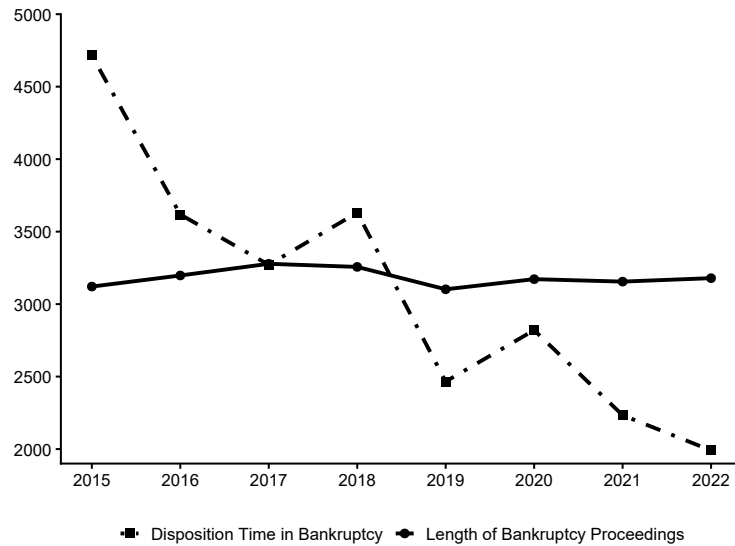


Figure 3: Measuring Judicial Performance: Proceeding Duration vs. Disposition Time

The figure plots the evolution of mean proceeding duration and disposition time for bankruptcy liquidation proceedings (both in days) over the period 2015–2022. Source: Italian Ministry of Justice.

To characterize leadership transitions, we construct three sets of president-specific variables. First, *Age Gap* is the difference in birth years between the incoming and outgoing presidents, and *Incoming President Age* is the incoming president’s age at the time of appointment, in years. Second, *Managerial Experience* equals one if the incoming president previously

held a managerial position, either as president of another court or as president of a judicial section, and zero otherwise. Third, we construct three measures of local ties. *Macro-Region Birth* equals one if the incoming president was born in the same macro-region as the court. *Macro-Region Former Office* equals one if the president’s previous court was located in the same macro-region as the new appointment. Finally, *Neighbor Provinces* equals one if the provinces associated with the president’s previous and current courts share a common border. Together, these variables capture different dimensions of local familiarity and institutional proximity.

Our third data source is the Central Credit Register (CR), a confidential database maintained by the Banca d’Italia that records credit exposures between financial intermediaries and borrowers.⁵ Consistent with [Rodano et al. \(2016\)](#) and [Rodano \(2021\)](#), we restrict attention to corporate borrowers. To isolate credit relationships potentially affected by judicial enforcement, we further require that a bank–firm relationship include at least one outstanding term loan during a given year. This restriction ensures that all observed bank–firm relationships are exposed to creditor recovery risk and therefore potentially affected by changes in bankruptcy-court efficiency. Using the CR, we construct three credit-market variables. *Late Payment* equals one if the firm is more than 90 days past due on any loan within the bank–firm relationship in year t , and zero otherwise. Following [Jappelli et al. \(2005\)](#) and [Rodano \(2021\)](#), *Intensive Margin* is measured as the natural logarithm of one plus short-term credit granted within a bank–firm relationship. Short-term credit represents the primary margin through which banks can adjust credit supply to incumbent borrowers, whereas changes in outstanding term loans largely reflect contractual amortization schedules. Finally, *Extensive Margin* equals one if a bank–firm relationship is newly established in year t , and zero otherwise.

Our fourth data source is the *Cerved Group* balance-sheet database, which provides accounting information for Italian firms. Using these data, we construct three measures of

⁵The €30,000 reporting threshold ensures near-comprehensive coverage of bank–firm credit exposures; for loans classified as *sofferenze* (bad loans) the threshold falls to €250.

firm investment. *Fixed Assets* is the natural logarithm of one plus total fixed assets, defined as the sum of tangible and intangible assets.⁶ *Intangible Asset Share* is the ratio of intangible assets to the sum of intangible and tangible assets, while *Tangible Asset Share* is defined analogously. Firms with no tangible and intangible assets are assigned a value of zero. Finally, we draw macroeconomic and geographical data from ISTAT.

We combine judicial, credit, firm-level, and macroeconomic information using the statutory assignment of insolvency jurisdiction under Italian law. Bankruptcy proceedings are administered by the court responsible for the district in which the debtor’s registered headquarters are located. Using municipality-level jurisdiction records from the *CSM*, we assign each firm in the *Cerved Group* database and each bank–firm relationship in the CR to a unique court. This procedure generates an exhaustive mapping between firms, credit relationships, and judicial districts. [Table 3](#) summarizes the geographical variation in court performance. Consistent with past evidence, substantial differences emerge both across and within regions. In particular, bankruptcy duration varies considerably even among courts operating under the same legal framework, highlighting the organizational heterogeneity that motivates our analysis.

[Table 4](#) reports summary statistics for the main variables. Considerable heterogeneity is present at each level of analysis. Bankruptcy duration ranges from approximately 1,100 to more than 6,600 days across courts. At the credit level, late payments occur in 1.2% of bank–firm relationships, while both intensive and extensive margins vary substantially. At the firm level, asset composition is highly skewed, with intangible assets concentrated among a relatively small subset of firms. Leadership characteristics also vary markedly: the median age gap between incoming and outgoing presidents is nine years, slightly more than half of incoming presidents have prior managerial experience, and measures of local ties vary widely across appointments.

⁶Recent work discusses potential issues arising from log transformations with zeros and count-like outcomes ([Cohn et al. 2022](#), [Chen & Roth 2024](#)). In our setting, the variables of interest are continuous-valued economic quantities whose distributions are largely concentrated away from zero ([Table 4](#)). Accordingly, we use $\log(1+x)$ only to accommodate boundary observations while preserving sample comparability.

Table 3: Length of Civil and Bankruptcy Trials in Italian Regions

The table summarizes the length of civil and bankruptcy trials, in days, in Italian regions and macro-regions during the period 2014–2022. The one-year extension relative to the main sample (2015–2022) accommodates the lagged growth rates used in the analysis. No. Courts refers to the number of courts operating within each region (macro-region). No. Obs denotes the number of observations for each region (macro-region). We obtain this by multiplying the number of courts by nine, the number of years for which data are available. *Panel a* reports descriptive statistics for civil trials, while *Panel b* reports descriptive statistics for bankruptcy trials. For each, Mean denotes the average length of proceedings within each region (macro-region); Median denotes the median length of proceedings; SD denotes the standard deviation; 1st and 99th denote the first and ninety-ninth percentiles of the length of proceedings. Data Sources: *Italian Ministry of Justice*. Data are at the court level.

Region	Courts	Obs	Panel A: Civil Law					Panel B: Bankruptcy: Liquidation				
			Mean	Median	SD	1st	99th	Mean	Median	SD	1st	99th
Abruzzo	8	72	753	676	256	386	1377	3309	3188	1121	1420	6513
Apulia	6	54	1126	1158	170	824	1450	3990	3942	633	2956	6101
Aosta Valley	1	9	461	351	262	293	1113	2140	1893	684	1315	3439
Basilicata	3	27	1557	1587	334	846	1996	4764	4816	775	3059	5792
Calabria	10	90	1386	1344	347	691	2271	3799	3714	1345	782	6785
Campania	10	90	1256	1268	384	194	2257	3479	3349	1300	24	6226
Emilia-Romagna	9	81	670	647	255	278	1613	2020	2010	569	793	3523
Friuli-Venezia Giulia	4	36	503	460	119	333	776	2041	1888	639	804	3491
Lazio	9	81	978	907	226	702	1991	3198	2870	1058	1628	7248
Liguria	4	36	632	666	139	359	894	3111	2866	1208	1313	6110
Lombardy	13	117	536	534	95	374	725	2190	2089	488	1049	3622
Marche	6	54	818	786	227	418	1511	3307	3157	650	2219	5384
Molise	3	27	1062	1033	295	644	1626	3401	3708	1481	191	6336
Piedmont	9	81	498	508	110	281	795	2469	2276	718	1396	4875
Sardinia	6	54	892	882	214	520	1381	3508	3414	1300	1174	9586
Sicily	16	144	1094	1006	449	471	2183	4911	4856	1055	2262	7135
Trentino-Alto Adige/South Tyrol	3	27	453	454	95	305	710	1843	1900	434	932	2581
Tuscany	10	90	835	838	215	269	1307	2550	2399	816	440	5566
Umbria	3	27	966	940	172	724	1316	2347	2621	963	131	3484
Veneto	7	63	792	740	229	495	2039	2463	2410	443	1274	3782
Macro-Regions												
Center	28	252	891	864	228	406	1511	2898	2746	959	440	5384
Islands	22	198	1039	972	408	471	2183	4528	4489	1287	1243	7232
North-East	23	207	650	612	245	305	1256	2136	2128	573	815	3491
North-West	27	243	535	530	124	328	828	2417	2196	782	1160	5278
South	40	360	1176	1188	399	413	2213	3692	3698	1236	508	6464
Total (Italy)	140	1260	888	808	394	331	2048	3163	2856	1310	793	6636

Table 4: **Descriptive Statistics**

This table reports summary statistics of the main variables used in the analysis. Appendix A describes all variable definitions. Data Sources: *CR*, *Cerved Group*, *CSM*, *Italian Ministry of Justice*, and *ISTAT*.

	Observations	Mean	Median	SD	Min	Max
Level 1: Judicial Data						
Age Gap (in years)	1,071	8.236	9.000	5.612	-4.000	19.000
Bankruptcy Duration Days	1,071	3176.487	2855.000	1283.860	1086.000	6639.000
Bankruptcy Duration Growth (in %)	1,071	5.122	0.357	29.332	-50.913	164.459
Civil Duration Days	1,071	876.358	791.000	391.768	342.000	2043.000
Civil Duration Growth (in %)	1,071	-0.094	-1.600	17.229	-30.128	48.643
Managerial Experience	1,071	0.522	1.000	0.500	0.000	1.000
Macro-Region Birth	1,071	0.502	1.000	0.500	0.000	1.000
Macro-Region Former Office	1,071	0.660	1.000	0.474	0.000	1.000
Neighbor Provinces	1,071	0.333	0.000	0.472	0.000	1.000
Level 2: CR Data						
Extensive Margin	1,621,320	0.135	0.000	0.342	0.000	1.000
Intensive Margin	1,621,320	6.286	9.210	5.488	0.000	14.509
Late Payment	1,621,320	0.012	0.000	0.108	0.000	1.000
Level 3: Firm Data						
Fixed Assets	379,353	5.155	5.258	2.280	0.000	10.234
Intangible Asset Share	379,353	0.179	0.025	0.280	0.000	1.000
Tangible Asset Share	379,353	0.791	0.965	0.311	0.000	1.000

5 Identification Strategy

We test our research hypotheses in three steps. First, we analyze court-level data to determine whether the president’s turnover reduces bankruptcy trial durations, indicating improved judicial performance (H_1). Then, we examine whether the age difference between the outgoing and incoming presidents affects changes in trial duration (H_{2A}). We further examine how prior managerial experience (H_{2B}) and local ties (H_{2C}) — measured by birthplace macro-region, the former-office macro-region, and shared provincial border — shape these outcomes. In the second step, we analyze CR data at the bank–borrower level to assess whether higher performance improves lending in quantity and/or quality. Specifically, we test our (H_3) hypotheses to understand whether improved judicial performance (and thus better protection of creditors’ rights) reduces borrowers’ moral hazard, leading to a higher likelihood of timely

repayment (H_{3A}) and increased credit to existing borrowers (H_{3B}) or new borrowers (H_{3C}). In the third step, we use firm-level data to evaluate the real effects of improved judicial systems by assessing their impact on firm investments (H_{3D}).

Consistent with past papers (Braghieri et al. 2022, Denes et al. 2023, Cornaggia et al. 2024, Brynjolfsson et al. 2025, Mensah et al. 2025, Avenancio-León et al. 2025, Cassella et al. 2026), we employ a staggered DiD approach, following Sun & Abraham (2021). Their method estimates the effects of a treatment applied to different units at different points in time, while addressing potential complications arising from heterogeneous treatment effects and spurious cohort differences that could bias the results. The approach proposed by Sun & Abraham (2021) mitigates these risks, allowing for a cleaner identification of the treatment effect. Accordingly, we specify the following baseline model:

$$y_{c,t} = \sum_{l=n}^N \Phi_l \cdot I\{t - turnover_c = l\}_{c,t} + \theta_c + \gamma_t + \varepsilon_{c,t} \quad (1)$$

The sum term in equation (1) represents our main variables of interest. It summarizes the independent variables and, in particular, collects a set of binary variables (I), which take the value one depending on the distance (l) between the year (t) and the year in which the change of the president of the court occurred ($turnover$). Because event time is defined relative to the treatment year, with leads denoting pre-treatment periods and lags denoting post-treatment periods, we must omit one period to avoid perfect collinearity among the event-time dummy variables. Following standard practice (Sun & Abraham 2021), we set the last pre-treatment period ($t-1$), i.e., the year immediately before the court president's turnover, as the reference category. This choice facilitates interpretation of the coefficients, as they are expressed relative to the period immediately preceding the treatment.

Our empirical strategy exploits temporal variation within courts. Court fixed effects (θ_c) net out all time-invariant heterogeneity across courts, and time fixed effects (γ_t) account for shocks common to all courts in a given year. Hence, we identify coefficients from within-court

changes around the turnover of the court president, rather than from cross-sectional differences across courts or judicial districts. The three analytical stages differ in their dependent variables, definitions of treated and control units, and included fixed effects, in line with the respective hypotheses. At all stages, we estimate an event-study model, restricting the event window to $n = -4$ and $N = 4$, which preserves statistical power and reflects the premise that performance improvements from a new court president should emerge within the first mandate after appointment.

Regarding the outcome variables (y), we proceed in three steps. In the first step, the dependent variable is the year-on-year change, expressed in percentage points, in the average length of judicial proceedings,⁷ calculated both for the total civil caseload and for each subject category listed in Appendix A. In the second step, we employ three variables: *Late Payment*, measuring firms’ incentives to repay debt promptly; *Extensive Margin*, reflecting banks’ willingness to establish new credit relationships; and *Intensive Margin* capturing the scale of existing lending (see Section 4 and Appendix A for details). In the third step, we focus on firms’ long-term capital: the *Fixed Assets* measures changes in investment scale, while the shares of tangible and intangible assets capture compositional adjustments within fixed capital. The interpretation of the estimated coefficients varies with the outcome variable. When the outcome is a growth rate (*Bankruptcy Duration Growth*), $\hat{\phi}_l$ identifies the per-period differential in the rate of change of judgment days between treated and control courts at event time, net of court-specific average growth and common time-varying shocks absorbed by the fixed effects. We therefore interpret $\hat{\phi}_l$ throughout as an effect on the rate of change rather than on the level of judgment days. Recovering a level-equivalent magnitude would require cumulating $\hat{\phi}_l$ along the event window, anchoring it to a pre-treatment benchmark, and imposing assumptions on the persistence of the growth differential and on the absence of dynamic feedback between the treatment and the baseline trajectory—none of which are maintained by our identification strategy. When the outcome is a log-transformed variable

⁷The year-on-year change in judicial performance has also been adopted by [Giacomelli et al. \(2017\)](#) and [Pezone \(2023\)](#).

(*Intensive Margin; Fixed Assets*), $\hat{\phi}_l$ approximates a differential percentage change. When the outcome is a binary indicator (*Late Payment; Extensive Margin*) or a share (tangible and intangible asset shares), $\hat{\phi}_l$ is expressed in percentage points and can be scaled by the mean of the outcome to obtain the corresponding relative effect.

The definition of the treatment group varies across the three stages of the analysis because sample sizes differ, while the control group remains constant across all stages.

First, we focus on the court-level setting: we compare the evolution of bankruptcy-proceeding duration in courts that experience a president turnover with that in courts that do not. A key feature of the institutional setting is that more than 93.4% of turnover events are triggered by factors outside the *CSM*'s discretion, such as mandatory retirement, death, or resignation. Because these events are predetermined or idiosyncratic, they are unlikely to reflect local economic conditions, court-specific congestion, or prior judicial performance. This institutional structure limits the scope for endogenous timing of leadership transitions and provides a plausible source of exogenous variation in court leadership. The control group comprises courts that did not experience a turnover in their president during the sample period (namely, Novara, L'Aquila, Vasto, Bolzano, Trento, and Oristano), which are contrasted with other courts in Italy where the president did change.⁸ Notably, our sample period 2015-2022 covers eight years, the maximum duration for which the court president remains unchanged: under Italian law, court presidents serve in their managerial roles (*incarichi direttivi*) for a four-year term, which can be renewed once at the same court. Consequently, a president at a given court may serve a maximum of 8 years.

Once the eight-year limit is reached, the sitting president is required to leave office, triggering a compulsory change in leadership that does not depend on the results of court cases or local economic conditions. If a court experiences more than one turnover event over the sample period, we retain only the first. The estimates of the parameters associated with these dummies (Φ_l) will therefore measure the evolution over time of the treatment effect,

⁸As a robustness check, we shorten the sample period to seven years, which allows us to expand the control group to 9 courts over 2015–2021 and to 13 courts over 2016–2022.

which in our case corresponds to the turnover of the president of the court, relative to the categories omitted, i.e., the dummy indicating the observations never treated and the dummy indicating the year just before the treatment. To absorb unobserved heterogeneity at the court level and in a given year, we include court fixed effects (θ_c) and time fixed effects (γ_t). Finally, $(\varepsilon_{c,t})$ is the court-level error term.

In the second and third steps, we rely on bank-borrower and firm balance sheet data, which provide larger samples and sharper identification. At these stages, sample construction affects only the composition of the treated group, not the source of identification. The control group always comprises courts whose presidents did not change during the sample period; the treatment group comprises courts in the same judicial districts where a presidential turnover occurred. We therefore restrict the analysis to firms associated with (treated) courts in districts containing at least one never-treated court, rather than using the full universe of Italian courts as in the first step. As usual, if a court experiences more than one turnover event over the sample period, we retain only the first. Under this constraint, the analysis covers 27 courts across four judicial districts.⁹ The large sample size allows us to incorporate a rich set of fixed effects to capture economic, social, and demographic variation across regions. For instance, areas facing more severe economic distress may experience higher bankruptcy rates; in courts with constrained capacity, this can lead to longer processing times, further exacerbating local conditions. Likewise, in distressed regions, courts often have difficulty promptly liquidating debtors' assets for creditors, thereby prolonging case resolution. In the second stage of our credit-channel analysis, we therefore maintain court fixed effects (θ_c) and time fixed effects (γ_t). In extended specifications of equation (1), we gradually add bank fixed effects (λ_b) and sector fixed effects (β_i), where (i) indexes the industry in which firm (f) operates, to account for unobserved bank-level heterogeneity and structural sector characteristics. These controls account for systematic differences that may

⁹For example, the Turin district encompasses ten courts (Alessandria, Aosta, Asti, Biella, Cuneo, Ivrea, Novara, Turin, Verbania, and Vercelli). Novara registered no president turnover during the sample period and therefore belongs to the control group; the remaining nine courts in the Turin district form the treated group. We apply the same logic to every district containing at least one never-treated court.

affect the dependent variables. The error term ($\varepsilon_{f,b,t}$) is clustered at the court–year level. In the third stage, which examines investment outcomes, we include court fixed effects, firm fixed effects, and area-by-year fixed effects to capture time-varying shocks or trends specific to each macro-area (North-East, North-West, South, and Islands) that might shape firms’ investment decisions. In this specification, the error term ($\varepsilon_{f,t}$) is likewise clustered at the court–year level.

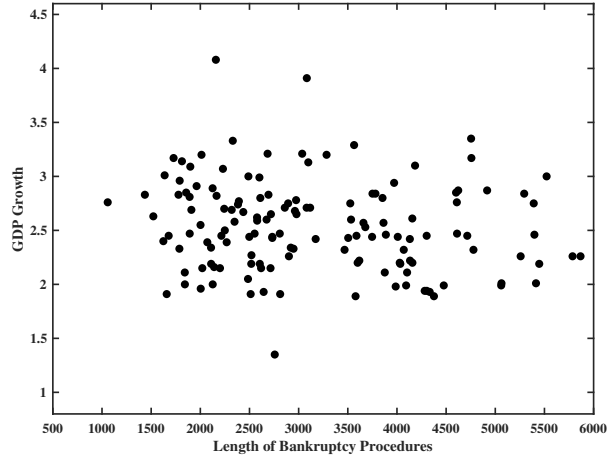
6 Results

6.1 Is Court President Turnover an Exogenous Shock?

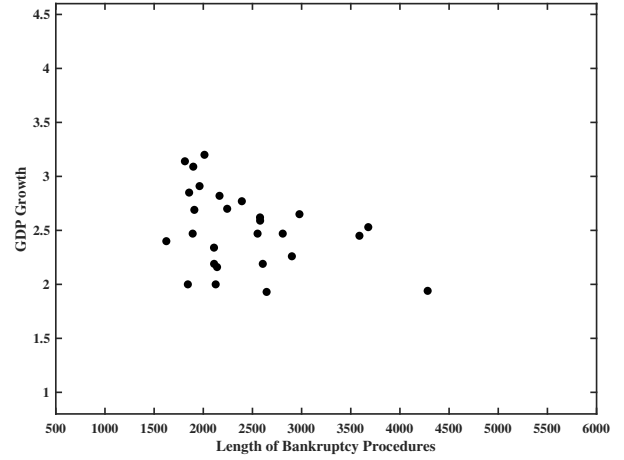
A key feature of our empirical setting is that turnover among Italian court presidents is driven mainly by institutional rules or the outgoing president’s own decision, rather than by court performance or the CSM’s strategic choices. As documented in [Table 2](#), turnover most often occurs because of retirement—either upon reaching the mandatory retirement age (49.6% of cases) or through early retirement (5.8%)—or because of death (2.2%), mandate expiry (2.9%), resignation (about 19.7%), or reassignment to a different court at the conclusion of the second term (13.1%). Only a small share of turnovers (fewer than 6.6%) stems from a discretionary decision by the CSM, namely a “transfer to another office” after the first mandate. Thus, in 93.4% of cases, turnover operates as a deterministic, rule-driven timing shock that generates credible quasi-experimental variation for identifying the causal impact of changes in court leadership. This institutional framework precludes courts from strategically adjusting the timing of leadership changes in response to local shocks, fluctuations in caseload pressure, congestion, or broader economic conditions. As a result, turnover timing is orthogonal to both judicial performance and the economic outcomes we examine—specifically, credit supply and firms’ investment. This institutional environment closely resembles that studied in recent empirical contributions ([Ash & MacLeod 2024](#), [Shumway & Wilson 2022](#)), in which fixed retirement ages and statutory term limits provide plausible exogenous sources of variation.

To evaluate whether our identification strategy may be affected by a small number of potentially endogenous cases (9 out of 137), we examine the relationship between court-president turnover and indicators of court performance and local economic conditions. We begin by presenting descriptive evidence on the relationship between provincial economic conditions and judicial performance across courts. [Figure 4](#) shows, for each province hosting a court, the average duration of bankruptcy proceedings, the category with the longest mean duration across all proceeding types, alongside GDP growth over 2015–2022. Visual inspection of these patterns reveals no discernible association between leadership changes and local economic performance, suggesting that court-president turnover does not systematically coincide with particularly favorable or adverse economic environments. This preliminary evidence supports the assumption that turnover in court leadership is plausibly exogenous with respect to provincial economic and judicial outcomes.

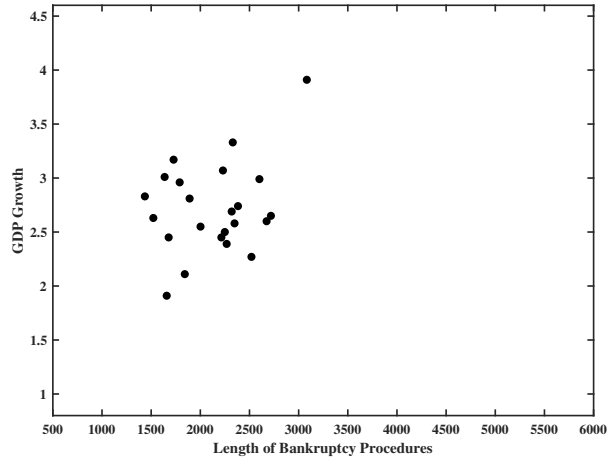
Second, we complement the graphical evidence by estimating a linear probability model using court–year panel data for 2015–2022. The dependent variable is an indicator equal to one when the court president changes. The key regressors capture court performance in levels (the log of the average duration of bankruptcy proceedings) or in growth (the year-over-year change in the average duration). We also control for the province’s GDP growth rate. Court performance and GDP growth are lagged by one and two years relative to the court president’s turnover. In all specifications, we also include fixed effects for court, year, and reason for turnover. Our results, presented in [Table 5](#), indicate that leadership changes are not systematically associated with prior court performance (columns 1 and 2). This finding is consistent across a broad set of subsamples, including those restricted to incoming presidents whose age at appointment falls below the median, the first quartile, or the first decile (columns 3–8).



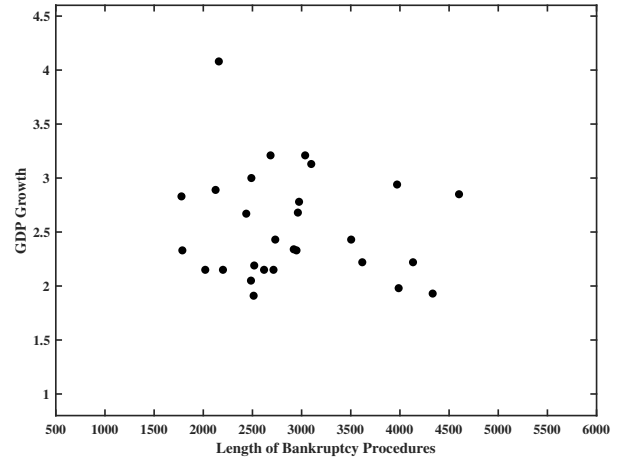
(a) Italy



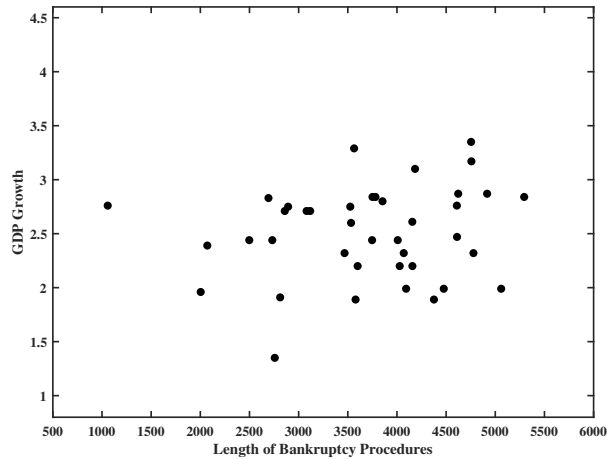
(b) North-West



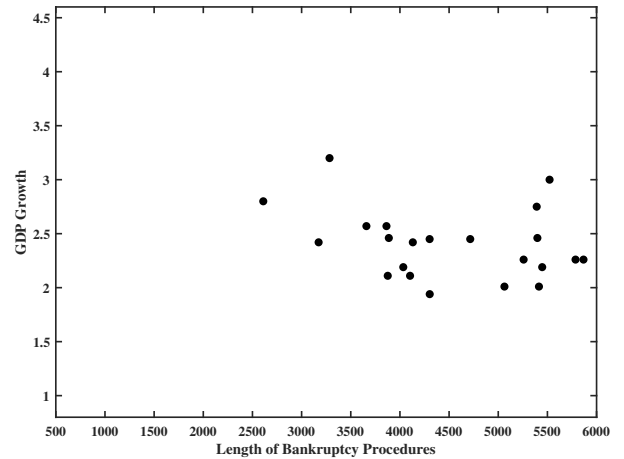
(c) North-East



(d) Center



(e) South



(f) Islands

Figure 4: Duration of Bankruptcy Procedures and Economic Growth

The figures show the correlation between the mean duration (in days) of bankruptcy liquidation proceedings in each court and the GDP growth rate across all provinces with courts (expressed in percentage points) over the period 2015-2022. Both variables are winsorized at the 1-percent level. Data Sources: *Italian Ministry of Justice*, *CSM*, and *ISTAT*.

Table 5: **Is Court President's Turnover Related to Court Performance?**

The table reports estimates from a linear probability model in which the dependent variable is *Turnover*. The key regressors are *Bankruptcy Liquidation (Days)* in columns (1), (3), (5), and (7), and *Bankruptcy Liquidation (Growth)* in columns (2), (4), (6), and (8). All specifications include court, year, and turnover-reason fixed effects. Columns (3)–(4) restrict the turnover definition to incoming presidents appointed below the first-quartile age; columns (5)–(6) further limit it to those below the first decile; and columns (7)–(8) apply a median-age threshold. Accordingly, in columns (3)–(8), the dependent variable equals 1 if the incoming president's age at appointment falls below the specified threshold, and 0 otherwise. Standard errors, shown in brackets, are clustered at the court level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CSM*, *ISTAT*, and *Italian Ministry of Justice*.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Turnover	Turnover	Turnover	Turnover	Turnover	Turnover	Turnover	Turnover
Bankruptcy Liquid. (Days) $_{t-1}$	0.049 (0.048)		0.041 (0.031)		0.011 (0.017)		0.045 (0.052)	
Bankruptcy Liquid. (Days) $_{t-2}$	-0.033 (0.040)		0.000 (0.025)		0.020 (0.016)		0.009 (0.041)	
Bankruptcy Liquid. (Growth) $_{t-1}$		0.058 (0.048)		0.030 (0.021)		-0.003 (0.007)		-0.010 (0.045)
Bankruptcy Liquid. (Growth) $_{t-2}$		0.023 (0.049)		-0.000 (0.018)		-0.001 (0.007)		-0.020 (0.033)
GDP Growth $_{t-1}$	0.001 (0.006)	-0.003 (0.007)	0.000 (0.004)	-0.002 (0.003)	-0.001 (0.002)	-0.002 (0.002)	0.004 (0.006)	-0.000 (0.005)
GDP Growth $_{t-2}$	0.001 (0.006)	0.004 (0.007)	-0.001 (0.004)	-0.003 (0.004)	-0.002 (0.003)	-0.004 (0.003)	0.003 (0.005)	0.005 (0.005)
Observations	980	840	980	840	980	840	980	840
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reason-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Turnover based on	All	All	< 1 st Age Quartile	< 1 st Age Quartile	< 1 st Age Decile	< 1 st Age Decile	< Age Median	< Age Median

6.2 Does Leadership Turnover Shorten Bankruptcy Proceedings?

This section tests our first two hypotheses by examining whether court leadership transitions affect the duration of judicial proceedings. We estimate the staggered difference-in-differences model in equation (1) using the [Sun & Abraham \(2021\)](#) estimator to identify heterogeneous treatment effects across cohorts and event times. Following the approach outlined in [Section 5](#), we begin with the aggregate civil caseload and then estimate the model separately for each of the eight substantive categories, to account for the substantial heterogeneity in proceeding duration documented in [Figure 2](#). The full results, reported in Appendix B, show that leadership transitions have no statistically significant effect on the mean duration of proceedings in any category except bankruptcy liquidation. Importantly for our research aims, this category has the greatest direct bearing on credit markets and firm investment, as the speed of asset liquidation determines bank recovery rates, shapes borrower repayment incentives, and governs access to external finance.

Three mechanisms explain why court-president turnover affects bankruptcy liquidation proceedings while leaving other civil litigation largely unaffected. First, bankruptcy cases are substantially more complex than standard civil disputes, requiring coordination among creditors, debtors, court-appointed receivers, and presiding judges over extended time horizons. This complexity affords the court president direct managerial leverage through judge assignment, case-handling priorities, and oversight of the insolvency section (*sezione fallimentare*), levers largely absent in ordinary civil proceedings where throughput depends primarily on caseload volume and staffing levels. Second, the scope for efficiency gains is greatest in bankruptcy: average durations are about three times those of civil matters, and persistent backlogs give an incoming president stronger incentives and broader scope to reallocate resources and redesign internal workflows. Third, the financial and reputational stakes of bankruptcy proceedings, where delays directly impair bank recovery rates and distort credit allocation, make this category particularly salient for a new president seeking to establish administrative credibility. Court-president turnover thus operates primarily

through a managerial channel, most potent where organizational complexity and entrenched inefficiency are greatest, consistent with [Mocetti et al. \(2026\)](#), who find that efficiency gains from judicial reforms are largest in complex proceedings such as bankruptcy. The remainder of the paper focuses on bankruptcy liquidation proceedings, the duration of which is hereafter referred to as *Bankruptcy Duration*. For completeness, [Table 6](#) reports estimates for both the aggregate civil caseload and bankruptcy liquidation.

The growth rate of bankruptcy duration evolves similarly across treated and control courts prior to the president’s turnover, supporting the parallel trends assumption. Treated courts exhibit a significantly lower growth rate of bankruptcy duration relative to control courts, with the differential remaining statistically significant from year two through year four. The estimated differential in the growth rate of bankruptcy duration grows steadily from -6.8% in year one to -15.5% by year four. This implies a nearly 11.5% gap in the annual growth rate of bankruptcy duration between courts with turnover and those without.¹⁰ Since the outcome variable is the growth rate of duration, the estimated gap between treated and control courts is a second-order effect: it measures the difference in the rates at which duration changes across the two groups, or equivalently, the deceleration in duration growth in treated courts relative to control courts attributable to leadership turnover. This distinction matters for interpretation: a negative differential does not mechanically imply an absolute reduction in duration, as both groups may exhibit positive growth rates, with treated courts growing more slowly. If never-treated courts were themselves improving (i.e., registering negative growth), the negative differential would instead indicate that treated courts improve more slowly. To address this concern, we compute the mean annual growth rate of duration for courts that never experienced a turnover, which equals 11.4% . Since never-treated courts display markedly positive duration growth, the negative differential for treated courts reflects a genuine deceleration: treated courts still experience positive duration growth, but at a substantially lower rate than control courts. The economic magnitude is substantial, corresponding to approximately one-third

¹⁰This is computed as the geometric mean of the horizon-specific estimates: $\bar{g}_{t,t+k} = \left(\prod_{l=1}^k (1 + \hat{\phi}_l)\right)^{1/k} - 1$.

Table 6: Court President's Turnover and the Duration of Bankruptcy Proceedings

This table reports estimates from equation (1). The dependent variable is *Civil Caseload (Growth)* in Panel (a) and *Bankruptcy Liquidation (Growth)* in Panel (b). The notation t indicates the year in which the new court president assumes office. All variables are described in Appendix A. The model is progressively saturated with region (column 1), court (column 2), court-and-year (column 3), and court-year-and-reason-for-turnover fixed effects (column 4), as detailed in Table 2. Column (5) excludes potentially endogenous turnovers, specifically CSM-initiated transfers following the first mandate. Standard errors, reported in brackets, are clustered at the court level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data sources: CSM, and Italian Ministry of Justice.

	(1)	(2)	(3)	(4)	(5)
Panel (a)	Civil Caseload Growth	Civil Caseload Growth	Civil Caseload Growth	Civil Caseload Growth	Civil Caseload Growth
t-4	-0.414 (1.872)	-2.343 (2.330)	-0.193 (2.878)	-0.198 (2.890)	-1.131 (2.769)
t-3	-1.521 (1.246)	-1.624 (1.584)	-3.963* (2.206)	-3.952* (2.220)	-3.765* (2.200)
t-2	-0.978 (1.442)	-0.816 (1.603)	-1.661 (2.104)	-1.659 (2.113)	-2.039 (2.064)
t	-0.539 (1.321)	-0.308 (1.313)	-1.187 (1.899)	-1.097 (1.926)	-0.431 (1.686)
t+1	-0.317 (1.394)	-0.177 (1.458)	-0.878 (1.933)	-0.711 (1.957)	-0.228 (1.823)
t+2	0.260 (1.408)	0.738 (1.446)	-0.870 (2.128)	-0.885 (2.126)	-0.019 (2.065)
t+3	-1.946 (1.532)	-1.021 (1.605)	-1.213 (2.247)	-1.066 (2.320)	-0.732 (2.294)
t+4	-0.594 (1.683)	0.196 (1.791)	-1.859 (2.319)	-1.963 (2.311)	-1.690 (2.349)
Panel (b)	Bankruptcy Liquid. Growth	Bankruptcy Liquid. Growth	Bankruptcy Liquid. Growth	Bankruptcy Liquid. Growth	Bankruptcy Liquid. Growth
t-4	-4.903 (6.758)	-3.646 (6.178)	3.267 (7.454)	3.214 (7.476)	0.867 (6.995)
t-3	4.380 (4.525)	4.983 (4.829)	-8.938 (6.475)	-8.582 (6.471)	-6.259 (6.078)
t-2	0.445 (4.301)	2.202 (4.690)	-7.395 (5.992)	-7.201 (6.013)	-4.331 (5.741)
t	-1.354 (3.200)	-0.376 (3.337)	-4.007 (4.746)	-4.200 (4.746)	-2.845 (4.291)
t+1	-6.897** (2.668)	-5.891** (2.823)	-6.789* (3.991)	-5.760 (3.926)	-5.176 (3.623)
t+2	-6.661*** (2.490)	-3.653 (2.378)	-10.454** (5.043)	-10.149** (5.076)	-7.967* (4.633)
t+3	-5.780* (3.103)	-2.938 (3.061)	-12.723*** (4.877)	-12.656** (4.897)	-9.276** (4.570)
t+4	-7.776*** (2.761)	-5.680* (2.881)	-15.539*** (5.178)	-15.764*** (5.209)	-12.300** (4.974)
Observations	1071	1071	1071	1071	1071
Region-FE	Yes	No	No	No	No
Court-FE	No	Yes	Yes	Yes	Yes
Year-FE	No	No	Yes	Yes	Yes
Reason-FE	No	No	No	Yes	No
Turnover cases	All	All	All	All	No CSM

of the standard deviation of the *Bankruptcy Duration Growth* among courts experiencing a turnover. For example, consider a court with an average bankruptcy duration of 3,000 days before the president’s transition. Under the average growth trajectory observed among never-treated courts (11.4% annually), the duration would reach approximately 4,620 days after four years. Following a turnover, the estimated differential implies a markedly flatter trajectory: applying the annual post-turnover differentials of -6.8% , -10.5% , -12.7% , and -15.5% to the same baseline yields a predicted duration of approximately 2,996 days, computed as $[(1.114 - 0.068) \times (1.114 - 0.105) \times (1.114 - 0.127) \times (1.114 - 0.155)] \times 3000\text{days}$. This yields a 35.1% shorter duration relative to the counterfactual trajectory implied by the control group.¹¹ These magnitudes are broadly comparable to the reduction in bankruptcy proceedings estimated by [Mocetti et al. \(2026\)](#) for the court geography reform, and to the 25% decline in disposition time reported by [Peyrache & Zago \(2024\)](#) for Italian civil proceedings over 2005–2012. [Li & Ponticelli \(2022\)](#) report a comparable effect, documenting a 36% reduction in the duration of insolvency proceedings following the staggered introduction of specialized bankruptcy courts in China, indicating that judicial specialization can yield substantial gains in procedural efficiency.

These findings strongly support our first research hypothesis. As shown in [Table 2](#), most turnover cases are due to existing rules or decisions of the outgoing president and are thus unrelated to the court’s performance. In [Section 6.1](#), we corroborate this logic with empirical evidence showing that turnover in the court’s president is an exogenous shock, unrelated to the province’s economic and judicial conditions. To further rule out the possibility that turnover may be an endogenous shock — potentially driven by the duration of bankruptcy proceedings in the court — we exclude from the sample all courts where the turnover is a discretionary decision of the *CSM* at the end of the first mandate (i.e., 9 cases of transfer to another position). The results shown in column (5) remain strongly consistent with the main

¹¹These comparisons should be read as illustrative, as all rely on the observed control-group trajectory as the counterfactual, but they indicate that leadership turnover is a quantitatively meaningful lever for reducing judicial delay.

findings in the other columns, indicating that the *CSM* decisions do not drive our primary results.

We conduct two additional robustness checks on the judicial performance results. First, we assess whether the small size of the never-treated group drives our findings. The baseline specification comprises only six never-treated courts; all remaining 134 courts undergo at least one transition over the 2015–2022 window. Re-estimating the baseline model on two narrower windows—2015–2021 and 2016–2022—reclassifies as never-treated those courts whose sole turnover falls outside the window, expanding the control group to nine and thirteen courts, respectively.¹² The richer counterfactual comes at the cost of dropping boundary-year observations: 140 observations from 2015 and 118 from 2022. Panels (a) and (b) of [Figure 5](#) show that the average post-turnover coefficients are stable in sign, magnitude, and statistical significance relative to the baseline. Second, retaining only the first turnover event for courts that experience multiple transitions may introduce a composition bias at longer horizons, as courts with a second turnover within four years are progressively dropped from the post-treatment sample. To address this, we re-estimate the baseline model on the subsample of courts that experience exactly one turnover over the full sample period (112 courts, 896 observations). Panel (c) of [Figure 5](#) confirms that the estimates are stable in sign, magnitude, and statistical significance. These findings are consistent with [Boyne et al. \(2011\)](#), who show that managerial turnover can serve as a corrective mechanism in the public sector when pre-existing inefficiencies are substantial, and complement [Bloom et al. \(2020\)](#), who document persistent effects of managerial interventions on organizational performance. Moreover, our findings could be consistent with a queue-up mechanism described by [Coviello et al. \(2015\)](#), whereby turnover among court presidents may induce courts to reallocate case-processing priorities toward older pending proceedings. However, we are unable to test this hypothesis due to the lack of granular data needed to characterize the within-court distribution of case durations.

¹²The 2015–2021 window adds Gorizia, La Spezia, and Lucca to the original six; the 2016–2022 window adds Belluno, Caltagirone, Cosenza, Lecco, Nuoro, Vallo della Lucania, and Vicenza to the original six.

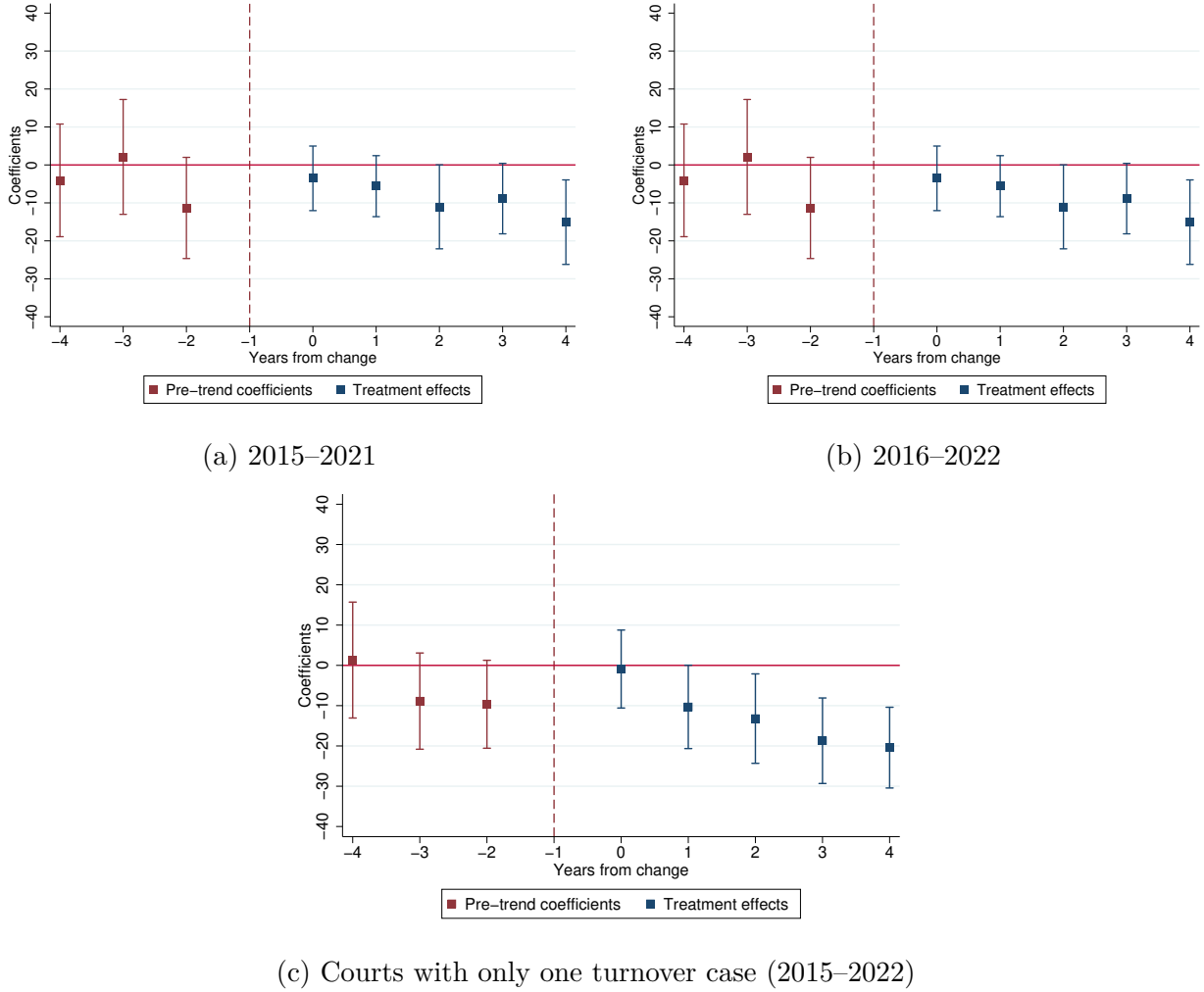


Figure 5: Court-President Turnover and Bankruptcy Duration: Alternative Control Groups

The figure reports dynamic difference-in-differences estimates of equation (1) from $t - 4$ to $t + 4$, estimated under alternative sample restrictions. Panel (a) restricts the sample to 2015–2021; Panel (b) restricts the sample to 2016–2022; Panel (c) restricts the sample to courts that experience exactly one turnover over 2015–2022. All specifications include court and year fixed effects. The dependent variable is *Bankruptcy Duration Growth*. t denotes the year in which the incoming court president assumes office. Confidence intervals are at the 95% level. Data sources: *CSM* and *Italian Ministry of Justice*.

6.3 Which Forms of Turnover Yield the Largest Benefits?

In this section, we examine our remaining research hypothesis to provide policymakers with insight into which forms of turnover yield the largest benefits. While the decision to replace a president is exogenous and thus beyond policymakers' direct influence, the choice of the

incoming president lies entirely with the *CSM*. Our hypotheses examine the magnitude of organizational shock, focusing on changes in the managerial profile arising from heterogeneity between outgoing and incoming presidents across three main dimensions: age, experience, and local ties.

First, we estimate equation (1) separately for four subsamples defined by the incoming president’s age. We begin with the *Age Gap* between incoming and outgoing presidents, split treated courts at the median gap of nine years, and retain the same control group of never-treated courts throughout.¹³ Figure 6 shows that efficiency gains are concentrated among large age-gap turnovers (Ash & MacLeod 2024, Bertrand et al. 2019): when the incoming president is at least nine years younger than the outgoing president (Panel b), the estimated decline in bankruptcy duration steadily increases from -14.1% in the first year to -24.8% in the fourth year, corresponding to a 20.2% differential in the annual growth rate of bankruptcy duration between treated and control courts—nearly two-thirds of the standard deviation of the *Bankruptcy Duration Growth* among treated courts at the time of treatment. Turnovers with a below-median age gap yield no discernible improvement (Panel a), supporting H_{2A} . The age gap, however, captures generational distance rather than the incoming president’s absolute career horizon: a large gap may reflect an unusually old outgoing president rather than an unusually young successor. To test the career-concern mechanism more directly, we re-estimate equation (1), splitting treated courts by the incoming president’s absolute age at appointment, using the sample median of 62 years. Panels (c) and (d) of Figure 6 confirm that performance gains are concentrated among turnovers involving younger incoming presidents, consistent with stronger career incentives driving the observed efficiency improvements.

Second, we turn to prior managerial experience. We estimate equation (1) separately for turnovers in which the incoming president has prior managerial experience and those in which the incoming president has none, retaining the same control group throughout.

¹³The median threshold is preferred over higher cutoffs (e.g., quartiles), which would yield subsamples too small for reliable estimation.

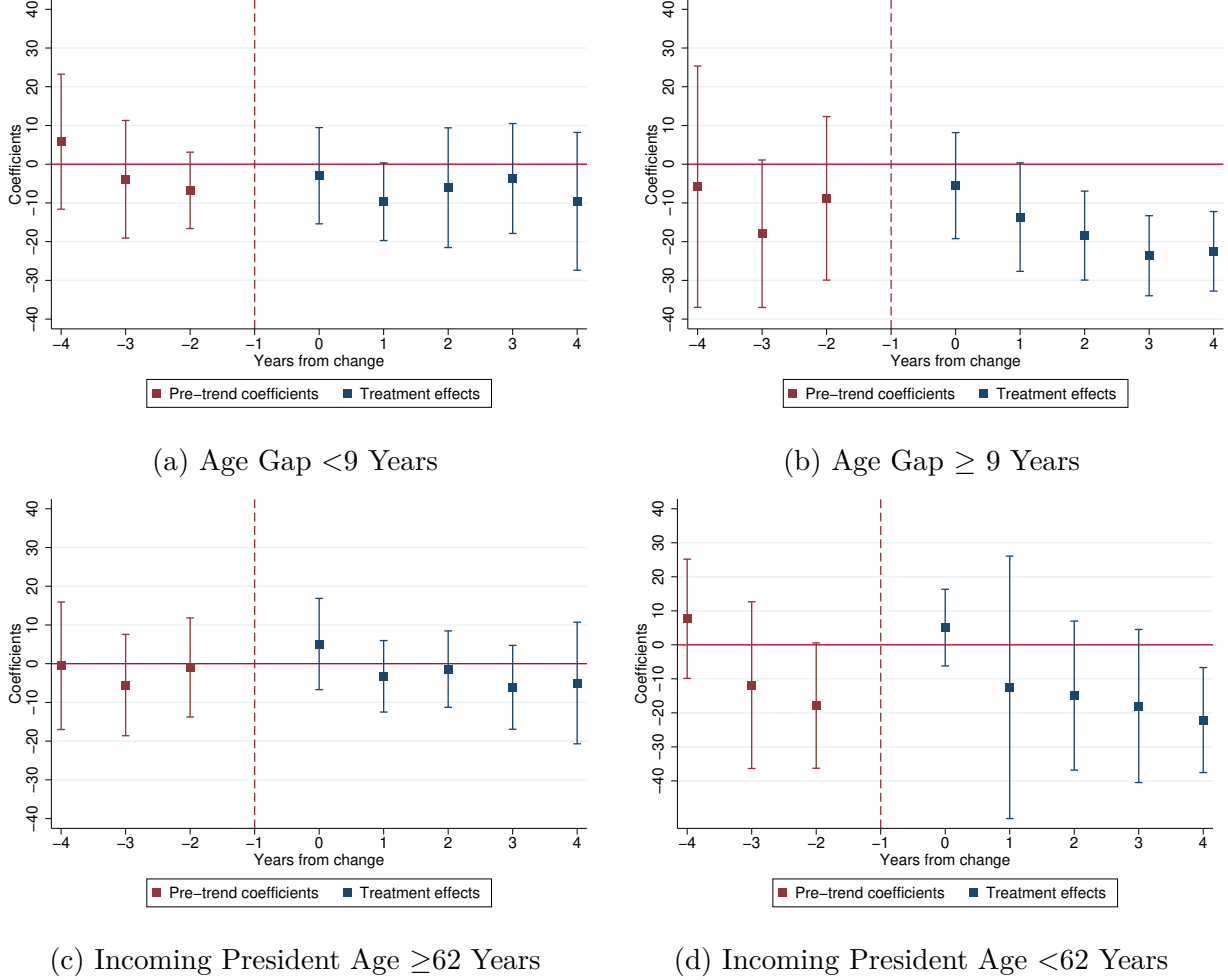


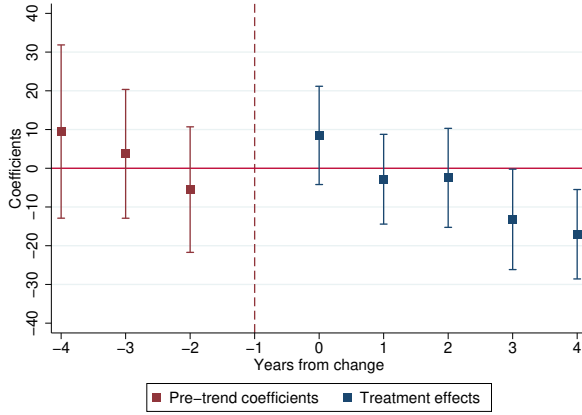
Figure 6: Court-President Turnover and Bankruptcy Duration: Age

The figure reports dynamic difference-in-differences estimates of equation (1) from $t - 4$ to $t + 4$, estimated separately for subsamples defined by the age gap between the incoming and outgoing presidents and by the incoming president's age at appointment. Panel (a) restricts to turnovers with an *Age Gap* below the sample median (< 9 years); Panel (b) to turnovers at or above the median (≥ 9 years); Panel (c) to turnovers in which the incoming president's age at appointment is at or above the median (≥ 62 years); Panel (d) to turnovers in which the incoming president's age at appointment is below the median (< 62 years). All specifications include court and year fixed effects. The dependent variable is *Bankruptcy Duration Growth*. t denotes the year in which the incoming court president assumes office. Confidence intervals are at the 95% level. Variable definitions follow Appendix A. Data sources: *CSM* and *Italian Ministry of Justice*.

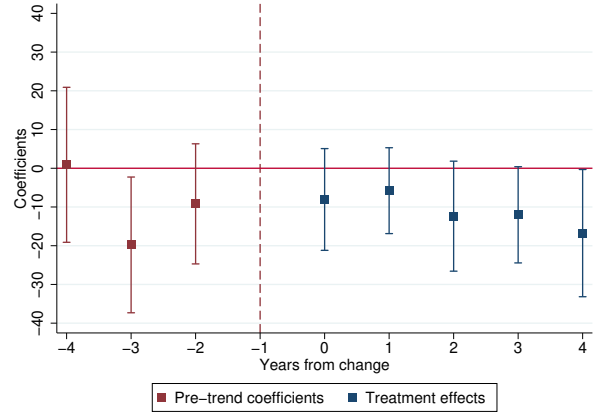
In principle, both types of appointments may improve court performance, albeit through distinct channels: experienced presidents bring organizational knowledge and reform capacity, whereas inexperienced ones may introduce fresh perspectives and face stronger incentives to demonstrate competence through organizational change. [Figure 7](#) shows that performance gains following leadership turnover are concentrated among courts in which the incoming president has prior managerial experience (Panel a). In this subsample, the effect materializes after the third post-turnover year: the estimated differential in the annual growth rate of bankruptcy duration is -13.2% in year three and -17.0% in year four, indicating sustained improvements in judicial performance over time. In contrast, when the incoming president lacks prior managerial experience (Panel b), we do not find statistically significant post-turnover effects on the growth rate of bankruptcy duration. The lack of observable improvements in this subsample indicates that leadership changes alone are insufficient to produce lasting efficiency gains without the managerial capabilities required to implement organizational change. Overall, this pattern corroborates hypothesis H_{2B} and aligns with prior evidence that managerial experience influences organizational performance by equipping leaders with the skills needed to implement effective interventions ([Graham et al. 2013](#), [Custódio & Metzger 2014](#), [Benmelech & Frydman 2015](#)).

Third, we examine whether local ties moderate performance gains from leadership turnover, capturing local ties via three binary indicators: *Macro-Region Birth*, *Macro-Region Former Office*, and *Neighbor Provinces*. Again, we estimate equation (1) separately for each turnover subsample.

The results consistently support H_{2C} . When the incoming president is born in the same macro-region as the court (Panel a of [Figure 8](#)), we observe significant post-turnover improvements in judicial performance. In this subsample, the growth rate of bankruptcy duration declines substantially, with an estimated reduction of 14.1% in the third year and 21.3% in the fourth year, indicating sustained efficiency gains following leadership turnover. By contrast, when the incoming president originates from a different macro-region (Panel b),



(a) With Managerial Experience

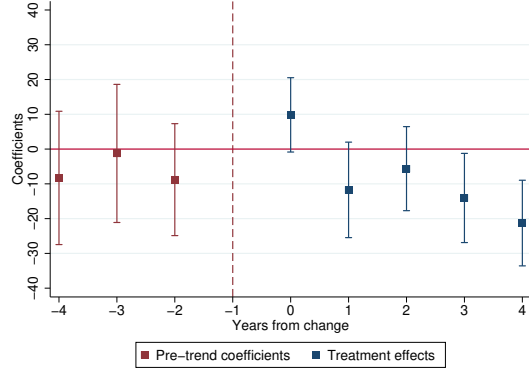


(b) Without Managerial Experience

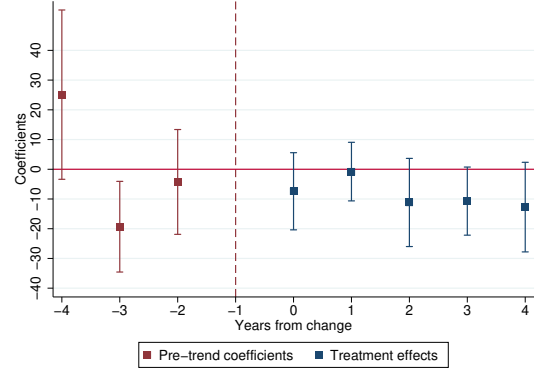
Figure 7: President Turnover and Proceedings Duration: Managerial Experience

The figure reports dynamic Difference-in-Differences estimates of the Average Treatment Effect from $t - 4$ to $t + 4$, obtained from equation (1) with court and year fixed effects. Each point reports the estimated differential in the annual growth rate of bankruptcy duration between treated and control courts. The dependent variable is *Bankruptcy Duration Growth* at the court level. The sample is split into two groups. The first category includes cases in which the incoming president already has managerial experience—either as president of another court or as a section president (Panel A). The second group comprises cases in which the incoming president lacks prior managerial experience (Panel B). Confidence intervals correspond to the 5th and 95th percentiles. Data Sources: *CSM* and *Italian Ministry of Justice*.

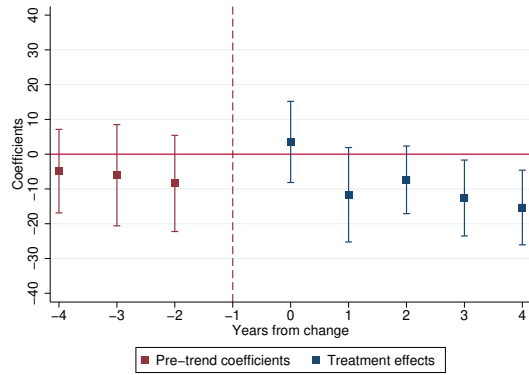
we do not detect statistically significant post-turnover effects on the growth rate of bankruptcy duration. This result suggests that birthplace-based local ties are important for translating leadership transitions into organizational change. A similar pattern emerges when local ties are measured using the location of the president’s prior office. Appointments involving presidents whose previous posting was in the same macro-region as the court (Panel c) are associated with post-turnover efficiency gains, whereas no statistically significant effects are detected for cross-region appointments (Panel d). The pattern is most pronounced when the incoming president is locally rooted within the court’s jurisdiction. When the incoming president’s previous office borders the region of the new court (Panel e), we observe sizable post-turnover efficiency gains, with an estimated decline in the growth rate of bankruptcy duration of 14.2% in the third year and 16.9% in the fourth year. By contrast, non-bordering appointments yield no discernible improvements in judicial performance (Panel f). Overall, these findings support H_{2C} and are consistent with evidence that birthplace (Ren et al. 2021,



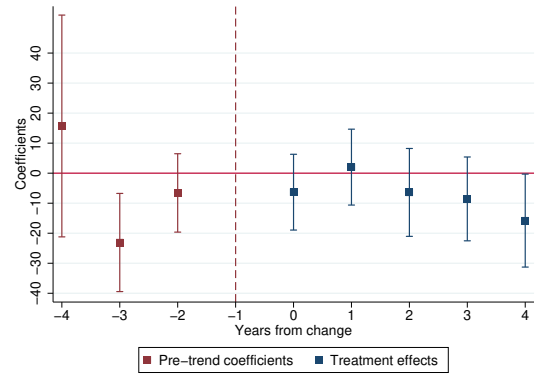
(a) Born in the same macro-region



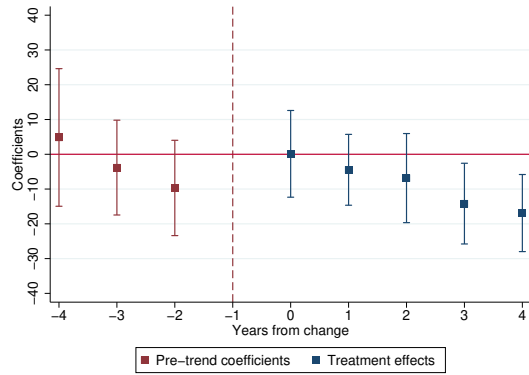
(b) Born in a different macro-region



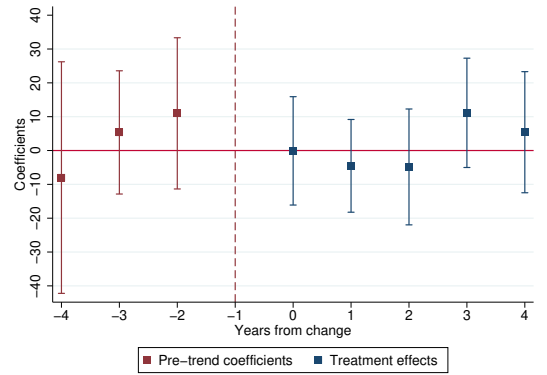
(c) Former office in the same macro-region



(d) Former office in different macro-regions



(e) Common border



(f) No common border

Figure 8: President Turnover and Proceedings Duration: The Role of Local Ties

The figure plots dynamic difference-in-differences estimates of the average treatment effect from $t - 4$ to $t + 4$, obtained from equation (1) with court and year fixed effects. The dependent variable is *Bankruptcy Duration Growth* at the court level. The sample is partitioned by three local-ties indicators. *Macro-Region Birth*: the incoming president's birth macro-region coincides with the court's macro-region (Panel a) or not (Panel b). *Macro-Region Former Office*: the incoming president's previous office macro-region coincides with the court's macro-region (Panel c) or not (Panel d). *Neighbor Provinces*: the incoming president's previous and current courts share a border (Panel e) or not (Panel f). Confidence intervals are at the 5th and 95th percentiles. Data Sources: *CSM* and *Italian Ministry of Justice*.

Yonker 2017, Pan et al. 2019, Chang et al. 2025), and local knowledge (Duchin et al. 2010) shape economic outcomes, extending this evidence to judicial leadership and court efficiency.

The limited sample size prevents us from conducting several additional robustness analyses. A joint exercise that simultaneously conditions on multiple characteristics—for example, restricting to turnovers where the age difference exceeds nine years, and the incoming president comes from a different macro-region—is not feasible because the resulting subsamples are too small. The same limitation affects the sample-window robustness analysis in Figure 5: given the smaller never-treated group, splitting the sample by incoming president characteristics—age gap, previous managerial experience, and local connections—produces subsamples that are too small for credible inference, so we confine that exercise to the aggregate specifications.

6.4 Does Appointing a New President Enhance Lending?

The previous section documents that court-president turnover significantly decelerates the growth rate of bankruptcy duration in treated courts relative to controls, and identifies the characteristics of incoming presidents that appear to generate the largest performance gains.

Table 7 reports the event-study estimates described in Section 5, which evaluate the effect of court-president turnover on the likelihood of late loan payments. We incrementally saturate the specification: Column (1) includes time and court fixed effects; Column (2) adds bank fixed effects; and Column (3) incorporates industry fixed effects. The pre-turnover coefficients are statistically indistinguishable from zero, consistent with the parallel-trends assumption. Following the president’s turnover, the estimates indicate a negative, statistically significant effect on the probability of late repayment, supporting hypothesis H_{3A} . The effect becomes apparent in the second year after the intervention and remains significant through the fourth year. Figure 9a illustrates this evidence by plotting the estimated coefficients from the most saturated specification of the Late Payment regression (see last column of Table 7). The average post-turnover effect is -0.001, statistically significant at the 5% level (Table 10),

implying a 0.10 percentage-point reduction in the likelihood of late repayment.¹⁴ Given that the average incidence of late payment is 1.2% (see [Table 4](#)), this corresponds to an 8.3% decrease relative to the baseline risk. Although the estimated coefficients may appear small, this reduction in loan delinquency is economically meaningful, especially at scale. These findings align with previous research ([Visaria 2009](#), [Schiantarelli et al. 2020](#), [D’Apice et al. 2024](#)), suggesting that improving judicial performance reduces credit risk by discouraging moral hazard among borrowers. Therefore, the results support hypothesis H_{3A} : a measurable deceleration in the growth of bankruptcy duration — driven by court-president turnover — translates into lower loan delinquency rates through strengthened creditor enforcement.

To further assess the effect of court president turnover on credit markets, we expand our analysis to the intensive margin by examining changes in the volume of credit granted within existing bank–firm relationships, as described in [Section 5](#). Before the president’s turnover, the coefficient estimates are not statistically significant, supporting the parallel-trends assumption. Starting in the second year after the turnover, lending volumes in the treated courts increased significantly relative to the control group ([Table 8](#)). [Figure 9b](#) presents the coefficient estimates from the Intensive Margin regression model, which includes the full set of fixed effects (see the last column of [Table 8](#)). The average estimated coefficient is 0.077 ([Table 10](#)), and it is statistically significant at the 1% level.

Because the dependent variable is measured as the natural logarithm of one plus short-term credit granted, the coefficient can be interpreted as an approximate percentage increase in short-term credit supplied. Specifically, an improved judicial system may encourage banks to increase short-term credit supplied within existing credit relationships by approximately 7%. This evidence supports the H_{3B} hypothesis, which posits that turnover among court presidents increases lending to incumbent borrowers. The finding is consistent with prior

¹⁴This average effect is computed linearly by combining the estimated post-turnover coefficients across periods to obtain a single aggregated estimate of the treatment effect. Specifically, we average the coefficients for the post-turnover periods included in the regression specification, weighting them equally, and compute the corresponding standard error using the variance-covariance matrix of the estimates.

Table 7: **Late Payment and Turnover of the Court President**

This table presents the results of the estimation of different specifications of the equation (1). The dependent variable (Late Payment) is a dummy variable that equals one if the firm has a loan repayment delay at the bank, and zero if the firm has only performing loans during the bank–firm match in a given year. Column (1) refers to the specification with the Year and Court fixed effects. To this first specification, we sequentially add the Bank fixed effects in column (2) and the Industry fixed effects in the last column (3). t denotes the year in which the new president of the court assumes office. All variables are defined in Appendix A. Standard errors, in brackets, are clustered at the court level by year. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

	(1) Late Payment	(2) Late Payment	(3) Late Payment
$t - 4$	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
$t - 3$	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
$t - 2$	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
t	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
$t + 1$	-0.001* (0.001)	-0.001 (0.001)	-0.001 (0.001)
$t + 2$	-0.002** (0.001)	-0.001* (0.001)	-0.001* (0.001)
$t + 3$	-0.002** (0.001)	-0.002** (0.001)	-0.002** (0.001)
$t + 4$	-0.002* (0.001)	-0.002* (0.001)	-0.002* (0.001)
Observations	1,621,320	1,621,320	1,621,320
Year-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Bank-FE	No	Yes	Yes
Industry-FE	No	No	Yes

Table 8: **Intensive Margin and Turnover of the Court President**

This table presents the results of the estimation of different specifications of equation (1). The dependent variable (Intensive Margin) is the natural logarithm of $(1 + \text{short-term credit granted})$ at the bank–firm match level. Column (1) refers to the specification with the Year and Court fixed effects. To this first specification, we sequentially add the Bank fixed effects in column (2), and the Industry fixed effects in the last column (3). (t) is the year in which the new president of the court starts. All variables are defined in Appendix A. Standard errors, in brackets, are clustered at court by year level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

	(1) Intensive Margin	(2) Intensive Margin	(3) Intensive Margin
$t - 4$	-0.123 (0.086)	-0.111 (0.078)	-0.106 (0.073)
$t - 3$	-0.059 (0.051)	-0.023 (0.045)	-0.012 (0.042)
$t - 2$	-0.029 (0.071)	-0.083 (0.059)	-0.074 (0.058)
t	0.017 (0.036)	0.006 (0.037)	0.007 (0.035)
$t + 1$	0.035 (0.036)	0.039 (0.039)	0.038 (0.036)
$t + 2$	0.062 (0.038)	0.096*** (0.036)	0.092*** (0.034)
$t + 3$	0.078** (0.037)	0.142*** (0.033)	0.137*** (0.032)
$t + 4$	0.089** (0.035)	0.106*** (0.033)	0.114*** (0.031)
Observations	1,621,320	1,621,320	1,621,320
Year-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Bank-FE	No	Yes	Yes
Industry-FE	No	No	Yes

research (Djankov et al. 2007, Jappelli et al. 2005, Bae & Goyal 2009, Rodano 2021), highlighting the importance of judicial efficiency and judicial reforms (Ponticelli & Alencar 2016) in fostering credit market development.

Finally, we analyze how court-president turnover affects the likelihood of initiating new bank–firm relationships (Table 9). Before the turnover, treated and control courts exhibit parallel trends, as shown by statistically insignificant coefficient estimates. Following the turnover, we detect a positive and statistically significant impact on the probability of forming new credit relationships. In particular, the probability of forming new credit relationships increases already in the turnover year and remains positive in subsequent periods. This extensive-margin reaction aligns with the forward-looking nature of banks’ lending decisions. The formation of new bank–firm ties is driven less by the actual realization of judicial outcomes and more by expectations about the broader enforcement environment and the reliability of future recovery in the event of default. Consequently, changes in court leadership can influence banks’ propensity to initiate new lending relationships by signaling shifts in organizational priorities and enforcement strength, potentially even before any improvement in court performance becomes visible in realized outcomes.

Figure 9c plots the event-study coefficients from the full specification (last column of Table 9). The average post-turnover coefficient is 0.009 (Table 10), significant at the 5% level, implying a 0.9 percentage-point increase in the likelihood of initiating a new bank–firm relationship. Against a baseline probability of 13.5% (Table 4), this corresponds to a 6.7% increase relative to the mean — modest in absolute terms but economically meaningful when aggregated across firms and time, as it reflects improved credit access for new borrowers. The effects also display non-uniform timing across outcomes: whereas the impact on borrower moral hazard and incumbent credit relationships emerges with a lag, the extensive-margin effect is contemporaneous with the turnover. This does not reflect pre-treatment dynamics: all pre-turnover coefficients are statistically indistinguishable from zero. The result is also robust to excluding the turnover year from the event-study window, with positive and significant

Table 9: **Extensive Margin and Turnover of the Court President**

This table presents the estimation results of different specifications of equation (1). The dependent variable (Extensive Margin) is set to one if the bank–firm pair is new in a given year compared to the previous one and zero otherwise. Column (1) refers to the specification with the Year and Court fixed effects. To this first specification, we sequentially add the Bank fixed effects in column (2) and the Industry fixed effects in the last column (3). All variables are defined in Appendix A. (t) is the year in which the new president of the court starts. Standard errors, in brackets, are clustered at court by year level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

	(1) Extensive Margin	(2) Extensive Margin	(3) Extensive Margin
$t - 4$	-0.001 (0.008)	-0.001 (0.008)	-0.001 (0.008)
$t - 3$	0.005 (0.007)	0.004 (0.007)	0.004 (0.007)
$t - 2$	-0.002 (0.004)	0.001 (0.005)	0.001 (0.005)
t	0.009** (0.004)	0.010** (0.004)	0.010** (0.004)
$t + 1$	0.006* (0.003)	0.006* (0.003)	0.006* (0.003)
$t + 2$	0.016*** (0.006)	0.015** (0.006)	0.015** (0.006)
$t + 3$	0.001 (0.005)	-0.001 (0.005)	-0.002 (0.005)
$t + 4$	0.018*** (0.006)	0.018*** (0.007)	0.017** (0.007)
Observations	1,621,320	1,621,320	1,621,320
Year-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Bank-FE	No	Yes	Yes
Industry-FE	No	No	Yes

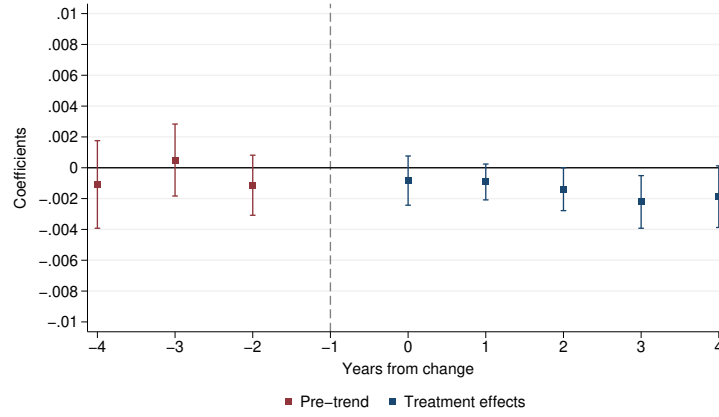
effects persisting in later post-turnover periods (notably at $t = 2$ and $t = 4$). Consistent with [Huson et al. \(2004\)](#), these findings suggest that turnover announcements convey information about future institutional performance, inducing a forward-looking expansion of the borrower pool. These results support hypothesis H_{3C} . Relative to [Rodano \(2021\)](#), who finds that judicial inefficiency constrains credit access primarily for high-risk borrowers, we document a meaningful average effect unconditional on borrower risk — a difference that likely reflects our broader credit perimeter: whereas [Rodano \(2021\)](#) focuses exclusively on term loans, we consider all credit granted within relationships that include at least one outstanding term loan.

Taken together, these results show that a slowdown in the growth of bankruptcy cases—caused by turnover among court presidents—spreads into credit markets via two complementary mechanisms: it curbs borrower moral hazard and increases the supply of credit available to both existing and new borrowers.

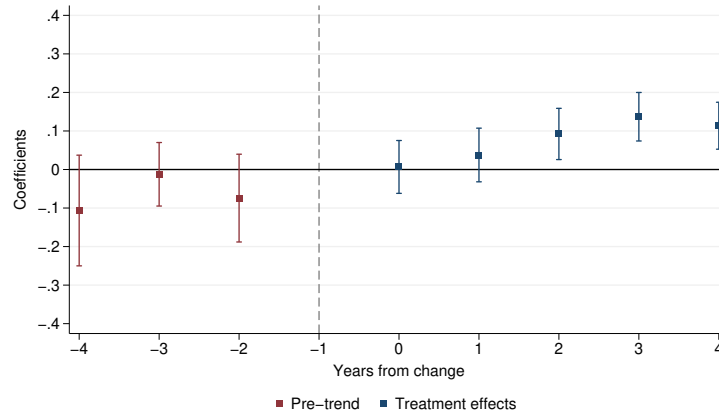
Table 10: **Average Effect of Court President Turnover on Lending**

The table reports the average post-treatment effect, computed as a linear combination of the event-study coefficients using the estimated variance-covariance matrix. Columns (1)–(3) correspond to the most saturated specifications in the main analysis, including all fixed effects. Standard errors, clustered at the court–year level, are reported in brackets. Variable definitions are in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

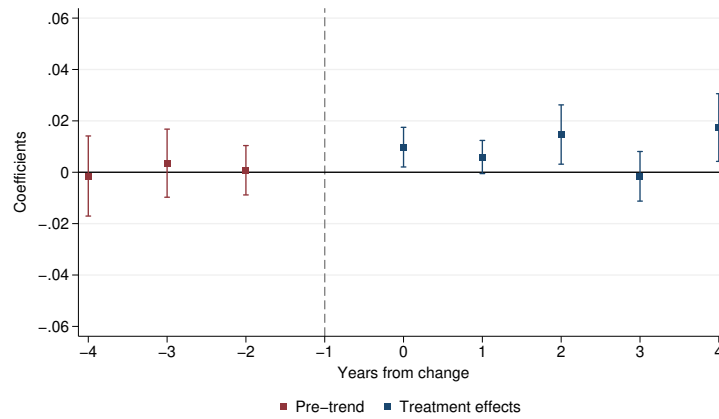
	(1) Late Payment	(2) Intensive Margin	(3) Extensive Margin
Post Turnover	-0.001** (0.001)	0.077*** (0.025)	0.009** (0.004)
Observations	1,621,320	1,621,320	1,621,320
Year-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes
Industry-FE	Yes	Yes	Yes



(a) Late Payment.



(b) Intensive Margin.



(c) Extensive Margin.

Figure 9: **Event Study: Effect of Court President Turnover on Lending**

The figure plots dynamic difference-in-differences estimates from equation (1), corresponding to the most saturated specifications in Table 7 (Panel a), Table 8 (Panel b), and Table 9 (Panel c). Confidence intervals are constructed using the 2.5th and 97.5th percentiles. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

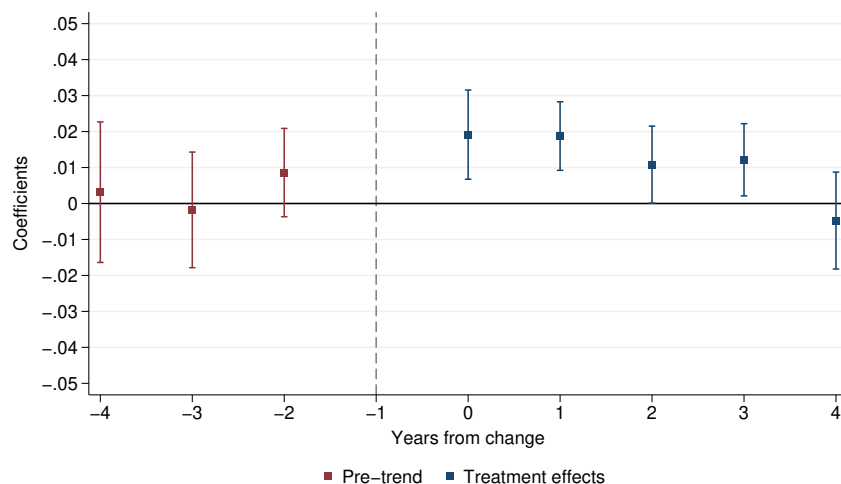
6.5 Does Court President Turnover Affect Firm Investment?

Our results to date show that changes in court presidents substantially reduce the pace at which bankruptcy case duration increases, our measure of judicial performance, thereby boosting the supply of credit and lowering the risk that borrowers become delinquent. We now examine whether these improvements spill over to firms' investment decisions. We focus on firms' fixed assets, which capture long-term investment and are less sensitive to short-term financial fluctuations.

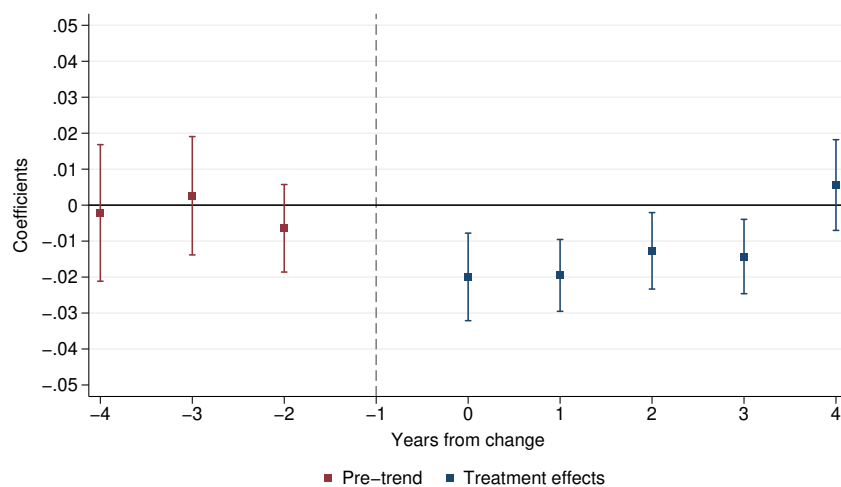
[Table 11](#) reports difference-in-differences estimates of the effect of court president turnover on total fixed assets and their composition—measured by intangible and tangible asset shares. Columns (1), (2), and (3) report results for the full sample; columns (4) and (5) restrict to large firms. For the full sample, neither the scale nor the composition of fixed assets changes significantly following turnover, rejecting H_{3D} . The evidence differs markedly for large firms. Pre-treatment coefficients are statistically insignificant, consistent with the parallel trends assumption ([Figure 10](#)). Following court-president turnover, the intangible asset share increases by 0.011 on average, while the tangible share declines by a similar magnitude ([Table 12](#)). This pattern points to a compositional shift from tangible to intangible assets rather than an expansion in overall fixed investment. Relative to sample averages ([Table 4](#)), these shifts correspond to a 6.1% increase in intangible intensity and a 1.5% decline in tangible share. Given the well-documented persistence of balance-sheet structures, these magnitudes are economically significant.

These results are consistent with the view that improvements in institutional quality disproportionately promote intangible investment: intangible assets are harder to collateralize and depend more heavily on effective contract enforcement ([Claessens & Laeven 2003](#), [Crouzet et al. 2022](#)). This concentration among large firms complements [Lilienfeld-Toal et al. \(2012\)](#), who show that larger firms are the primary beneficiaries of increased credit provision following judicial reform, and reflects the greater organizational capacity and exposure to contractual complexity that make large firms more sensitive to improvements in enforcement quality.

This pattern is reinforced by [Bellucci et al. \(2019\)](#), who show that greater informational transparency among large firms, relative to their smaller counterparts, reduces collateral requirements and eases access to external financing.



(a) Intangible Asset Share.



(b) Tangible Asset Share.

Figure 10: Event Study: Effect of Court President Turnover on Firms' Investments
The figure plots dynamic difference-in-differences estimates from equation (1), corresponding to columns (4) and (5) of [Table 11](#) for Panel (a) and Panel (b), respectively. Confidence intervals are constructed using the 2.5th and 97.5th percentiles. Data Sources: *Cerved Group*, *CSM*, and *Italian Ministry of Justice*.

Table 11: **Firm Investment and Turnover of the Court President**

The table reports difference-in-differences estimates of the effect of court president turnover on firm-level investment, using data collapsed at the firm-year level and matched to *Cerved Group*. Column (1) uses the natural logarithm of one plus total fixed assets as the dependent variable. Columns (2)–(5) decompose fixed assets into intangible and tangible components, each scaled by the sum of intangible and tangible assets. Columns (2) and (3) report results for the full sample; columns (4) and (5) restrict to large firms, defined following Commission Recommendation 2003/361/EC as firms employing at least 250 annual work units, or reporting annual turnover of at least €50 million and a balance sheet total of at least €43 million. *Area* denotes the geographical macro-region of the court (North-East, North-West, South, Islands). *t* denotes the year in which the newly appointed president assumes office. Standard errors, clustered at the court-year level, are reported in brackets. Variable definitions are in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *Cerved Group*, *CSM*, and *Italian Ministry of Justice*.

	(1) Fixed Assets	(2) Intangible Asset Share	(3) Tangible Asset Share	(4) Intangible Asset Share	(5) Tangible Asset Share
$t - 4$	-0.007 (0.013)	-0.001 (0.002)	-0.003 (0.002)	0.003 (0.010)	-0.002 (0.001)
$t - 3$	-0.015 (0.010)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.008)	0.003 (0.008)
$t - 2$	-0.003 (0.007)	0.001 (0.001)	-0.001 (0.001)	0.009 (0.006)	-0.006 (0.006)
t	0.003 (0.006)	-0.001 (0.001)	0.001 (0.001)	0.019*** (0.006)	-0.020*** (0.006)
$t + 1$	-0.010 (0.007)	-0.000 (0.001)	0.000 (0.001)	0.019*** (0.005)	-0.020*** (0.005)
$t + 2$	-0.012 (0.008)	-0.001 (0.001)	0.001 (0.001)	0.011** (0.006)	-0.013** (0.005)
$t + 3$	-0.012 (0.008)	-0.000 (0.002)	-0.000 (0.001)	0.012** (0.001)	-0.014*** (0.005)
$t + 4$	-0.009 (0.010)	-0.000 (0.001)	-0.000 (0.001)	-0.005 (0.007)	0.006 (0.006)
Sample includes	All firms	All firms	All firms	Large firms	Large firms
Observations	379,353	379,353	379,353	5,900	5,900
Year*Area-FE	Yes	Yes	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes	Yes	Yes
Firm-FE	Yes	Yes	Yes	Yes	Yes

Table 12: **Average Effect of President Turnover on Firm Investment**

The table reports the average post-treatment effect for each specification in Table 11, computed as a linear combination of the event-study coefficients using the estimated variance-covariance matrix. Standard errors, clustered at the court-year level, are reported in brackets. Variable definitions are in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *Cerved Group*, *CSM*, and *Italian Ministry of Justice*.

	(1) Fixed Assets	(2) Intangible Asset Share	(3) Tangible Asset Share	(4) Intangible Asset Share	(5) Tangible Asset Share
Post Turnover	-0.008 (0.006)	-0.000 (0.001)	0.000 (0.001)	0.011*** (0.004)	-0.012*** (0.004)
Sample includes	All firms	All firms	All firms	Large firms	Large firms
Observations	379,353	379,353	379,353	5,900	5,900
Year*Area-FE	Yes	Yes	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes	Yes	Yes
Firm-FE	Yes	Yes	Yes	Yes	Yes

7 Robustness Tests

This section presents additional tests validating our main results on the financial and real effects of court president turnover.

A key concern in difference-in-differences designs is violation of the parallel trends assumption. Our staggered DiD specification includes pre-treatment dummies spanning the four years before each turnover event; the absence of statistically significant pre-event coefficients provides graphical evidence that treated and control courts follow parallel trajectories before treatment. We supplement this with a joint significance test on all pre-event leads in the event-study specification. Failure to reject the null hypothesis confirms the absence of systematic differential pre-trends, supporting the plausibility of our identification strategy.

We also conduct a randomized placebo test to assess potential time-varying confounders, measurement error in the outcome variable (Roth et al. 2023), and spurious correlations arising from heterogeneous treatment effects (Miller 2023). Each treated court—restricted to those with actual treatment occurring after 2015—is randomly assigned a placebo treatment date between 2015 and its true treatment year. We then re-estimate the baseline models,

replacing the true treatment dummies with placebo treatment indicators (*Placebo Turnover*). Rather than simulating a placebo distribution, this single randomization examines whether a plausible counterfactual assignment generates spurious patterns in the estimated coefficients, providing a direct falsification test of whether endogenous treatment timing or confounding dynamics drive the baseline results.

Results are reported in [Table 13](#) for credit outcomes and [Table 14](#) for firm investment. Across all specifications, the placebo coefficients are statistically indistinguishable from zero, confirming that our identification strategy is not susceptible to false positives under randomized treatment assignment and reinforcing the credibility of the parallel trends assumption.

Table 13: Falsification Test: Placebo President Turnover and Lending

This table presents the results of a placebo exercise on average treatment effects, complementing the main event-study estimates previously reported. The variable *Placebo Turnover* is obtained by randomly assigning placebo treatment dates between 2015 and the actual turnover year, replacing the true event dates. The dependent variables are those analyzed in the current section. Columns (1) - (3) correspond to the most saturated specifications used in the main analysis, including controls and fixed effects. Standard errors, in brackets, are clustered at court by year level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *CR*, *CSM*, and *Italian Ministry of Justice*.

	(1) Late Payment	(2) Intensive Margin	(3) Extensive Margin
Placebo Turnover	0.000 (0.001)	-0.018 (0.035)	-0.001 (0.004)
Observations	1,621,320	1,621,320	1,621,320
Year-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Bank-FE	Yes	Yes	Yes
Industry-FE	Yes	Yes	Yes

Table 14: **Falsification Test: Placebo President Turnover and Firm Investment**

This table presents the results of a placebo exercise on average treatment effects, complementing the main event-study estimates previously reported. The variable *Placebo Turnover* is obtained by randomly assigning placebo treatment dates between 2015 and the actual turnover year, replacing the true event dates. The dependent variables are those analyzed in the current section. Standard errors, in brackets, are clustered at the court level by year. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Data Sources: *Cerved Group, CSM, and Italian Ministry of Justice.*

	(1) Fixed Assets	(2) Intangible Asset Share	(3) Tangible Asset Share
Placebo Turnover	-0.003 (0.007)	-0.000 (0.001)	0.000 (0.001)
Sample includes	All firms	All firms	All firms
Observations	379,353	379,353	379,353
Year*Area-FE	Yes	Yes	Yes
Court-FE	Yes	Yes	Yes
Firm-FE	Yes	Yes	Yes

8 Conclusions

Courts operating under the same legal framework and facing comparable caseloads nonetheless exhibit pronounced and persistent differences in efficiency that cannot be fully explained by observable institutional characteristics. This paper investigates whether internal judicial governance — specifically, the replacement of court presidents — accounts for a meaningful share of the residual heterogeneity. Exploiting the staggered rotation of court presidents across Italian courts from 2015–2022, we show that appointing a new president significantly reduces the growth rate of bankruptcy duration, highlighting managerial and organizational factors as important determinants of judicial performance.

Leadership effects are heterogeneous. Efficiency gains are larger when incoming presidents are younger, have prior managerial experience, or are locally rooted in the court’s jurisdiction, consistent with career-concern and organizational-learning mechanisms. These gains persist over time, ruling out transient attention effects and pointing to durable improvements in court organization.

Improved judicial performance also propagates to credit markets, as documented in the existing literature. Court president turnover expands credit supply along both the extensive margin—banks form new lending relationships—and the intensive margin—credit within existing relationships increases. We interpret these as supply-side responses to a more predictable enforcement environment that lowers uncertainty and reduces lending barriers. Firms also respond: late payments decline following leadership turnover, consistent with enhanced judicial enforcement raising the expected cost of strategic default, and strengthening incentives to meet contractual obligations, rather than merely reflecting changes in banks’ reclassification practices.

Turning to real outcomes, improved judicial performance reshapes the composition of firm investment rather than its aggregate level. Total fixed assets do not increase following leadership turnover; instead, firms reallocate capital from tangible to intangible assets. Since intangible assets rely more heavily on enforceable contracts and offer limited collateral value, this shift reflects the relaxation of enforcement-driven distortions in investment decisions. These effects are concentrated among larger firms, which possess the organizational capacity, internal governance, and financial access required to scale intangible-intensive investment and face greater exposure to contractual complexity and enforcement risk. Smaller, more collateral-dependent firms exhibit more limited adjustment.

Although our analysis focuses on Italy, the mechanism we study is not country-specific. A consistent finding in the literature is that legal reforms can improve judicial performance but do not eliminate substantial heterogeneity across courts, leaving an important role for internal organization and managerial practices ([Ponticelli & Alencar 2016](#), [Lilienfeld-Toal et al. 2012](#), [Visaria 2009](#)). Court leadership operates precisely on this margin by shaping organizational priorities, administrative coordination, and the credibility of enforcement. Accordingly, the economic effects we document should extend to jurisdictions in which courts play a central role in creditor enforcement and managerial discretion is non-trivial. Consistent with this interpretation, our heterogeneity results mirror patterns documented in other hierarchical

public institutions ([Ash & MacLeod 2024](#), [Bertrand et al. 2019](#)): performance gains are concentrated among younger leaders and those with prior managerial experience, characteristics commonly associated with stronger career incentives and managerial effectiveness. The evidence suggests that court leadership is an important yet often overlooked determinant of judicial performance and its downstream economic consequences.

Taken together, our findings reveal three interconnected channels through which internal judicial governance shapes economic outcomes: expanded credit supply, strengthened repayment incentives, and reallocation of productive capital toward enforcement-dependent activities. From a policy perspective, these results do not position leadership turnover as a stand-alone reform tool. Rather, they highlight internal judicial governance as a relevant but underappreciated dimension of institutional performance, and underscore that its economic consequences depend critically on the interaction between organizational features and firm heterogeneity. While our data preclude prescriptive recommendations on appointment criteria, the documented heterogeneity offers policymakers an empirical benchmark: performance gains are largest when incoming presidents are younger, have prior managerial experience, or are locally rooted in the court’s jurisdiction, providing a basis for evaluating past appointment practices and informing institutional design.

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Appendix A

Variable Definitions and Data Sources

The table reports definitions and sources for all variables used in the analysis. ⁽⁺⁾ Central Credit Register (CR); ⁽⁺⁺⁾ Cerved Group; ^(*) High Council of the Judiciary (*Consiglio Superiore della Magistratura – CSM*); ^(**) Italian Ministry of Justice; ^(#) Italian National Institute of Statistics (ISTAT).

Variable	Description
<i>Age Gap</i> ^(*)	The age differential, measured in years, between the incoming president and the outgoing predecessor.
<i>Bankruptcy Liquidation (Days)</i> ^(**)	The mean duration, in days, of bankruptcy liquidation cases (<i>liquidazione esecutiva</i>) at the court-year level, covering the asset-realization, creditor distribution, and formal closure phases of insolvency proceedings.
<i>Bankruptcy Liquidation (Growth)</i> ^(**)	The year-on-year change in the mean duration of bankruptcy liquidation proceedings (i.e., <i>Bankruptcy Liquidation (Days)</i>) at the court-year level, expressed in percentage points.
<i>Bankruptcy Declaration (Days)</i> ^(**)	The mean duration, in days, of pending bankruptcy declaration cases (<i>liquidazione dichiarativa</i>) at the court-year level, covering the formal opening phase of insolvency proceedings through judicial certification of the debtor's insolvency.
<i>Bankruptcy Declaration (Growth)</i> ^(**)	The year-on-year change in the mean duration of bankruptcy declaration proceedings (i.e., <i>Bankruptcy Declaration (Days)</i>) at the court-year level, expressed in percentage points.
<i>Civil Caseload (Days)</i> ^(**)	The mean duration, in days, of pending civil cases across all matter types (<i>totale materie</i>) at the court-year level.
<i>Civil Caseload (Growth)</i> ^(**)	The year-on-year change in the mean duration of all civil proceedings (i.e., <i>Civil Caseload (Days)</i>) at the court-year level, expressed in percentage points. This is the baseline measure of judicial dynamics used throughout the analysis.
<i>Enforcement: Real Estate (Days)</i> ^(**)	The mean duration, in days, of pending enforcement proceedings against real property (<i>esecuzione immobiliare</i>) at the court-year level.
<i>Enforcement: Real Estate (Growth)</i> ^(**)	The year-on-year change in the mean duration of real estate enforcement proceedings (i.e., <i>Enforcement: Real Estate (Days)</i>) at the court-year level, expressed in percentage points.
<i>Enforcement: Other Assets (Days)</i> ^(**)	The mean duration, in days, of pending enforcement proceedings against other assets, as financial instruments and receivables (<i>esecuzione mobiliare</i>), at the court-year level.
<i>Enforcement: Other Assets (Growth)</i> ^(**)	The year-on-year change in the mean duration of other asset enforcement proceedings (i.e., <i>Enforcement: Other Assets (Days)</i>) at the court-year level, expressed in percentage points.
<i>Extensive Margin</i> ⁽⁺⁾	An indicator equal to one if a bank-firm credit relationship appears in the CR in year t but did not exist in year $t - 1$, and zero otherwise.
<i>Firm Size</i> ⁽⁺⁺⁾	A categorical variable (1–4) reflecting firm size (Micro, Small, Medium, Large) based on the European Union (EU) 2003-361-EC workforce and turnover thresholds.
<i>Fixed Assets</i> ⁽⁺⁺⁾	The natural logarithm of one plus total fixed assets.
<i>GDP Growth Rate</i> ^(#)	Local economic growth, calculated as the year-on-year percentage change in the Gross Domestic Product (GDP) of the Province.

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Table A-1 (Continued) — Variable Descriptions

Variable	Description
<i>Incoming President Age</i> ^(*)	The age of the incoming president, in years, at the time of appointment.
<i>Intangible Asset Share</i> ⁽⁺⁺⁾	The ratio of the value of intangible assets to the sum of intangible and tangible assets. Firms with no tangible and intangible assets are assigned a value of zero.
<i>Intensive Margin</i> ⁽⁺⁾	The natural logarithm of one plus short-term credit granted within a bank-firm relationship with at least one outstanding term loan.
<i>Labor (Days)</i> ^(**)	The mean duration, in days, of pending employment dispute cases (<i>lavoro</i>) at the court-year level.
<i>Labor (Growth)</i> ^(**)	The year-on-year change in the mean duration of employment dispute proceedings (i.e., <i>Labor (Days)</i>) at the court-year level, expressed in percentage points.
<i>Late Payment</i> ⁽⁺⁾	An indicator equal to one if the firm is in arrears for more than 90 days on any loan within the bank-firm relationship, consistent with European Banking Authority (EBA) prudential standards, and zero otherwise.
<i>Macro-Region Birth</i> ^(*)	An indicator equal to one if the incoming president's birth macro-region coincides with that of the court, and zero otherwise.
<i>Macro-Region Former Office</i> ^(*)	An indicator equal to one if the incoming president's previous posting was in the same macro-region as the new court, and zero otherwise.
<i>Managerial Experience</i> ^(*)	An indicator equal to one if the incoming president held a managerial role prior to appointment, either as president of another court or as section president, and zero otherwise.
<i>Neighbor Provinces</i> ^(*#)	An indicator equal to one if the provinces of the incoming president's previous and current courts share a border, and zero otherwise.
<i>Non-Contentious (Days)</i> ^(**)	The mean duration, in days, of pending non-contentious matters (<i>volontaria giurisdizione</i>) at the court-year level, covering court supervision of private legal arrangements where no adversarial dispute exists.
<i>Non-Contentious (Growth)</i> ^(**)	The year-on-year change in the mean duration of non-contentious proceedings (i.e., <i>Non-Contentious (Days)</i>) at the court-year level, expressed in percentage points.
<i>Social Security (Days)</i> ^(**)	The mean duration, in days, of pending social-security and pension claims (<i>previdenza</i>) at the court-year level.
<i>Social Security (Growth)</i> ^(**)	The year-on-year change in the mean duration of social-security and pension proceedings (i.e., <i>Social Security (Days)</i>) at the court-year level, expressed in percentage points.
<i>Special Proceedings (Days)</i> ^(**)	The mean duration, in days, of pending cases conducted outside the ordinary civil track (<i>procedimenti speciali</i>), including payment injunctions and urgent interim relief, at the court-year level.
<i>Special Proceedings (Growth)</i> ^(**)	The year-on-year change in the mean duration of special proceedings (i.e., <i>Special Proceedings (Days)</i>) at the court-year level, expressed in percentage points.
<i>Tangible Asset Share</i> ⁽⁺⁺⁾	The ratio of the value of tangible assets to the sum of intangible and tangible assets. Firms with no tangible and intangible assets are assigned a value of zero.
<i>Turnover</i> ^(*)	An indicator equal to one if court <i>c</i> undergoes a presidential transition in year <i>t</i> , capturing the discrete leadership shock to judicial management.

Appendix B

Court President's Turnover and the Duration of Judicial Proceedings

The appendix plots dynamic DiD estimates from equation (1) with court and year fixed effects. The dependent variable is *Bankruptcy Declaration (Growth)* in Panel (a), *Bankruptcy Liquidation (Growth)* in Panel (b), *Labor (Growth)* in Panel (c), *Social Security (Growth)* in Panel (d), *Enforcement: Real Estate (Growth)* in Panel (e), *Enforcement: Other Assets (Growth)* in Panel (f), *Special Proceedings (Growth)* in Panel (g), and *Non-Contentious (Growth)* in Panel (h). Confidence intervals are reported at the 5th and 95th percentiles. Variable definitions are provided in Appendix A. Data Sources: CSM and the *Italian Ministry of Justice*.

