Remote Work Incentives and State Capacity

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Abstract

Leveraging data on 10 million civil proceedings in Italy between 2016 and 2026, this paper studies the impact of remote work incentives on judicial productivity and spatial inequality in state capacity. Exploiting judges' rotation across courts, I estimate that judges account for 60 percent of the variation in judicial productivity, compared with 11 percent explained by courts. Physical transfers disrupt judicial production, generating output losses equivalent to five months of work. While more productive judges previously received no higher pay, in 2025 the government introduced compensation for judges adjudicating cases remotely in target courts with the longest disposition times. Remote-work incentives increased judges' completed proceedings by 16 percent and their complexity-weighted productivity by 30 percent. In turn, remote adjudication raised output in target courts by 19 percent and reduced their backlogs and disposition times by 5 and 6 percent within ten months. These findings show that remote work can reduce spatial inequality in state capacity without costly worker relocation.

1 Introduction

State capacity is a key determinant of economic growth but large differences persist even within countries, reflecting past investments in legal and fiscal institutions (Besley and Persson, 2009; Acemoglu et al., 2015). Reallocating public resources toward lagging areas is often constrained by the costs of worker relocation (Schmutz and Sidibé, 2019; Freitas, 2025).[1] As a result, state capacity tends to be weaker in historically disadvantaged regions, reinforcing existing spatial economic inequalities. The recent diffusion of remote work technologies may mitigate these disparities by allowing governments to reallocate public-sector work towards disadvantaged areas without costly worker relocation (Barrero et al., 2023).

This paper asks whether remote work can reduce spatial inequality in state capacity. To answer this question, it studies a reform in the Italian judiciary that incentivized judges from

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[1] These costs can include compensation for workers' location preferences, approval from the administration losing staff, temporary parallel staffing, and investments in local infrastructure.

home courts to adjudicate cases remotely for *target courts* facing the most severe backlogs. Combined with evidence on judicial transfers, the reform provides an ideal setting to compare two mechanisms for reallocating state capacity across space: moving workers physically or moving their work remotely.

The empirical analysis is based on a newly assembled dataset of almost 10 million civil proceedings in Italy between 2016 and 2026, collected by web scraping the Ministry of Justice's online platform *Banca Dati di Merito* (Ministero della Giustizia, 2026a). The data contain information on judges, courts, case characteristics, filing and disposition dates, and appellate outcomes. Judicial productivity is decomposed into judge and court effects using an AKM framework (Abowd et al., 1999), and the impact of the remote work incentive scheme is estimated using an event-study design.

First, I document substantial heterogeneity in first-instance judicial productivity prior to the introduction of the incentive scheme. Judges at the 25th percentile of the productivity distribution completed 14 proceedings per quarter, compared with 72 at the 75th percentile. Variation across courts is smaller: courts at the 25th percentile processed 43 proceedings per judge per quarter on average, compared with 70 at the 75th percentile. Using bias-corrected estimates of the variance components (Andrews et al., 2008), I find that judge fixed effects account for 60 percent of the total variation in complexity-weighted productivity, whereas court fixed effects account for 11 percent.[2]

Judges' productivity declines sharply in the quarter preceding a transfer, reaches its minimum in the transfer quarter, and gradually recovers, even when they move from the least to the most productive courts. The productivity dip likely reflects transition costs, such as case handovers, reviewing ongoing cases, and learning new procedures. The cumulative loss corresponds to approximately 237 forgone proceedings per transfer, or five months of average judicial output. This finding shows that transfers of public-sector workers impose not only monetary but also productivity costs on governments. Therefore, remote work may help reallocate part of state capacity across space without the deadweight losses associated with worker relocation.

Second, I investigate the determinants of judicial productivity by combining data on court characteristics with newly collected data on judges' characteristics from Ministry of

[2]Complexity-weighted productivity is the number of proceedings a judge completes over a given period, weighted by each proceeding type's average duration relative to the average duration across all proceedings.

Justice bulletins (2007–2025) (Ministero della Giustizia, 2026b).[3] Prior to the introduction of the incentive scheme, judicial productivity is unrelated to predicted compensation. This is unsurprising given that judicial pay is determined primarily by tenure and by periodic performance evaluations in which 99 percent of judges receive the highest rating (Negri, 2026). I also find no evidence of a quantity–quality trade-off: more productive judges also deliver higher-quality decisions, with slightly lower reversal rates on appeal and shorter case duration.

Third, I study the impact of the remote work incentive scheme. It was introduced as part of Italy's Recovery and Resilience Plan (PNRR) to reduce case disposition time by 40 percent relative to its 2019 level (Presidenza del Consiglio dei Ministri, 2025).[4] The program targeted 48 courts with the longest disposition times and was designed to involve up to 500 judges, although only 212 ultimately participated (Consiglio Superiore della Magistratura, 2025; Negri, 2025). Incentivized judges remained responsible for their regular workload in their home courts, while also adjudicating cases remotely for designated target courts. Hearings could be conducted through videoconference or written submissions, while preserving the right of any party to request an in-person hearing.

Judges who remotely disposed of 50 cases between September 2025 and June 2026 received a lump-sum bonus equal to three months of salary for an ordinary judge with three years of seniority. This corresponded to a net incentive of approximately €9,500 (about €190 per case), together with additional seniority credits that counted toward career progression. Judges who completed the assignment ahead of schedule could take on additional batches of 50 cases and earn the bonus multiple times. I classified a judge as incentivized if they adjudicated cases exclusively in their home court during the year preceding September 2025 and adjudicated cases in both their home court and a target court between September 2025 and June 2026. Applying this criterion, I identified 139 incentivized first-instance judges.[5] Judges who participated in the incentive scheme were more productive than average, but productivity trends remain parallel between incentivized and non-incentivized judges until

[3] I was able to retrieve information for 57 percent of the judges in my sample, accounting for 76 percent of all proceedings.

[4] Disposition time measures the average time required for a court to dispose of its pending caseload. It is computed as the ratio of pending cases at the end of a year to completed cases during the same year.

[5] The remaining 73 participants belonged to courts outside my sample, such as courts of appeal and juvenile courts, were serving on secondment, or specialized in subject matters not covered by my data, such as family law.

the introduction of the incentive scheme, supporting the validity of the research design.

The scheme increased incentivized judges' monthly case completions (both in-person and remote proceedings) by 4 cases from a baseline of 24, implying approximately a 16 percent increase in unweighted productivity. Although home-court presidents were required to monitor the in-person productivity of incentivized judges, I estimate a decline of 3 monthly in-person case completions. This reduction was therefore more than offset by an average of 7 monthly remote case completions. Together, these effects suggest a limited displacement of non-incentivized in-person adjudication by incentivized remote proceedings. When proceedings are weighted by complexity, the estimated total effect rises to 30 percent, indicating that the cases assigned by target courts were more complex than average. Back-of-the-envelope calculations indicate that the scheme produced additional judgments at a cost approximately 6 percent below the average cost per proceeding, making it a cost-effective policy instrument.

Using non-target courts without incentivized judges as control courts, I find no evidence of negative spillover effects on the productivity of non-incentivized judges, either in target courts or in home courts. Turning to court-level outcomes, I find that the scheme increased the number of completed proceedings in target courts by 9 percent and complexity-weighted output by 19 percent relative to control courts. The judicial capacity provided by remote judges reduced backlog and disposition time in target courts by 5 and 6 percent, respectively, within ten months. Producing the same additional output in target courts through physical transfers would have required relocating approximately 117 judges and generated a deadweight loss of almost 28 thousand proceedings, roughly equal to the additional output delivered.

The paper first contributes to the literature on within-country variation in state capacity. Existing work shows that weak state capacity can undermine the protection of civil rights in peripheral regions (O'Donnell, 1993) and foster the onset and persistence of civil conflict (Fearon and Laitin, 2003). A related literature documents that spatial differences in state capacity are important determinants of within-country variation in economic development (Michalopoulos and Papaioannou, 2013; Acemoglu et al., 2015; Bandyopadhyay and Green, 2016). To the best of my knowledge, this paper provides the first causal evidence that remote-work incentives can reduce spatial inequality in state capacity, offering a potential way to mitigate its negative consequences.

Second, the paper contributes to the literature on remote work. Existing research has examined its effects on worker productivity, labor supply, job satisfaction, promotions, turnover, absenteeism, fertility and motherhood penalty (Bloom et al., 2015; Linos, 2018; Choudhury et al., 2021; Bloom et al., 2022; Gibbs et al., 2023; Emanuel et al., 2023; Angelici and Profeta, 2024; Fenizia and Kirchmaier, 2024; Choudhury et al., 2024; Davis et al., 2026; Basso et al., 2026). I instead study remote work incentives as a technology for reallocating productive capacity across space. The results show that organizations can address geographic mismatches between service supply and demand, avoiding the deadweight loss of worker relocation while increasing aggregate output.[6]

Finally, a growing literature shows that individual public-sector workers differ substantially in productivity and that these differences have important consequences for organizational performance (Fenizia, 2022; Best et al., 2023; Bennedsen et al., 2024; Muñoz and Prem, 2024; Muñoz and Otero, 2025; Dahis et al., 2025; Miglino and Smurra, 2025; Facchetti, 2025). Related work studies how incentive schemes affect worker selection and performance in the public sector (Bénabou and Tirole, 2006; Dal Bó et al., 2013; Ashraf et al., 2014; Finan et al., 2017; Deserranno, 2019; Ashraf et al., 2020; Deserranno et al., 2025). Combining an AKM framework with a real-world incentive scheme, I show that higher productivity workers are more likely to participate but incentives partially crowd out effort on unrewarded tasks, highlighting both selection and crowding-out effects of performance pay.

The policy implications of these findings extend beyond the justice system. In any public service where state capacity is unevenly distributed and remote work is feasible, governments can reallocate skilled labor toward underserved areas without costly worker relocation. Potential applications include public procurement, urban planning and permitting, tax administration, pension and welfare processing, and telemedicine. As digital technologies continue to expand the set of tasks that can be performed remotely, this mechanism may offer a cost-effective tool to close territorial gaps across a wide range of public services.

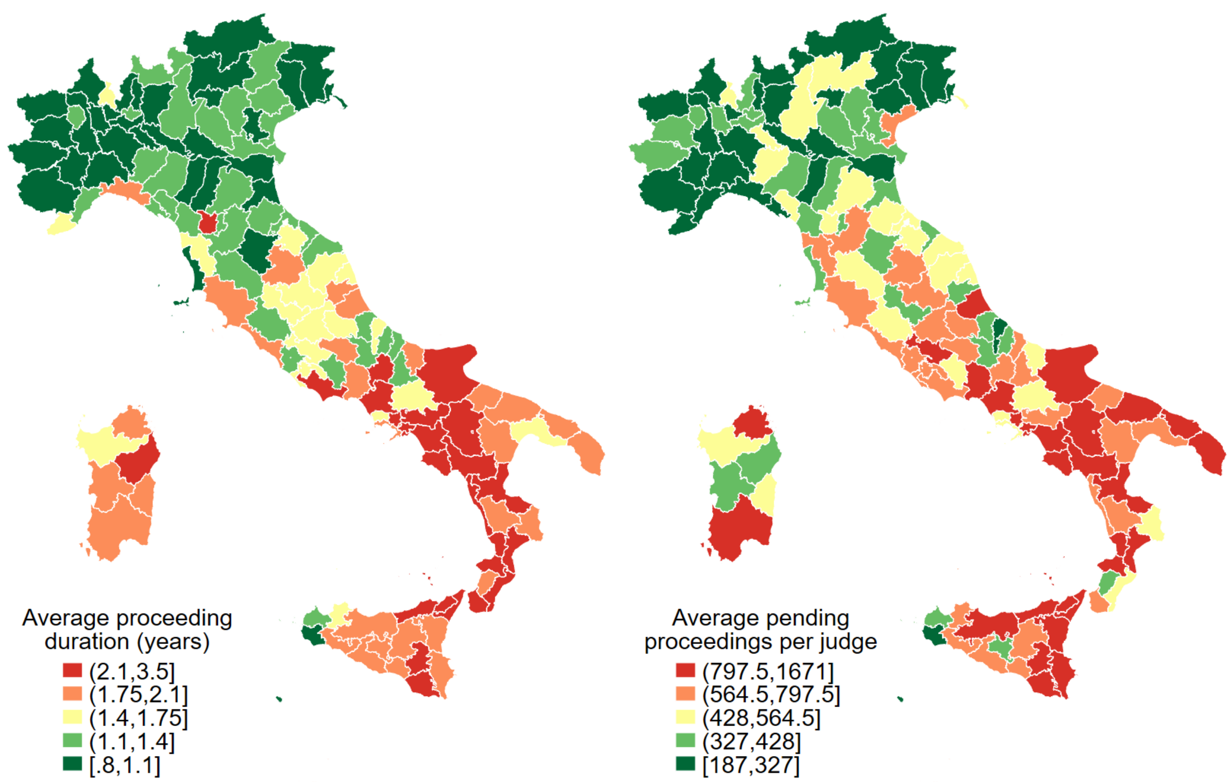
The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the remote work incentive scheme. Section 3 presents the data and Section 4 examines the determinants of judicial productivity before the incentive scheme. Section 5 evaluates the effects of remote work incentives on judges and courts. Section 6 concludes.

[6]This paper also contributes to the literature on policies aimed at improving judicial performance. Previous work has shown how to increase judicial productivity through better time and task allocation (Coviello et al., 2014, 2015), improved selection of judges (Dahis et al., 2025), and the composition of judicial teams (Salvestrini and Ronchi, 2026).

2 Institutional background

Although Italy's judicial expenditure is close to the European Union average, at about 0.3 percent of GDP (Figure D1), Italy has the second slowest civil justice system in Europe (Figure D2).[7] These delays have macroeconomic consequences. Ciapanna et al. (2023) estimate an elasticity of total factor productivity with respect to disposition time of about -0.03. Under this estimate, meeting the PNRR target of a 40 percent reduction in disposition time would raise Italian GDP by approximately 1.2 percent in the long term.

Figure 1: Spatial inequality in state capacity: the Italian justice system.



(a) Duration **(b)** Backlog

Notes: Panel (a) shows the average duration (in years) of first-instance proceedings completed in Italian courts between 2016 and 2025. Panel (b) shows the average annual backlog (pending proceedings per judge) between 2016 and 2025. Proceedings include judgments, orders and decrees.

[7] The number of professional judges per 100,000 inhabitants is below the EU median (Figure D4), while judicial salaries are comparatively higher, especially at the end of the career (Figure D3).

The average slowness of the Italian justice system hides large spatial disparities in judicial capacity. Figure 1a shows that first-instance proceedings take longer in southern courts, with an average duration of 2.1 years compared to 1.3 years in the rest of Italy (+62 percent). Focusing on judgments, the average time rises to 3.9 years in the South, relative to 2.8 years elsewhere (+39 percent; Figure D6).[8] Figure 1b shows that southern courts also face larger backlogs, averaging 751 pending proceedings per judge compared to 411 in the rest of Italy (+83 percent). Judicial congestion and delays are therefore concentrated in the country's poorest regions: in 2023, GDP per capita in Southern Italy was only 56.7 percent of the level in the rest of the country (Accetturo et al., 2022). These regional averages mask substantial heterogeneity across courts: some Southern courts perform relatively well, while some Northern courts face severe congestion.

The Italian justice system is just one of the many settings in which state capacity is weaker in historically disadvantaged regions, reinforcing existing spatial economic inequalities. For instance, Southern Italy exhibits longer completion times for public procurement (Baltrunaite et al., 2021), longer payment delays (Accetturo et al., 2026), lower-quality public transportation (Mocetti and Roma, 2021), slower emergency and rescue services (Miglino and Smurra, 2025), lower levels of digitization in local public administrations (Ciapanna et al., 2025), and weaker institutional quality overall (Cannella et al., 2025).

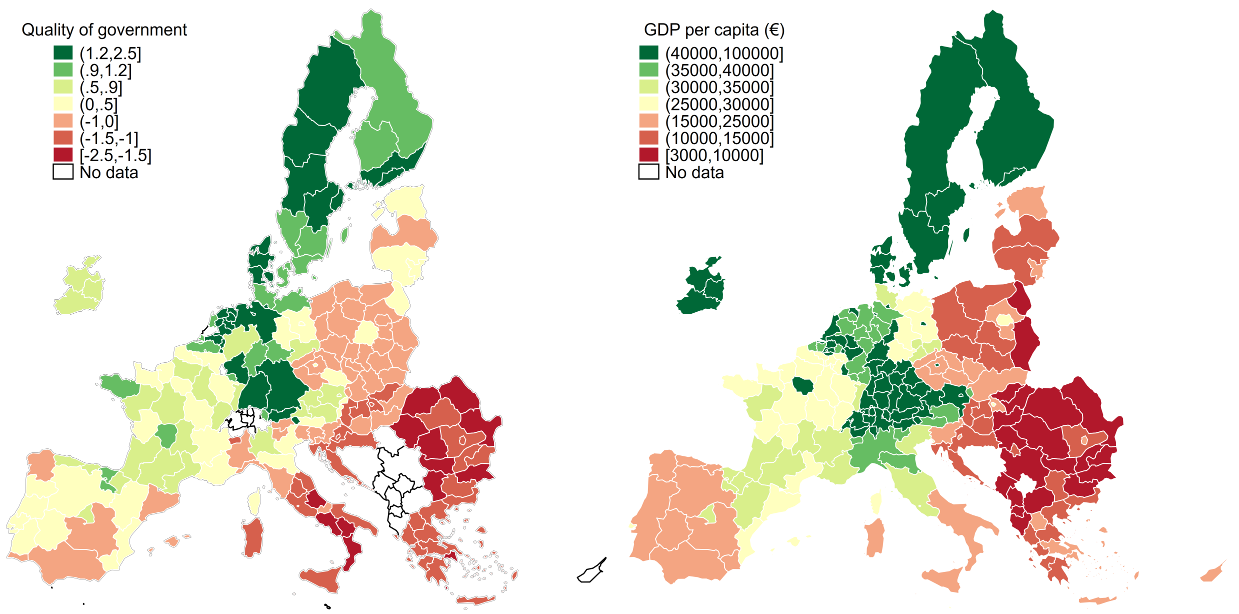
Spatial disparities in state capacity are not unique to Italy. They are particularly pronounced in developing countries, where limited state presence across large parts of the territory contributes to subnational variation in economic development (Acemoglu et al., 2015). Although less pronounced than in Italy, spatial disparities in state capacity also arise in other developed countries. Since reunification, East Germany has exhibited weaker fiscal and administrative capacity than West Germany (Dwenger and Gumpert, 2025). In Spain, judicial efficacy and congestion vary substantially across provinces affecting firm size, entrepreneurship, and investment (García-Posada and Mora-Sanguinetti, 2015b,a; Dejuan-Bitria and Mora-Sanguinetti, 2019). In the United States, judicial performance differs substantially across state supreme courts (Ash and MacLeod, 2021), while postal delivery errors

[8] Proceedings are of three types: judgments (*sentenze*), orders (*ordinanze*), and decrees (*decreti*). A judgment decides all or part of the merits of a dispute or addresses procedural questions. An order generally regulates the conduct of proceedings, provides brief reasons, and is issued after the parties have had an opportunity to be heard. A decree also generally regulates the course of proceedings but is ordinarily issued without an adversarial hearing (Enciclopedia Treccani, 2026).

and bureaucratic productivity varied markedly across cities during the Gilded Age (Aneja and Xu, 2024).

The correlation between state capacity and economic development extends across European Union regions (Figure 2). Higher-income areas, such as Northern Italy, Madrid, and West Germany, systematically score higher on the European Quality of Government Index than lower-income areas such as Southern Italy, Southern Spain, and East Germany. The correlation between regional GDP per capita and the Quality of Government Index is 0.68. Taken together, these patterns suggest that state capacity tends to be weaker in historically disadvantaged regions, reinforcing persistent spatial economic inequalities.

Figure 2: Spatial inequality in state capacity: quality of government in European regions.



(a) Quality of government

(b) GDP per capita (€)

Notes: Panel (a) shows the quality indicator of the European Quality of Government Index across European NUTS2 regions in 2017 (Charron et al., 2019). Panel (b) shows GDP per capita (in €) across European NUTS2 regions in 2017 (Eurostat, 2026a,b).

2.1 Remote work incentives

As part of the PNRR, Article 3 of Decree–Law 117/2025 introduced an incentive program for judges to perform additional casework remotely alongside their regular duties (Presidenza del Consiglio dei Ministri, 2025). For first-instance civil courts, the PNRR aimed to reduce average disposition time by 40 percent relative to 2019 and the stock of cases pending in 2022 that had been filed since 2017 by 90 percent by 30 June 2026. To achieve these targets, the High Council of the Judiciary identified 48 courts most in need of support and set the maximum number of judges that could be assigned remotely to each.

The overall program was capped at 500 volunteer judges. Only 212 applications were received but the judge allocations remained proportional to the distribution originally planned (Negri, 2025). Ordinary judges serving in courts other than the identified target courts, as well as judges on secondment, were eligible to apply for the incentive scheme between September 4 and September 18, 2025.[9] Within ten days of receiving an application, the High Council assigned each judge to a target court based on the submission date. Judges could not express preferences regarding their assignment.

Each participating judge was required to complete 50 civil judgments remotely by June 30, 2026. Within ten days of assignment, the head of the relevant target court selected these cases from macro-categories aligned with the PNRR objectives (Consiglio Superiore della Magistratura, 2025). Hearings could be conducted through audiovisual telecommunication or by exchanging written submissions, while preserving the right of any party to request an in-person hearing.[10] Judges on remote assignment continued to serve in person at their home court, and the head of that court was required to ensure that their in-person productivity did not fall below the court's average.

Completion of the 50 assigned cases entitled the judge to a lump-sum payment equal to three times the monthly salary of an ordinary judge with three years of seniority. This corresponded to a net incentive of about 9,515 euros (190 euros per proceeding).[11] In addition,

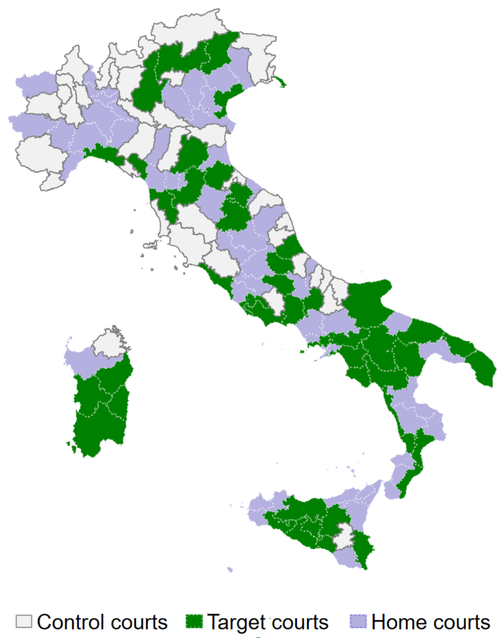
[9]The pool of potential applicants included 1,461 first-instance judges, 677 judges serving in courts of appeal, 141 judges in the Court of Cassation, 180 judges in juvenile courts, and 150 judges on secondment, totaling 2,609 eligible candidates (Consiglio Superiore della Magistratura, 2025).

[10]If a party successfully objected to a remote procedure, the case was reassigned to an in-person judge and another proceeding was allocated to the remote assignment judge.

[11]The monthly salary of an ordinary judge with three years of seniority was 6,114 euros gross in 2026 (Area Democratica per la Giustizia, 2024). The net amount is obtained by multiplying the gross monthly salary by three and deducting pension contributions (9 percent) and marginal income tax for the highest tax

the judge received 0.16 seniority points toward career progression. Judges who completed their assigned cases before the deadline could request additional batches of 50 cases to be resolved by 30 June 2026, multiplying the total incentive payment. Lump-sum payments and seniority points were awarded only upon completion of each *entire* batch of 50 cases.

Figure 3: Target and home courts in the remote work incentive scheme



Notes: The figure maps the courts involved in the remote work incentive scheme. Target courts are shown in green, while the home courts of the incentivized first-instance judges identified in my sample are shown in purple. Courts shown in gray are neither target courts nor home courts of any incentivized judge (control courts). Data are drawn from Consiglio Superiore della Magistratura (2025) and Ministero della Giustizia (2026a).

Using web-scraped data, I classified a judge as incentivized if they adjudicated cases exclusively in their home court in the year before September 2025 and subsequently adjudicated cases in both their home court and a target court between September 2025 and June 2026. Applying this criterion, I identified 139 incentivized first-instance judges. The remaining 73 participants belonged to courts outside my sample, such as courts of appeal and juvenile courts, or were serving on secondment. Figure 3 shows the target courts and home courts

bracket (43 percent).

of the incentivized judges. Courts that are neither target courts nor home courts of any incentivized judge are defined as control courts. Given within-region variation in court performance, target and home courts are located in both Northern and Southern Italy. Table C1 reports the target courts ranked by level of need, together with the maximum number of judges that could be assigned remotely to each court and the number of incentivized judges identified in my sample.[12]

To assess the extent of misclassification, I apply the same classification rule to earlier years, when the incentive was not in place.[13] Table C2 shows that the procedure classifies, on average, 4 judges as incentivized in these placebo periods. This corresponds to approximately 3 percent of the treated group, suggesting that false-positive classification is limited. Such misclassification would induce only mild attenuation bias and the estimated effects may slightly understate the true effects.

3 Data

I web-scraped the Italian Ministry of Justice's platform *Banca Dati di Merito*, which contains information on proceedings issued by first-instance civil courts and courts of appeal between 2016 and 2026.[14] The dataset covers all civil subject matters, except for family law, minors, and personal status (D'Alessandro et al., 2024; Ministero della Giustizia, 2026a).[15] This allowed me to construct a dataset that links each first-instance ruling to the corresponding judge and court, enabling the tracking of judges across courts over time. The dataset includes the year the proceeding was initiated, the date the first-instance ruling was issued, the judge's name and surname, the court and judicial district, the subject matter, and proceeding keywords. The keyword categories are very fine (e.g. eviction for non-payment of rent contract, individual dismissals for just cause) and include 905 different classifications.

[12] Information on the allocation of all 212 incentivized judges across target courts is not publicly available.

[13] In each year t, judges are classified as incentivized if they: complete proceedings only for a non-target court from September of year $t-1$ to August of year t; complete proceedings for both a home and a target court from September of year t to June of year $t+1$; and do not complete any proceedings for any target court from July to December of year $t+1$.

[14] I verified the completeness of the web scraping process by checking that the total number of decisions downloaded matched the monthly counts reported on the website for each court.

[15] It also excludes property and insolvency enforcements, proceedings before justices of the peace or concluded without a decision, such as settlements and withdrawals.

Accounting for the type of proceeding is essential to avoid conflating differences in judicial productivity with differences in the variety of cases judges adjudicate. Figure D10 shows that the data downloaded from the *Banca Dati di Merito* replicate the trends in the aggregate number of judgments and proceedings reported by the Ministry of Justice for the years 2016–2025.

Maximizing the number of judicial decisions can conflict with maintaining their quality, generating a multitasking problem for judges (Holmstrom and Milgrom, 1991). When this trade-off is relevant, bureaucrats and institutions may appear to be high performers based on volume alone, even if their output is offset by lower-quality decisions. To measure decision quality, I web-scraped appellate rulings from the *Banca Dati di Merito* and classified each first-instance judgment as upheld or overturned.[16] I linked appellate rulings to the corresponding first-instance proceedings using a unique case identifier provided by the Courts of Appeal.[17] The current sample covers five Courts of Appeal: Ancona, Bologna, Brescia, Cagliari, and Caltanissetta. Figures D7 and D8 illustrate examples of first-instance and appellate proceedings web-scraped from *Banca Dati di Merito*.

The Ministry of Justice also provides aggregate data on the production function of justice. For demand and output, I observe the number of incoming, resolved, and pending civil proceedings, disaggregated by court, year, and subject matter for the period 2016–2025. For labor inputs, the dataset includes the number of judges, as well as administrative staff (clerks, officials, auxiliary staff), by court and year for the period 2017–2023. For capital inputs, I use cross-sectional data on the surface area (in square meters) of courtrooms, offices, libraries, and archives by court, available for the year 2021. Regarding technology, for each court and year from 2016 to 2020, I observe the number of digitally filed judgments. Dividing this by the total number of judgments yields a digital filing rate, which serves as a proxy for the degree of court digitization (Cugno et al., 2022; Cannella et al., 2024).

Finally, I extracted judges' gender, date and place of birth from all official Ministry of Justice bulletins published between 2007 and 2025. The resulting data cover 57 percent of judges in the sample, corresponding to 76 percent of proceedings (Ministero della Giustizia, 2025a). Figure D9 provides an example of the demographic information available for a judge

[16]Partially successful appeals are classified as overturned.

[17]The identifier was kindly provided by 17 of the 29 Courts of Appeal in response to a freedom-of-information request.

from these bulletins. Using these data, and exploiting the fact that judicial compensation in Italy is determined almost entirely by tenure and periodic performance evaluations, of which 99 percent result in the highest rating (Figure D11), I reconstructed judges' wage profiles (Negri, 2026). Specifically, for each judge-year I assigned wages based on the statutory pay schedule under the assumption that judges entered the judiciary at age 25 and successfully passed all performance evaluations on schedule (Area Democratica per la Giustizia, 2024).

3.1 Summary statistics

Judge characteristics. Table C3 reports summary statistics on judges' observable characteristics. Column 1 presents the full sample, Column 2 restricts attention to judges who served in at least two different courts during the sample period (movers), and Column 3 focuses on incentivized judges. The dataset includes 6,242 first-instance judges, of whom 1,463 (23 percent) are movers and 139 (2.2 percent) are incentivized.

The majority of judges (53 percent) are born in Southern Italy, consistent with the relatively high public-sector wage premium and limited private-sector employment opportunities in these regions. Movers closely resemble the overall sample in terms of observable characteristics: they are younger on average (46 versus 49 years old), but have nearly identical gender composition (57–58 percent female) and geographic origins. Incentivized judges differ more from the full sample: they are 5 years younger, 7 percentage points less likely to be female, and 4 percentage points more likely to have been born in Southern Italy. Panel C reports data availability for the variables shown in Panels A and B. These characteristics can be observed in the Ministry of Justice bulletins for 57 percent of judges in the full sample, 82 percent of movers, and 83 percent of incentivized judges.

Sample structure. Table 1 summarizes the structure of the sample. The full sample includes 140 active courts: 48 courts targeted by the remote work incentive scheme, 55 home courts of incentivized judges, and 37 other courts that serve as controls.[18] The identification of court and judge fixed effects relies on judges' mobility between courts (Abowd et al., 1999). Every court in the sample is connected through at least one judge transfer, and this ensures that all courts belong to a single connected set.

[18] 25 courts were abolished between 2013 and 2014 (Mocetti et al., 2026). Proceedings initiated in these courts are assigned to the courts into which they were incorporated.

There are 9.5 million first-instance proceedings completed between January 2016 and June 2026. Of these, 2.6 million resulted in judgments, 1 million in orders, and 5.8 million in decrees.[19] I have also collected data on approximately 30 thousand appeals from five Courts of Appeal. 48 percent of these appeals were accepted, either fully or partially, resulting in a change of the first-instance decision.

On average, first-instance proceedings last 1.6 years. Judgments are the most time-consuming at 3.3 years, twice the overall average, followed by orders (2.0 years) and decrees (0.8 years). Target courts are those with the highest case backlogs and lowest case disposition rates. Consistent with this, proceedings in target courts take longer than in home and control courts, with average durations of 3.8 years for judgments, 2.2 years for orders, and 0.9 years for decrees.

[19] Multiple orders or decrees may relate to the same case. I treat each of them as a separate proceeding.

Table 1: Sample structure

	(1) Full sample	(2) Target courts	(3) Home courts	(4) Control courts
# Judges	6242	2712	3196	1151
# Incentivized judges	139	0	139	0
# Movers	1453	758	908	359
# Courts	140	48	55	37
# Courts with at least 1 mover	140	48	55	37
# Connected sets	1	1	1	1
# All first-instance proceedings	9491104	3275164	4983517	1232423
# judgments	2614452	976072	1350940	287440
# orders	1060850	391643	537018	132189
# decrees	5815802	1907449	3095559	812794
# Courts of Appeal	5	5	3	4
# Appeals	30067	12062	3886	14119
# Accepted or partially accepted	14536	6155	1927	6454
First-instance duration (years)	1.64	1.93	1.56	1.22
Judgment duration	3.28	3.77	3.05	2.71
Order duration	2.00	2.24	1.96	1.47
Decree duration	0.84	0.93	0.84	0.66
Appeal duration	2.71	2.61	2.92	2.75

Note: The table reports the characteristics for all courts in column (1), for target courts of the incentive scheme in column (2), for home courts of the incentivized judges in column (3), for courts that were neither target not home of incentivized judges in column (4). *Judges in more than 1 court* represents the number of judges that ruled on first-instance cases belonging to at least two courts over my sample period. All first-instance proceedings include first-instance judgments, orders, and decrees. Duration is the average number of days from January 1^{st} of the starting year to the exact end date of the proceeding, divided by 365.

Productivity. Let type k denote a unique combination of proceeding form (judgment, order, or decree) and keywords (e.g., individual dismissal for just cause). I define the quarterly productivity of judge j in quarter t, denoted by P_{jt}, as the weighted sum of the number of proceedings p_{kjt} of each type k completed by judge j in that quarter:

$$P_{jt} \equiv \sum_k p_{kjt} \cdot \frac{\bar{d}_k}{\bar{d}} \tag{1}$$

The weight $\frac{\bar{d}_k}{\bar{d}}$ captures the relative complexity of type-k proceedings, defined as the ratio of the national average duration of type k proceedings ($\bar{d}_k$) to the national average duration of all proceedings across all types over the sample period ($\bar{d}$). As a result, P_{jt} provides a standardized measure of quarterly judicial productivity, where one unit corresponds to completing a proceeding of average complexity in a quarter at the national level.

I interpret this as a measure of productivity rather than output, since all judges have the same number of paid working hours, overtime is not compensated and no judge was granted a part-time contract during the sample period (Consiglio Superiore della Magistratura, 2018; Ministero della Giustizia, 2025b).[20] Also, all specifications include quarter or month fixed effects to absorb any variation in the number of working days across periods. The measure captures productivity per unit of contractual time, which may also reflect longer unpaid hours. From the State's perspective, however, this is the relevant outcome: output produced within a fixed amount of contractual time.

Note that complexity-weighted productivity is not driven by the allocation of cases across judges. Within each Italian court, cases are assigned through pre-established random procedures mandated by Article 25 of the Constitution, ensuring that assignment is independent of case characteristics. Judges may be replaced only under limited statutory circumstances, with substitutions also governed by predetermined random rules (Salvestrini and Ronchi, 2026).

A common concern when measuring productivity is that observed dispersion may reflect fluctuations in demand: judges may appear less productive simply because they have fewer proceedings to work on. However, this is unlikely to be a concern in this context. All courts face a substantial backlog: on average, each judge has 565 pending proceedings at year-end, and even in the court with the smallest backlog, the figure is 121.

4 Judicial productivity prior to the incentive scheme.

4.1 Judges matter more than courts.

Productivity varies more across judges than across courts (Figures D12a–D12b). Judges at the 25^{th} percentile of the individual-level distribution complete 14 proceedings per quarter, compared with 72 for judges at the 75^{th} percentile. At the court level, the corresponding figures are closer: 43 and 70 proceedings per quarter.

I exploit the movement of judges across courts to decompose productivity into judge-

[20] Judges can take up to five months of maternity or paternity leave. When quarterly proceedings are zero, I set these observations to missing because the dependent variable is expressed in logarithms.

specific, court-specific and time-specific components. To assess the contribution of judges and courts to variation in judicial productivity, I estimate the following two-way fixed effects specification at the judge-quarter level (Abowd et al., 1999):

$$\ln(P_{jt}) = \alpha_j + \psi_{c(j,t)} + \tau_t + \beta X_{jt} + u_{jt} \tag{2}$$

The dependent variable is the natural logarithm of complexity-weighted productivity as defined in Equation (1). The term α_j captures judge effects and $\psi_{c(j,t)}$ denotes court effects, where $c(\cdot,\cdot)$ is a function that allocates each judge-quarter observation to a court c.[21] τ_t represents quarter fixed effects and accounts for seasonality and common shocks. The vector X_{jt} includes indicators for the pre-transfer quarter and each quarter in the two years following a court transfer to account for adjustment effects (see Section 4.2).[22] u_{jt} is the error term.

In Table 2, I examine how the adjusted R^2 from a regression of the logarithm of complexity-weighted productivity changes as time, court, and judge fixed effects are sequentially introduced. Column 1 includes only time fixed effects and yields an adjusted R^2 of 0.019. Columns 2, 3, and 4 add court fixed effects, judge fixed effects, and both sets of fixed effects, respectively. The adjusted R^2 increases only modestly when court fixed effects are included (to 0.059), but rises substantially when judge fixed effects are added (to 0.488). Column 4 shows that introducing court fixed effects after controlling for judge fixed effects explains almost no additional variation. Nevertheless, F-tests reject the null hypothesis that all court fixed effects or all judge fixed effects are jointly equal to zero.

[21] If a judge worked for two courts within the same quarter, I assign the judge to the modal court: the court in which they completed the largest number of proceedings during that quarter.

[22] Results are very similar when X_{jt} is excluded from the regressions.

Table 2: Analysis of variance of log productivity

	(1)	(2)	(3)	(4)	(5)
N	140194	140194	140194	140194	140194
R-squared	0.020	0.060	0.509	0.515	0.537
Adjusted R-squared	0.019	0.059	0.488	0.494	0.514
P-value courts		0.000		0.000	
P-value judges			0.000	0.000	
Time FE	YES	YES	YES	YES	YES
Court FE	NO	YES	NO	YES	NO
Judge FE	NO	NO	YES	YES	NO
Judge-by-Court FE	NO	NO	NO	NO	YES

Notes: This table analyzes how much of the variance in log productivity is explained by the judge, court, and time components in sample of proceedings. All regressions control for each quarter of the post-transfer year. N represents the number of proceedings. The p-value tests the null hypothesis that judge effects are jointly zero.

To better quantify the relative importance of judges, courts and court-judge match effects I apply a variance decomposition to Equation (2).[23] The variance of log productivity can be decomposed as:

$$\begin{aligned}\mathrm{Var}(\ln(P_{jt})) = {} & \mathrm{Var}(\alpha_j) + \mathrm{Var}(\psi_{c(j,t)}) + \mathrm{Var}(\tau_t) + \mathrm{Var}(u_{jt}) \\ & + 2 \cdot \mathrm{Cov}(\alpha_j, \psi_{c(j,t)}) + 2 \cdot \mathrm{Cov}(\psi_{c(j,t)}, \tau_t) + 2 \cdot \mathrm{Cov}(\alpha_j, \tau_t)\end{aligned} \tag{3}$$

Andrews et al. (2008) show that the sample variances of judge and court effects are upward biased, while the sample covariance is biased downward when covariates are orthogonal to the fixed effects. This bias becomes larger when there is limited mobility in the data (Abowd et al., 1999).[24]

Table 3 reports bias-corrected estimates of the variance of court and judge effects, as well as their covariance, using the estimator by Andrews et al. (2008). The variance decomposition in Table 3 indicates that judges account for 60 percent of the total variance in productivity. By comparison, courts explain only 11 percent. Consistent with Fenizia (2022) and Dahis et al. (2025), I observe a negative covariance between bureaucrat (judge) and

[23]For simplicity, I exclude the indicators for the post-transfer quarters X from the variance-covariance decomposition. The results are virtually unchanged when they are included.

[24]The variance of judge effects is also inconsistent regardless of the asymptotic setting, while the variance of court effects and the judge-court covariance become inconsistent when the number of courts increases with the sample size (Andrews et al., 2008).

office (court) effects: more productive judges tend to be assigned to less productive courts.

Table 3: Variance-covariance decomposition of log productivity

	(1) (Component)	(2) (Share)
Var($ln(P_{jt})$)	2.3847	100.00 %
Var(Judge)	1.4334	60.11 %
Var(Court)	0.2624	11.00 %
Var(Quarter)	0.0981	4.11 %
Var(Residual)	1.2174	51.05 %
Cov(Judge, Court)	-0.2713	-11.38 %
Cov(Judge, Quarter)	-0.0403	-1.69 %
Cov(Court, Quarter)	-0.0016	-0.07 %
N	140194	

Notes: This table reports the variance-covariance decomposition of the natural logarithm of the number of proceedings completed per month (weighted by their complexity). The model includes judge, court and quarter fixed effects. Covariances are multiplied by 2 in the shares.

The share of variation attributable to judges is higher than previous estimates for public-sector managers and bureaucrats. For instance, Janke et al. (2019) and Miglino and Smurra (2025) find that top managers in hospitals and fire departments explain little to none of the variation in performance, while other studies report more modest contributions: 9 percent for local branch managers at the Italian pension institute (Fenizia, 2022), 21 percent for procurement bureaucrats in Russia (Best et al., 2023), and 23 percent for judges in Brazil (Dahis et al., 2025). One possible explanation for the difference between my estimates and those of Dahis et al. (2025) is the level at which output is measured. Their variance decomposition is based on the total number of proceedings completed by a court in a given period, whereas my analysis uses the number of proceedings completed by an individual judge in a given period. Aggregation at the court level may reduce the contribution of individual judges by averaging productivity across multiple judges within the same court.

Overall, this section shows that judge-specific heterogeneity is the primary determinant of judicial productivity, while court-specific factors account for a comparatively small share of the observed variation. The evidence presented in Tables 2 and 3 suggests substantial scope for improving the performance of the judicial system through policies aimed at enhancing judges' productivity.

4.2 Identification.

Identification of the permanent components of judges and courts relies on the assumption that judge mobility is as-good-as random, conditional on court and quarter-year fixed effects. This allows judges to be assigned to proceedings of a court on the basis of the permanent components of court (ψ_c) or judge (α_j) productivity. That is, more productive judges sorting into more productive courts would not violate the identifying assumptions.

Following Card et al. (2013), I assume that the error term in Equation (2) consists of three random effects:

$$u_{jt} = \eta_{jc(j,t)} + \zeta_{jt} + \varepsilon_{jt} \tag{4}$$

$\eta_{jc(j,t)}$ is an idiosyncratic productivity premium specific to the match between judge j and court c. ζ_{jt} is a unit-root component representing drift in the portable part of judge j's productivity, potentially influenced by factors such as human capital accumulation or health shocks. ε_{jt} is a transitory shock to judge productivity.[25]

Card et al. (2013) show that three forms of endogenous mobility can bias the estimates of the fixed effects. The first is sorting driven by the match component of productivity $\eta_{jc(j,t)}$, whereby judges move to another court seeking a productivity premium specific to that judge–court match. If sorting based on this comparative advantage were important, the productivity loss from moving from the most to the least productive courts would be smaller (in absolute value) than the productivity gain from moving in the opposite direction. Figure 4 plots the average log productivity of judges, relative to the quarter of the transfer, by terciles of the change in court fixed effects ($\psi_{destination} - \psi_{origin}$). Once we exclude two years after the transfer (Figure 4b), the average decrease in judges' log productivity for the first tercile is symmetric to the productivity gain for the third tercile. This symmetry suggests that sorting on comparative advantage is not quantitatively important.

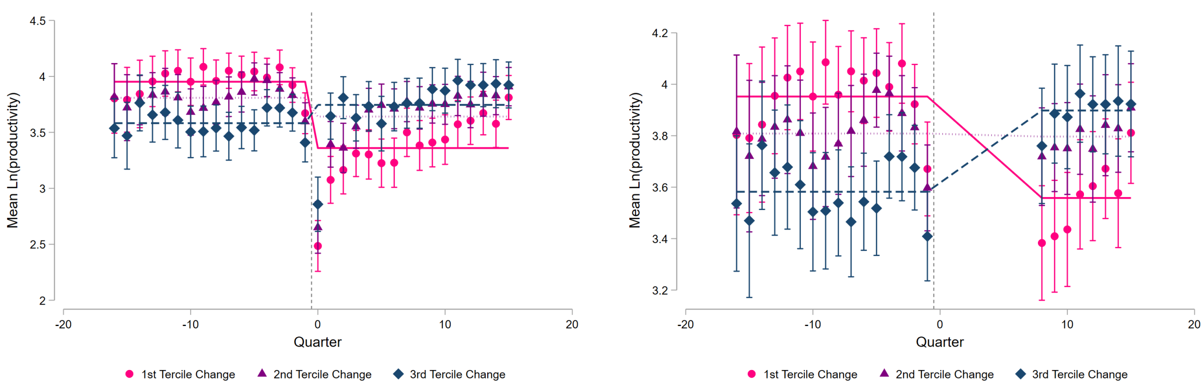
Further evidence comes from comparing the explanatory power of a model with judge-by-court fixed effects to the additively separable specification in Equation (2). If match effects were important, the former should deliver a substantially higher R^2. The model fit improves only marginally when judge-by-court interaction effects are added in Column 5, with the adjusted R^2 increasing from 0.49 to 0.51. This suggests that match-specific effects

[25]The assumptions are that: $\eta_{jc(j,t)}$ has mean zero for all judges and courts in the sample interval; both ζ_{jt} and ε_{jt} have mean zero for each judge in the sample interval, but the former contains a unit root.

between judges and courts play a relatively minor role in this setting, validating the two-way fixed effect specification. Figure D14 also shows that average residuals within quartiles of the estimated judge and court effects are small in all cells, with the largest mean residual equal to 0.01 in absolute value. These results support the additive separability assumption and indicate that match effects account for little of the variation in judges' productivity.

A second source of endogenous mobility comes from the drift component ζ_{jt}. If judges experiencing rising productivity were systematically assigned to higher-performing courts, the model would overstate the role of court effects, as the drift in judges' productivity would be positively correlated with changes in court effects. Figure 4 shows no clear pre-trends across the three terciles. The only noticeable pattern is a decline in productivity during the quarter preceding a court transfer, which is consistent across all terciles and likely reflects a reduction in case completions as judges hand over pending cases before leaving their current court.

Figure 4: Average log productivity for judges experiencing a long-term change in court, by tercile of changes in court FE.



(a) All quarters

(b) Excluding post-transfer adjustment period

Notes: Both panels show the average log productivity and the associated 95-percent confidence intervals four years before and after a long-term change in court. A long-term change is defined as a transfer in which a judge spends at least one year in both the outgoing and incoming courts. Panel (a) includes all quarters, whereas panel (b) excludes two years after the transfer. Both panels plot three types of long-term changes, by terciles of the change in court fixed effects: (1) an increase in court quality (diamonds), (2) a decrease (circles), and (3) no change (triangles). The horizontal axes report the distance in quarters relative to the long-term change in court.

A third potential source of endogeneity is the possibility that judges are reallocated in response to transitory negative or positive shocks in their performance ε_{it} (Card et al., 2013). Figure 4a shows a sharp fall in productivity immediately after a court change, followed by a gradual recovery that extends over approximately two years. More than responses to transitory negative shocks, this sharp reduction in productivity likely reflects short-term disruptions such as case handovers, administrative duties, and adaptation to new procedures, rather than pre-event selection dynamics (Ashenfelter, 1978).

Figure 4a shows that the productivity dip during the court change is observed even when judges move from the least to the most productive courts (the third tercile change). This finding provides new evidence that transfers of public-sector workers impose not only monetary costs but also productivity costs on governments. More broadly, it suggests that remote work may help reallocate state capacity without the performance losses associated with worker relocation.

4.3 Productivity costs of transfers

To formally estimate the productivity cost of judicial transfers, I regress the logarithm of quarterly complexity-weighted and unweighted proceedings completed by judge j in quarter t on indicators for event time relative to the transfer quarter T:

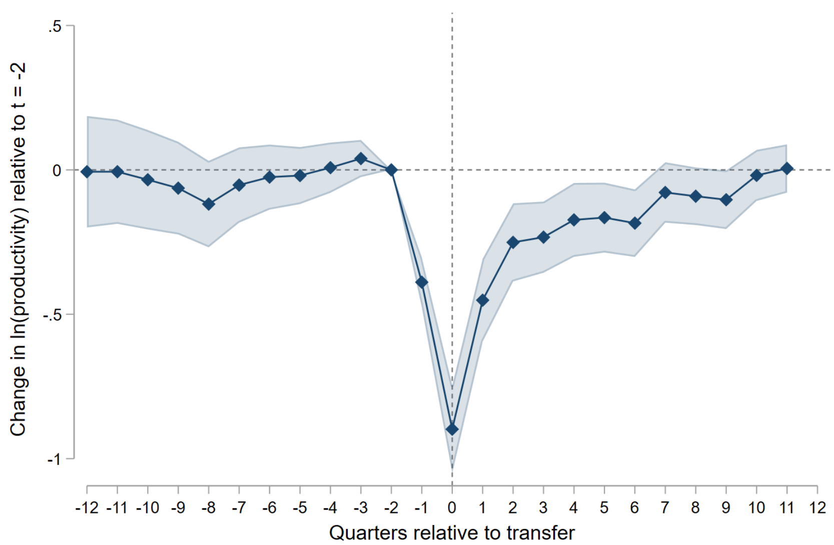
$$\ln(P_{jt}) = \alpha_j + \psi_{c(j,t)} + \tau_t + \sum_{s=-12}^{11} \delta_{T+s} \mathbf{1}\{t = T + s\} + u_{jt}. \tag{5}$$

The specification includes judge, court, and quarter fixed effects. To avoid overlapping event windows and temporary court assignments, I restrict the sample to judges who experience a single transfer and to the three years before and after the transfer.

Judicial productivity begins to decline in the quarter preceding the transfer, reaches its minimum in the transfer quarter, and gradually recovers thereafter (Figures 5 and D15). Estimates are not significantly different from zero in any earlier pre-transfer quarters, providing no evidence of differential pre-trends. They become negative and statistically significant in the quarter preceding the transfer and remain so through six quarters after, with productivity returning to its pre-transfer level after roughly two years (Table C5).

To quantify the cumulative productivity cost, I convert the estimated log changes into forgone proceedings. Given average monthly output of 42.8 proceedings per judge (Ministero della Giustizia, 2025c), the cumulative loss is $\sum_{s=-1}^{6}\left(1 - e^{\hat{\delta}_{T+s}}\right) \cdot 42.8 \cdot 3$.[26] Therefore, I estimate that a transfer is associated with 237 forgone proceedings, 193 of them at the destination court. This deadweight loss is equivalent to approximately five months of average judicial output: about one month in the origin court and four months in the destination court.

Figure 5: The productivity costs of transfers



Notes: The figure reports the estimated coefficients δ_{T+s} from Equation (5), using the logarithm of complexity-weighted quarterly productivity as the dependent variable. The estimation sample is restricted to the three years before and after a transfer to another court. The second quarter before the transfer serves as the omitted reference period. Standard errors are clustered at the judge level, and shaded areas indicate 95-percent confidence intervals.

4.4 No trade-off between productivity and quality.

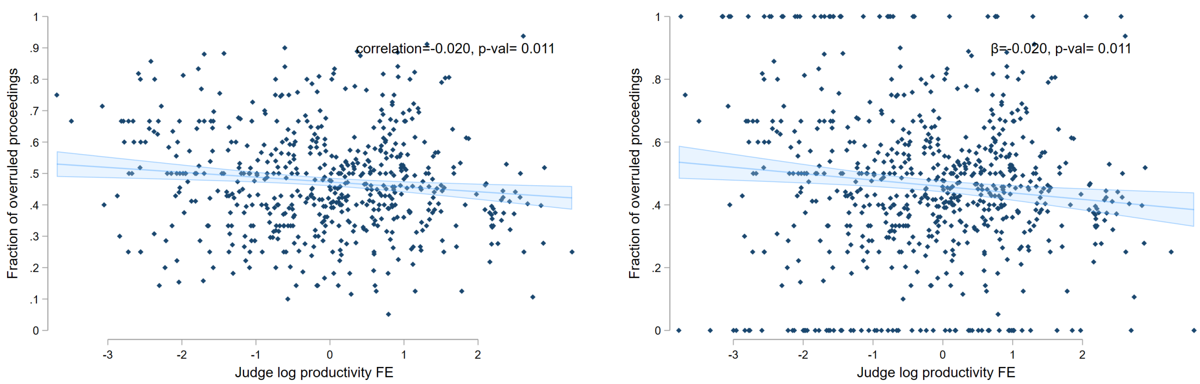
Section 4.1 documents substantial heterogeneity in judicial productivity across judges. A potential concern with incentive schemes based solely on productivity is that rewarding performance along a single dimension may adversely affect other outcomes, such as the

[26] To measure average monthly output here and throughout the paper, I use court-level data on completed proceedings and the number of judges provided by the Ministry of Justice (Ministero della Giustizia, 2025c). These data cover all completed proceedings, including those outside my dataset, such as family law cases.

quality of judicial decisions (Holmstrom and Milgrom, 1991). Assessing this possibility is crucial for understanding the broader consequences of productivity-based incentives. To this end, I examine whether a trade-off exists between judicial productivity and decision quality prior to the incentive scheme.

Figure 6 provides no evidence of a trade-off between the quantity and quality of first-instance judicial decisions. The figure shows a small and negative correlation (-0.02) between the judge fixed effects in log productivity, estimated from Equation (2), and the share of rulings overturned on appeal. This pattern does not change whether the reversal rate is computed over all first-instance rulings or only among those that were appealed. If anything, the figure suggests that more productive judges are slightly more accurate, as reflected in their lower reversal rates on appeal.[27]

Figure 6: Correlation between judge log productivity and reversal on appeal



(a) Reversal, unconditional

(b) Reversal, conditional on appeal

Notes: For each judge, both panels plot the share of first-instance rulings reversed on appeal on the vertical axis against the log productivity fixed effect estimated in Table 2 on the horizontal axis. The left panel includes all first-instance rulings, whereas the right panel restricts the sample to rulings that were appealed. The sample is limited to judges serving in courts for which appeal data are available.

To formally test the hypothesis, I estimate the following regression for first-instance ruling i by judge j in month t:

[27] Judges may have some limited discretion over whether to issue a judgment, order, or decree. The quality analysis includes type fixed effects. Residual selection may remain, although it is likely limited: 91 percent of appealed proceedings are judgments, 8 percent orders, and 1 percent decrees.

$$r_{ijt} = \mu\hat{\alpha_j} + \phi_{c(j,t)} + \xi_t + \omega_{k(i)} + \nu_{ijt} \tag{6}$$

where $\hat{\alpha_j}$ are the log-productivity fixed effects estimated in Table 2. ξ_t and $\phi_{c(j,t)}$ are month and first-instance court fixed effects. $\omega_{k(i)}$ are fixed effects for the keyword category of the proceeding. As a robustness check, one specification additionally includes appeal judge fixed effects to account for heterogeneity in appellate judges' propensity to overturn first-instance decisions.

The estimates reported in Table C6 are negative but not statistically different from zero. Doubling productivity is associated with less than a percentage point reduction in the unconditional probability of reversal, as well as in the probability of reversal conditional on appeal. Thus, there is no evidence that more productive judges produce lower-quality decisions. If anything, the estimates suggest a weak positive relation between judges' productivity and decision quality, consistent with evidence from other areas of the public sector (Fenizia, 2022; Best et al., 2023; Deserranno et al., 2025).

Another commonly used measure of judicial quality is case duration, as timely dispute resolution is essential for the effective protection of legal rights (Mocetti et al., 2026). Unlike productivity, however, case duration appears to reflect judges' idiosyncratic ability only to a limited extent. The AKM estimates and the variance decompositions reported in Tables C7 and C8 show that the type of proceeding accounts for 64 percent of the variation in case duration, whereas judge effects account for only 2.6 percent, the same share as court effects.[28] This pattern is consistent with substantial heterogeneity across types of proceedings and with court-level congestion: even a highly productive judge may face long case durations when assigned complex proceedings or when operating in a court with a large backlog. With this caveat in mind, Figure D16 and Table C9 show a modest negative association between the judge fixed effects for productivity and case duration: doubling productivity is associated with approximately a 1–4 percent reduction in proceeding duration, depending on the specification.

[28] For the AKM analysis of case duration, I estimate

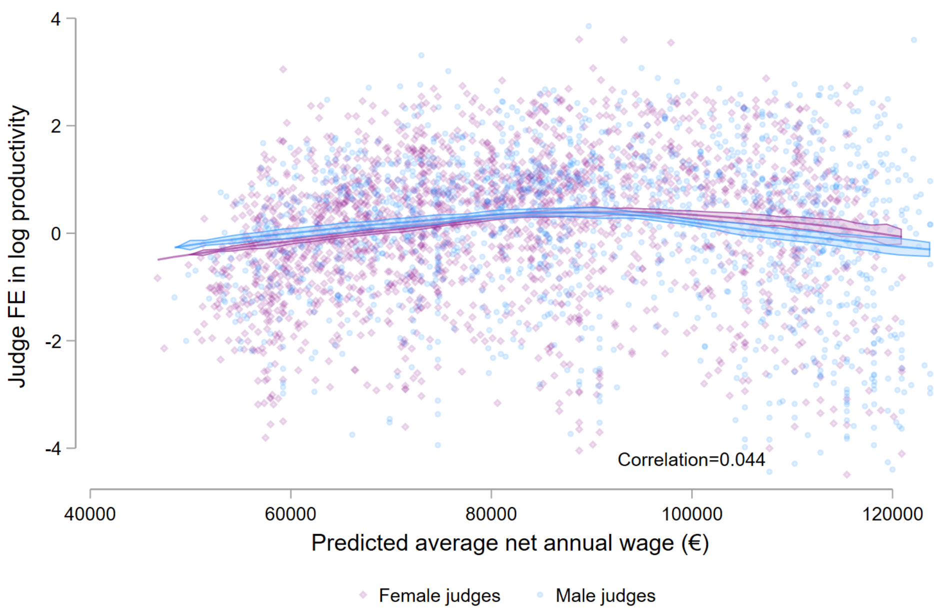
$$\ln(D_{ijt}) = \gamma_j + \phi_{c(j,t)} + \theta_t + \kappa_{k(i)} + \nu_{ijt}, \tag{7}$$

where $\ln(D_{ijt})$ denotes the natural logarithm of the number of days between January 1 of the year in which proceeding i was filed and the date of the first-instance ruling. The terms γ_j, $\phi_{c(j,t)}$, θ_t, and $\kappa_{k(i)}$ denote judge, court, quarter, and proceeding-type fixed effects, respectively.

4.5 More productive judges are not better paid.

In the Italian justice system, career progression is determined largely by seniority, as in most public-sector organizations (Bertrand et al., 2020). Every two years of service, a judge receives a 2.5% salary increase (Dipartimento della Funzione Pubblica, 2025). Judges also undergo seven professional evaluations by the Superior Council of the Judiciary (CSM), one every four years, which, upon passing, move them into a higher salary band (Area Democratica per la Giustizia, 2025; Dipartimento della Funzione Pubblica, 2025). These evaluations are not meaningful performance-based incentives. Figure D11 shows that 99 percent of judges received a positive evaluation and the associated pay increase in 2021-24 (Negri, 2026).

Figure 7: Correlation between wages and judge FE in log productivity



Notes: This figure shows how the judge fixed effects in log productivity vary by judges' predicted wages, separately for female and male judges. The shaded areas represent the 95-percent confidence intervals of a kernel-weighted local polynomial regression of the judge FE on predicted net annual wage. Predicted net annual wage is predicted, assuming the judge started their career at age 25 and passed each of the seven professional evaluations every four years (Dipartimento della Funzione Pubblica, 2025; Area Democratica per la Giustizia, 2025).

This raises the question of whether the highest-paid judges are also the most productive. Although I do not observe individual judges' wages, I reconstruct them using the average gross compensation (*oneri medi*) paid by the State for each salary band (Dipartimento della Funzione Pubblica, 2025). To impute earnings profiles, I assume that judges enter the profession at age 25 and pass each of the seven professional evaluations at the first available opportunity.

Under these assumptions, predicted wages increase stepwise with age until retirement. Figure 7 plots the judge fixed effects in log productivity from Equation (2) against predicted average annual net earnings. The relationship is weak and slightly concave, with a correlation of 0.032. Overall, the evidence indicates that, prior to the introduction of the incentive scheme, more productive judges did not receive higher pay.

4.6 Productivity increases in specialization and backlog.

To investigate what predicts judicial productivity, I regress the estimated judge fixed effects from Equation (2) on observable characteristics of the judge and of the court in which they work. Figure 8 reports coefficients from a multivariate regression of the judge effect in log productivity. All variables are standardized, so coefficients measure the association between a one–standard deviation change in the predictor and the judge's impact on log productivity.

Although the evidence is correlational, Figure 8 suggests that specialization and backlog are relevant drivers of judges' productivity. I measure specialization using a Herfindahl–Hirschman Index computed over the distribution of completed proceedings across subject matters for each judge. The index ranges from $1/N$, corresponding to an equal distribution of completed proceedings across N subject matters, to 1, indicating that all completed proceedings fall within a single subject matter. Higher values therefore reflect greater specialization.[29] An average judge handles cases spanning 19 different subject matters and a reduction of one subject matter is associated with a 1 percent increase in productivity.

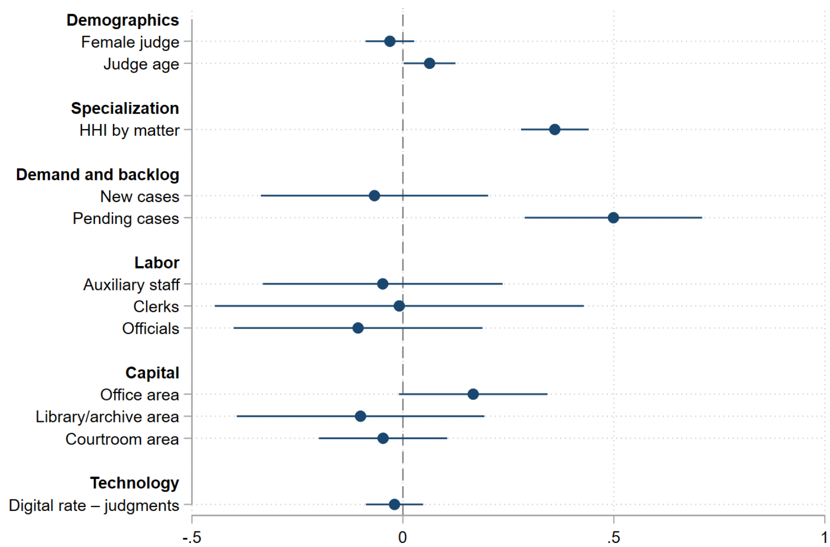
The positive correlation between backlog and productivity explains why target courts, despite facing the largest backlogs, are more productive on average (Figure D5).[30] This

[29]Time-varying judge and court characteristics are averaged across all proceedings resolved by a given judge. For judges serving in multiple courts, court-level variables are constructed as weighted averages across courts, using the number of resolved proceedings as weights.

[30]This correlation may partly reflect the introduction of the *Ufficio per il Processo*, a judicial support

finding suggests that productivity improvements within backlogged courts may be insufficient to reduce spatial disparities, motivating policies such as remote work to draw on judicial capacity from other jurisdictions.

Figure 8: Predictors of judges' productivity fixed effects



Notes: The figure shows the coefficients of a multivariate regression of judge fixed effects of log productivity on a set of predictors. Judge fixed effects are estimated as in Equation (2). Labor, capital, technology and demand covariates refer to the court in which the judge is working. Demographics and specialization covariates are at the judge level.

4.7 Incentivized judges were ex-ante more productive.

A unique feature of this paper is the combination of an AKM decomposition of judicial productivity with an event-study analysis of a voluntary productivity-based incentive scheme. The AKM framework identifies judges' underlying productivity in the absence of performance pay, while the event-study design allows me to examine which judges choose to participate once monetary incentives are introduced. This approach provides novel evidence on the self-selection of workers into performance-based pay in the public sector, allowing to assess

program that, beginning in 2022, hired additional staff to assist judges with legal research, case preparation, and administrative tasks. Staff were disproportionately allocated to courts with larger backlogs and longer disposition times (Cannella et al., 2024).

whether the scheme attracted judges who were already highly productive or instead induced less productive judges to increase their effort.

Prior to the introduction of the incentive scheme, judges that later participated in the remote work incentive scheme were already more productive than their peers (Figure D17): they completed an average of 80 proceedings per quarter, compared with 51 among other judges. This indicates positive selection into the scheme, consistent with participants having either stronger intrinsic motivation or lower marginal cost of effort.

5 The impact of remote work incentives

5.1 Remote work incentives raise productivity of incentivized judges

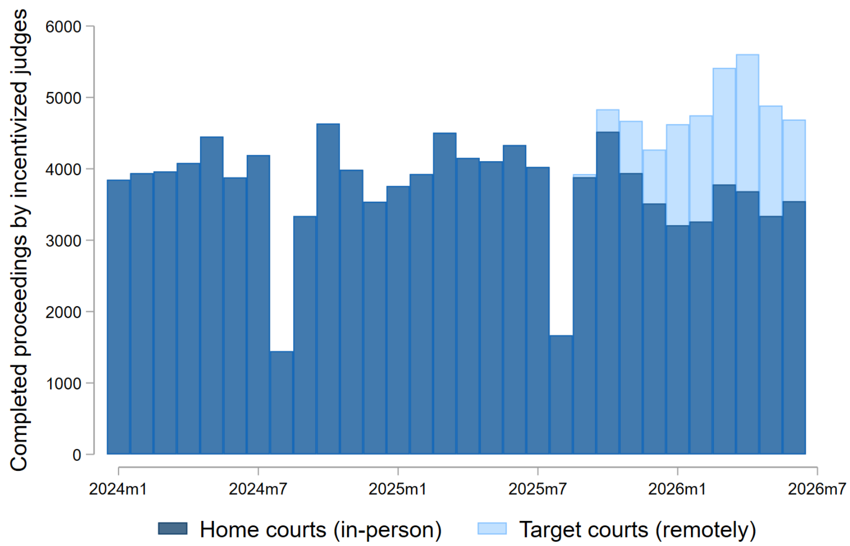
Figure 9 shows the aggregate monthly number of proceedings completed by incentivized judges, distinguishing between in-person proceedings completed for their home courts and proceedings completed remotely to assist target courts. Judicial productivity rises shortly after September 2025, when the remote work incentive scheme was introduced. While the total number of proceedings completed in April 2024 and April 2025 was below four thousand, it exceeded five thousand in April 2026.

To formally assess the dynamic impact of remote work incentives on judges' productivity, I estimate the following event-study specification for judge j in month t:

$$\ln(P_{jt}) = \sum_{s=-11}^{9} \beta_{T+s} I_j \mathbf{1}\{t = T + s\} + \alpha_j + \psi_{c(j,t)} + \tau_t + \epsilon_{jt}, \text{ for } t \in [T-12, T+9] \tag{8}$$

where T denotes September 2025, the month in which judges applied to participate in the incentive scheme. I_j equals 1 if judge is a participant of the incentive scheme, 0 otherwise. α_j, $\psi_c(j,t)$, τ_t are judge, home court and month fixed effects. β_{T+s} represent the coefficients of interest: for $s \in [0, 9]$ the coefficients test for dynamic treatment effects during the incentive period, for $s \in [-11, -1]$ the coefficients indirectly test for the validity of the assumption of parallel trends between incentivized and non-incentivized judges. In the main specifications, I use all non-incentivized judges as the control group. To account for seasonality and facilitate comparisons with the same calendar month in the preceding year, September 2024 is omitted as the reference period.

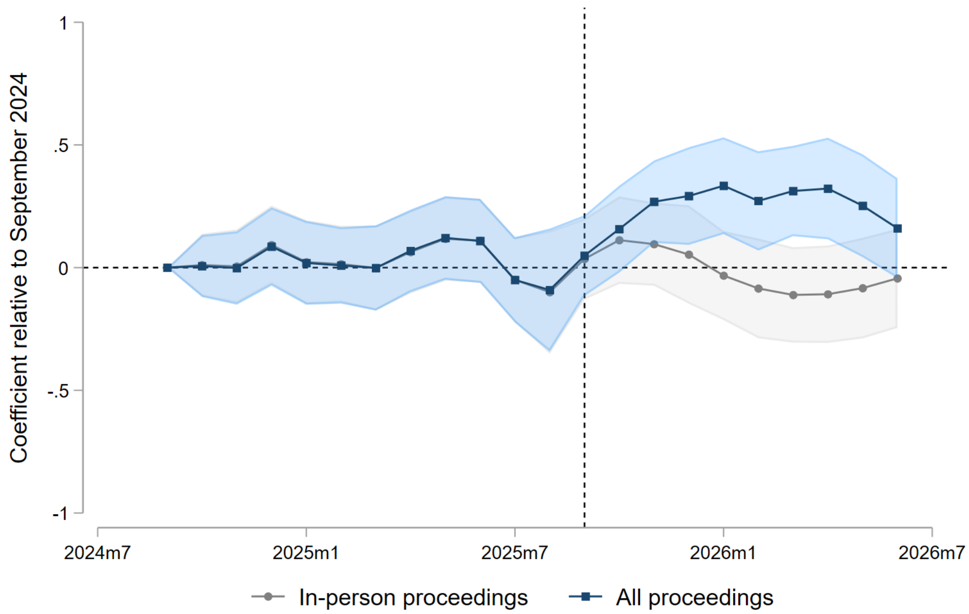
Figure 9: Proceedings completed by judges participating in the remote work incentive scheme.



Notes: The figure shows the total number of proceedings completed each month by judges participating in the remote work incentive scheme. Dark blue bars denote proceedings completed for home courts, for which judges received no additional compensation. Light blue bars denote proceedings completed remotely for target courts, for which judges were eligible for incentive payments. The incentive scheme was introduced at the end of September 2025.

Figure 10 reports the point estimates and 95-percent confidence intervals for β_{T+s}, using as dependent variables the logarithm of total complexity-weighted productivity (in blue) and the logarithm of in-person complexity-weighted productivity (in gray). Supporting the validity of the parallel-trends assumption between incentivized and non-incentivized judges, none of the pre-treatment coefficients is statistically different from zero. In contrast, the post-treatment coefficients for total productivity become significantly positive beginning in November 2025, stabilize at around one-third above pre-treatment levels, and decline again at the end of the scheme. Results using unweighted productivity measures are qualitatively similar, although smaller in magnitude, suggesting that remotely completed proceedings were more complex than the average proceeding (Figure D18).

Figure 10: The effects of remote work incentives on incentivized judges' total and in-person productivity.



Notes: The figure plots the event-study coefficients β_{T+s} from Equation (8), with September 2024 as the omitted reference month. The blue line shows estimates for log complexity-weighted total proceedings completed per month (in-person and remote), while the gray line shows estimates for log complexity-weighted in-person proceedings only. Shaded areas denote 95-percent confidence intervals.

To quantify the overall impact of the incentive scheme, I estimate the average treatment effect on the treated using the following difference-in-differences specification over the entire sample period from 2016 to 2026:

$$\ln(P_{jt}) = \beta I_j \cdot Post_t + \alpha_j + \psi_{c(j,t)} + \tau_t + \sum_{s=1}^{12} \lambda_s I_j \cdot \mathbf{1}\{m = s\} + \epsilon_{jt} \tag{9}$$

where $Post_t$ is an indicator equal to one for months between September 2025 and June 2026, and zero otherwise. To account for potential differences in seasonal productivity patterns between participating and non-participating judges, the specification also includes interactions between the participation indicator and month-of-year fixed effects (e.g., January).[31] The coefficient of interest, β, captures the average effect of the incentive scheme on participating judges, so that $(100 \cdot \beta)$ can be interpreted approximately as the percentage increase in productivity induced by the remote work incentive.

Table 4: The effect of remote-work incentives on the productivity of incentivized judges

	Proceedings (1)	Ln(Proceedings) (2)	In-person proceedings (3)	Ln(in-person proceedings) (4)
Post*Incentivized judge	7.439***	0.302***	-4.465**	0.007
	(1.897)	(0.062)	(1.811)	(0.066)
N	399842	399842	399842	399147
Mean	23.414	2.541	23.366	2.540
R-squared	0.417	0.447	0.417	0.447
Time FE	YES	YES	YES	YES
Judge FE	YES	YES	YES	YES
Court FE	YES	YES	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the effect of the incentives on the productivity of judges participating in the incentive scheme. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2); the number of proceedings completed in-personly (column 3); the logarithm of the number of proceedings completed in-personly (column 4). All proceedings are weighted by their complexity. All specifications include fixed effects for decision month, home court and judge fixed effect. In all regressions we control for the interaction between each one of the twelve months and the indicator for incentivized judges. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme.

Remote work incentives led participating judges to complete approximately 7 additional complexity-weighted proceedings per month relative to a pre-treatment average of 23, corresponding to a 30 percent increase in weighted productivity (Table 4). Although judges completed 4 fewer in-person complexity-weighted proceedings per month, this effect is statistically different from zero only in the linear and absent in the log specification.

The effects are smaller when productivity is not weighted by case complexity. Incentivized judges completed 4 additional proceedings per month relative to a pre-treatment average of 24 (column 1 of Table C10), implying an increase in unweighted productivity of approximately 16 percent (column 2). At the same time, participating judges completed about 3 fewer in-person proceedings per month, although these estimates are only marginally significant (column 3). Put differently, the completion of 7 remote proceedings per month more than offset the decline in in-person activity, resulting in a net increase in productivity. These estimates capture the overall effect of the remote work incentive scheme and may partly reflect administrative support provided by target courts. When the control group is

[31] The main results remain qualitatively unchanged when these controls are omitted.

restricted to judges serving in control courts, the positive effects of the incentives are qualitatively unchanged but the negative impact on in-person proceedings disappears (Tables C13-C14).

5.2 Absence of negative spillover effects on other judges.

Reallocating judges' effort toward target courts could generate negative spillover effects on non-incentivized judges in both home and target courts. For instance, non-incentivized judges may be required to absorb additional administrative burdens or devote time to coordinating with remotely assigned colleagues. To test for spillover effects, I re-estimate Equation (9) replacing I_j with indicators for non-participating judges working either in home courts of incentivized judges or in target courts. The control group consists exclusively of judges serving in control courts.

Figures D19a-D19b provide no evidence of negative spillover effects. For non-participating judges in home courts, none of the post-September 2025 coefficients is statistically different from zero. For judges in target courts, the only statistically significant post-treatment coefficient is positive.[32]

Tables C11–C12 provide no evidence of adverse effects of the remote work incentive scheme on judges who did not participate. Productivity among non-participating judges in home courts is unaffected, while the estimates for judges in target courts are positive. The latter should not necessarily be interpreted as a positive spillover from remote adjudication, since target courts were also subject to other PNRR interventions, such as the allocation of additional administrative staff (Cannella et al., 2024).

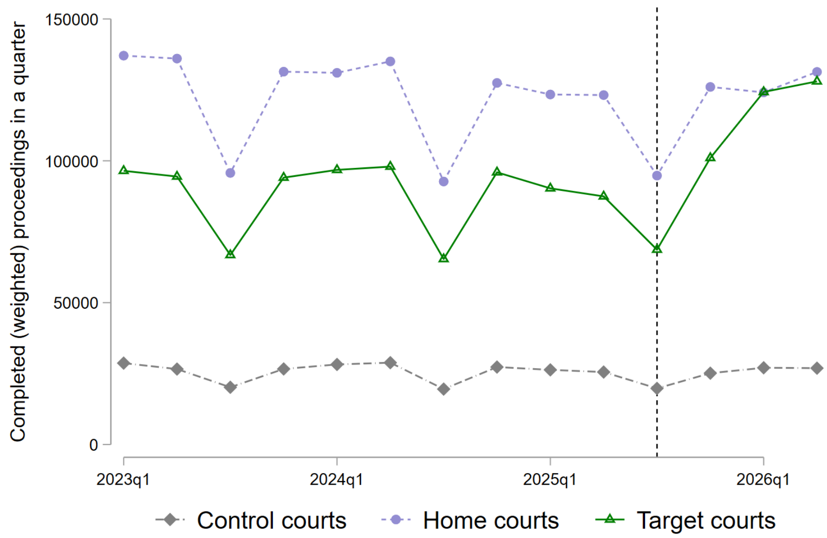
5.3 Remote work incentives reduce spatial gaps in state capacity.

To assess whether remote work incentives reduced spatial disparities in state capacity, I begin by examining whether the aggregate number of complexity-weighted proceedings completed in target courts changed following the introduction of the scheme in the last quarter of 2025. This analysis includes proceedings completed remotely by incentivized judges into the total

[32] A negative coefficient is estimated for August 2025. This appears to reflect differences in holiday-taking behavior between judges in target and control courts rather than an anticipation effect. I therefore exclude August 2025 from the difference-in-differences analysis reported in Tables C11-C12.

number of proceedings completed by target courts.

Figure 11: Completed proceedings in target, home, and control courts before and after the introduction of remote work incentives.



Notes: The figure shows the total number of completed proceedings (weighted by their complexity). The blue line shows estimates for log complexity-weighted total proceedings completed per month (in-person and remote), while the gray line shows estimates for log complexity-weighted in-person proceedings only. Shaded areas denote 95-percent confidence intervals.

Figure 11 shows a sharp increase in output at target courts following the introduction of the incentive scheme, closing the gap in complexity-weighted proceedings relative to home courts. By the first quarter of 2026, target courts were completing approximately one third more complexity-weighted proceedings than in the corresponding quarter of the previous year.

To formally test whether remote work incentives mitigate spatial inequality in state capacity, I estimate the following event-study and difference-in-differences regressions collapsing the data at court-month level:

$$\ln(P_{ct}) = \sum_{s=-11}^{9} \gamma_{T+s}\, Target_c \mathbf{1}\{t = T + s\} + \theta_c + \pi_t + v_{ct}, \text{ for } t \in [T - 12, T + 9] \tag{10}$$

$$\ln(P_{ct}) = \gamma \, Target_c \cdot Post_t + \theta_c + \pi_t + \sum_{s=1}^{12} \rho_s \, Target_c \cdot 1\{m = s\} + \kappa \, Target_c \cdot t + v_{ct} \quad (11)$$

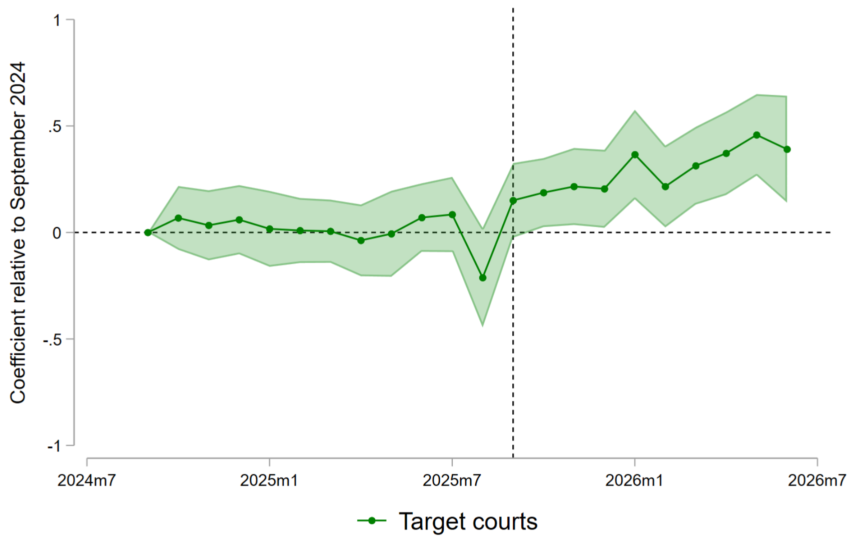
where P_{ct} are the total number of completed proceedings by judges working for court c in month t, weighted by their complexity. $Target_c$ is an indicator equal to 1 if the court is a target court, and 0 if it is a control court. Home courts are excluded from the control group to avoid capturing any reduction in the in-person productivity of incentivized judges.

Note that completed proceedings in target courts include those resolved remotely by the 139 incentivized judges identified in my sample, as well as by the additional 73 incentivized judges assigned from non-first-instance courts. In the difference-in-differences specification, I include the interaction between $Target_c$ and a time trend to account for a slight upward pre-trend in the output of target courts that can be noticed in Figure 12.

Figures 12, D20, and Table 5 show that the incentive scheme significantly increased the output of target courts during its ten months of operation. The number of proceedings completed each month in a target court increased by approximately 57 relative to a pre-treatment average of 532, while complexity-weighted output rose by 19 percent. Given 48 target courts and a ten-month implementation period, the estimates imply an increase of 27,549 completed proceedings in target courts associated with the introduction of the remote work incentive scheme.[33]

[33] This estimate captures the overall change in target-court output and therefore also includes any contemporaneous change in the productivity of incumbent judges.

Figure 12: The effects of remote work incentives on log monthly complexity-weighted proceedings completed by target courts.



Notes: The figure plots the event study coefficients, γ_{T+s}, from Equation (10), using July 2025 as the omitted reference month. The green line reports the point estimates for the log of complexity weighted total proceedings completed per month by judges working for the court, including both judges working on site and those working remotely from other courts. The shaded area represents 95-percent confidence intervals.

At the end of 2025, there were 1.2 million pending proceedings in Italian first-instance civil courts and 554 thousand pending proceedings in target courts (Ministero della Giustizia, 2025c).[34] The additional output generated by remote work incentives therefore reduced the national backlog by 2 percent and the backlog of target courts by 5 percent within ten months. In 2025, Italian first-instance civil courts completed 1.3 million proceedings, of which 453 thousand were completed in target courts. The remote work incentives therefore reduced disposition time, measured as the ratio of pending to completed proceedings, by 2 percent nationally and by 6 percent in target courts within ten months.

[34] The backlog calculation is based on proceedings recorded in the SICID administrative data. Proceedings before justices of the peace and enforcement proceedings (SIECIC) are not covered in this analysis and are therefore excluded from the backlog measure.

Table 5: The effect of remote-work incentives on monthly output of target courts

	Unweighted		Weighted	
	Proceedings (1)	Ln(Proceedings) (2)	Proceedings (3)	Ln(Proceedings) (4)
Post*Target court	57.393**	0.092***	134.611**	0.193***
	(23.144)	(0.033)	(51.922)	(0.048)
N	10709	10709	10709	10709
Mean	531.869	5.849	558.681	5.885
R-squared	0.908	0.908	0.875	0.894
Time FE	YES	YES	YES	YES
Court FE	YES	YES	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the effect of the incentives on monthly output of target courts. The dependent variables are: the number of completed proceedings in a month (column 1); the logarithm of the number of completed proceedings in a month (column 2); the number of proceedings completed in a month and weighted by complexity (column 3); the logarithm of the number of proceedings completed in a month and weighted by complexity (column 4). All specifications include fixed effects for decision month and first instance court. In all regressions we control for the interaction between each one of the twelve months and the indicator for target courts, as well as the interaction between a time trend and the indicator for target courts. Standard errors are clustered at the court level. Mean is the average of the dependent variable in target courts before the beginning of the incentive scheme.

5.4 Remote work versus worker relocation

To assess the cost-effectiveness of remote incentives as a short-run tool for reducing spatial inequality in state capacity, I benchmark them against two alternatives: producing output through the regular judicial workforce and reallocating existing judges through physical transfers.

The incentive awards three monthly gross salaries of a junior judge (€6,114 per month) for every 50 incentivized judgments (*sentenze*). As shown in Section 3.1, judgments take twice as long as the average proceeding and therefore count as two standard-equivalent proceedings under the weighting procedure described in that section. Consequently, the scheme remunerates 100 standard-equivalent proceedings.

The incentive, however, partly displaces in-person judicial activity. As shown in Table 4, every 7.439 additional standard-equivalent proceedings completed due to the incentive there was a reduction of 4.465 standard proceedings completed in person. Accounting for this displacement, the effective cost of producing one additional standard-equivalent proceeding through the incentive is $\frac{3 \cdot 6{,}114}{2 \cdot 50 \cdot \left(1-\frac{4.465}{7.439+4.465}\right)}$ = €294 per standard proceeding. As a benchmark, the average gross monthly salary of an Italian judge in 2023 was €13,334. Given an average monthly productivity of 42.8 standard-equivalent proceedings, the corresponding average cost of producing one standard-equivalent proceeding through the regular judicial workforce is $\frac{13334}{42.8}$ =€312 per standard proceeding. The incentive therefore produced additional proceedings at a cost approximately 6 percent below the average cost per proceeding.

The estimated cost savings are likely conservative. The benchmark excludes recruitment, training, and other administrative costs of expanding the judiciary. It also ignores that some judges may not complete a full batch of 50 incentivized judgments and therefore receive no incentive payment.

Another policy-relevant counterfactual is if the government tried to provide the same amount of judicial capacity to the target courts by physically transferring judges. Both physical transfers and remote-work incentives can reallocate judicial capacity toward backlogged courts. However, physical transfers completely deplete the origin court of the transferred judges' capacity and generate deadweight output losses from relocation, whereas remote-work incentives preserve most home-court capacity while increasing aggregate judicial output.

In this case, target-court output increased by an estimated 27,549 proceedings following the introduction of the remote work incentive scheme. Achieving the same increase through physical reallocation over ten months would have required transferring approximately 117 judges to target courts, given average monthly productivity of 42.8 proceedings and a deadweight loss of 193 proceedings per transferred judge at the destination court ($27549/(42.8 \cdot 10 - 193)$). Once the productivity loss at the origin court is also taken into account, each transfer generates a total deadweight loss of 237 proceedings, implying an aggregate output loss of $237 \cdot 117 = 27,729$ proceedings. In other words, reallocating capacity through physical mobility would have generated a deadweight output loss approximately as large as the additional capacity delivered to target courts.

This comparison does not imply that remote work can fully substitute for the physical reallocation of personnel. The adjustment costs of transfers are amortized over time, and some proceedings may still benefit from in-person interaction between judges and litigants. Remote work may therefore be more suitable for quickly reducing persistent backlogs or addressing temporary staffing shortages and local demand spikes.

6 Conclusions

In 2025, the Italian justice system faced a backlog of approximately 1.2 million pending first-instance civil cases, concentrated in 48 lagging courts (Ministero della Giustizia, 2025c). To reduce geographic disparities in judicial capacity, the government introduced a remote work incentive scheme that enabled judges from other courts to assist backlogged jurisdictions by adjudicating cases remotely. Given that the gross incentive paid by the government for 50 remotely completed proceedings was 18,342 euros, eliminating the entire backlog through the incentive scheme would cost 440 million euros. This is equivalent to only 0.02 percent of Italian GDP and less than one tenth of annual government expenditure on justice (ISTAT, 2026; CEPEJ, 2024). Remote work incentives increased judicial productivity, reduced backlog, and shortened disposition times in the 48 target courts, proving to be a cost-effective tool to strengthen state capacity in historically disadvantaged areas. However, they raise three potential concerns.

First, the scheme's impact is constrained by limited take-up: only 212 judges participated. Even if every participant completed 100 remote proceedings over ten months, clearing the current backlog would take 47 years. Take-up could be increased by simplifying the application process and making the payment structure more flexible. Rather than compensating judges upon completion of fixed batches of 50 judgments, the scheme could pay per case. This would reduce the risk of unpaid effort and allow judges with limited spare capacity to contribute. If scaled up, the scheme may also require complementary investments in digital infrastructure and technical staff to support judges in handling remote proceedings efficiently.

Second, rewarding judges solely for the number of completed proceedings could reduce decision quality, increasing appeals and shifting congestion to higher courts. The absence of an ex-ante negative correlation between judges' productivity and the quality of their decisions is reassuring in this sense. However, formally testing this hypothesis would require appeal data for proceedings completed between September 2025 and June 2026, which are not yet available.

Third, judges who joined the scheme were more productive than average, potentially reflecting lower effort costs or stronger intrinsic motivation. Expanding the scheme may therefore draw in less motivated judges, increasing the risk of crowding out non-incentivized in-person work or reducing decision quality.

These considerations raise the question of how remote work incentives could be redesigned to achieve larger gains while preserving the quality of judicial decisions and in-person productivity. A reasonable approach would be to implement a flexible incentive scheme that is open to all first-instance judges, rewards both in-person work and remote assistance to lagging courts, but penalizes decisions that are reversed on appeal.

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Schmutz, B. and M. Sidibé (2019). Frictional labour mobility. *The Review of Economic Studies 86*(4), 1779–1826.

A Additional figures

Figure D1: Judicial budget as a share of GDP. Notes: Shares are expressed in percent of GDP. Source: CEPEJ (2024).

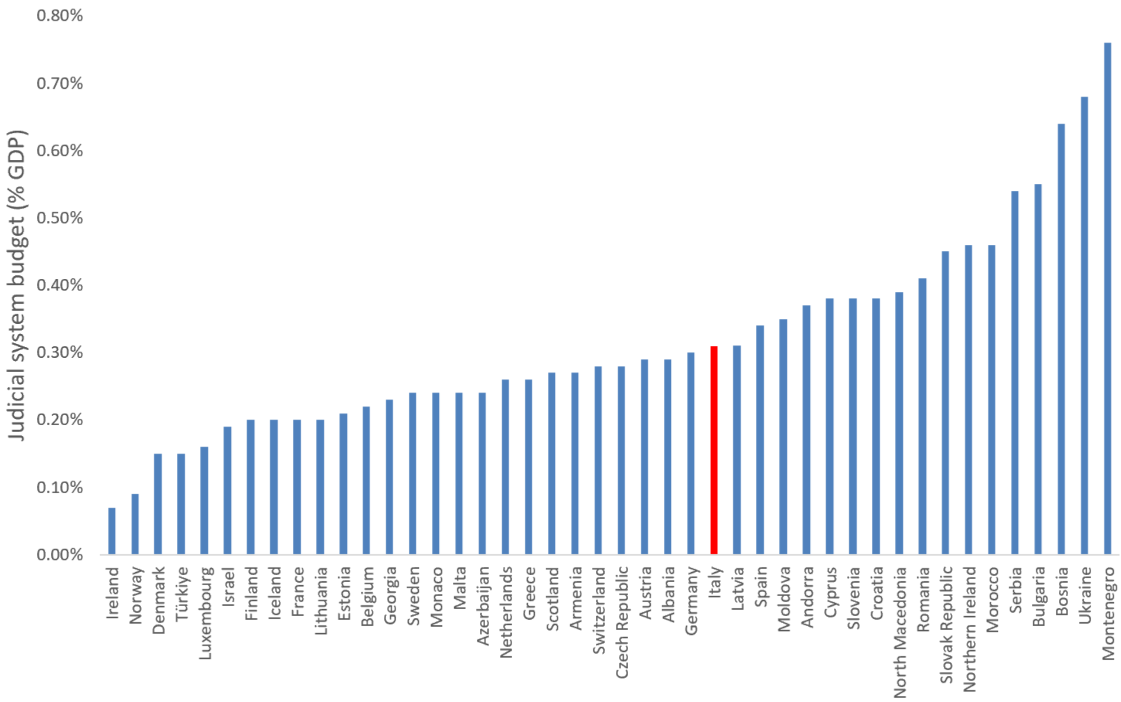


Figure D2: Disposition time in civil and commercial cases across Europe. Notes: Higher values indicate slower case processing. Source: CEPEJ (2024).

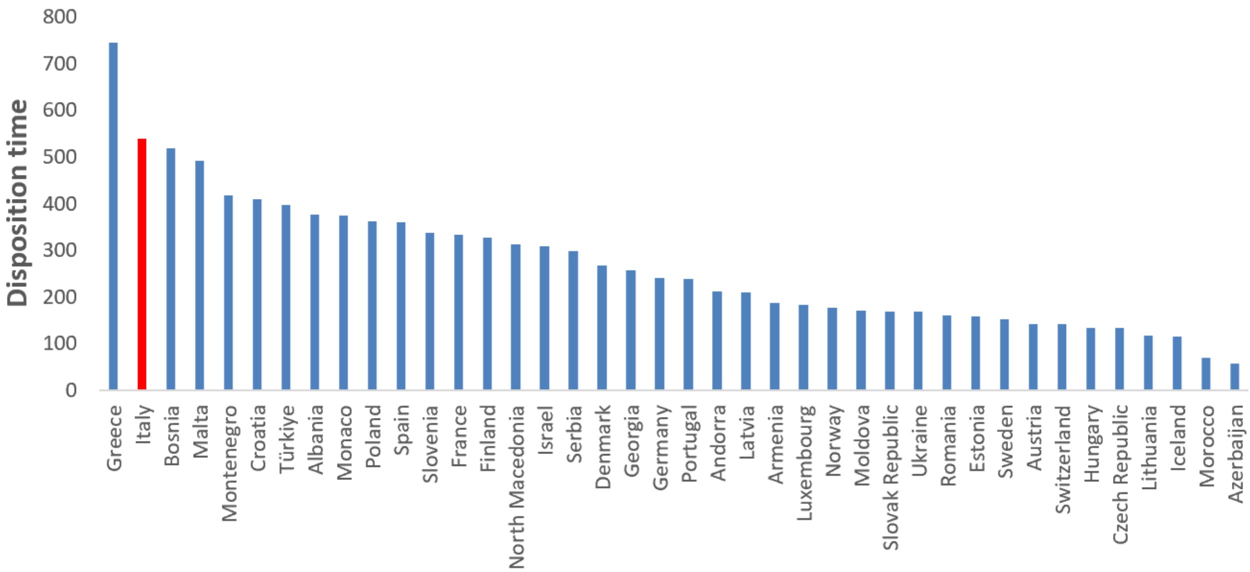


Figure D3: Gross annual salaries of judges at the end of career. Notes: Nominal gross pay, latest year available. Source: CEPEJ (2024).

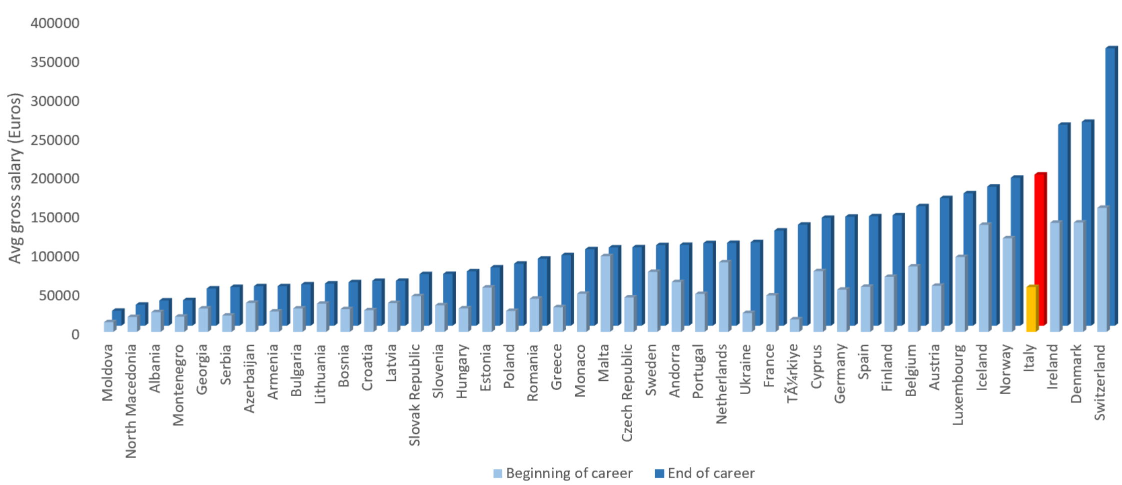


Figure D4: Number of professional judges per 100,000 inhabitants. Source: CEPEJ (2024) (CEPEJ, 2024).

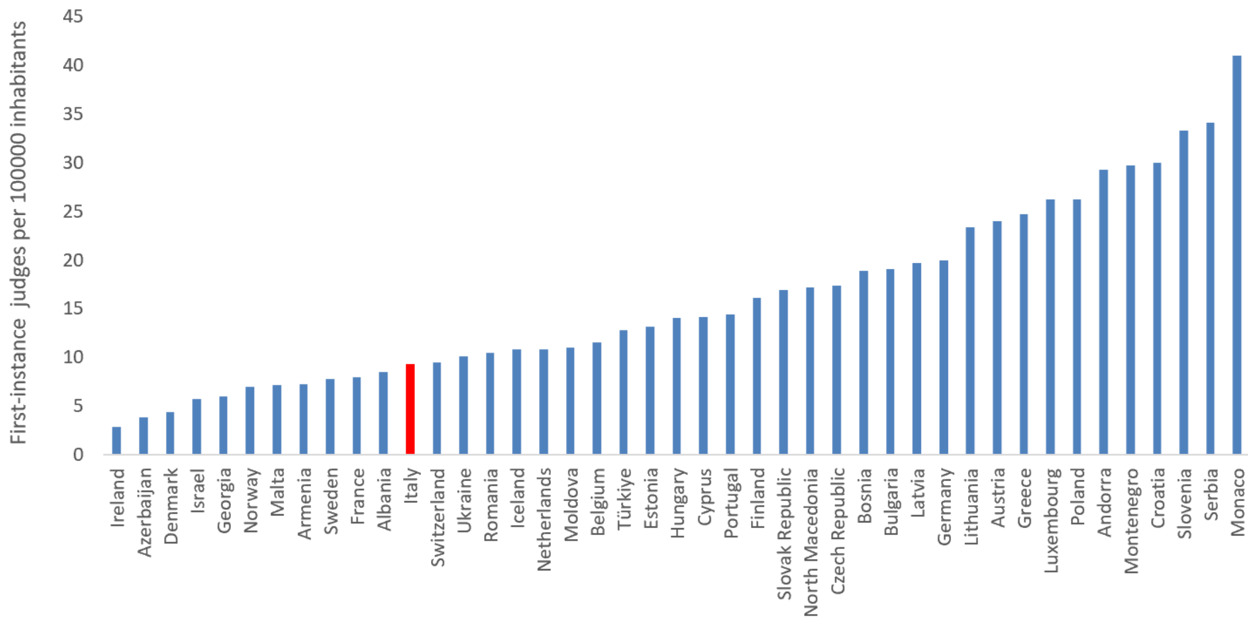
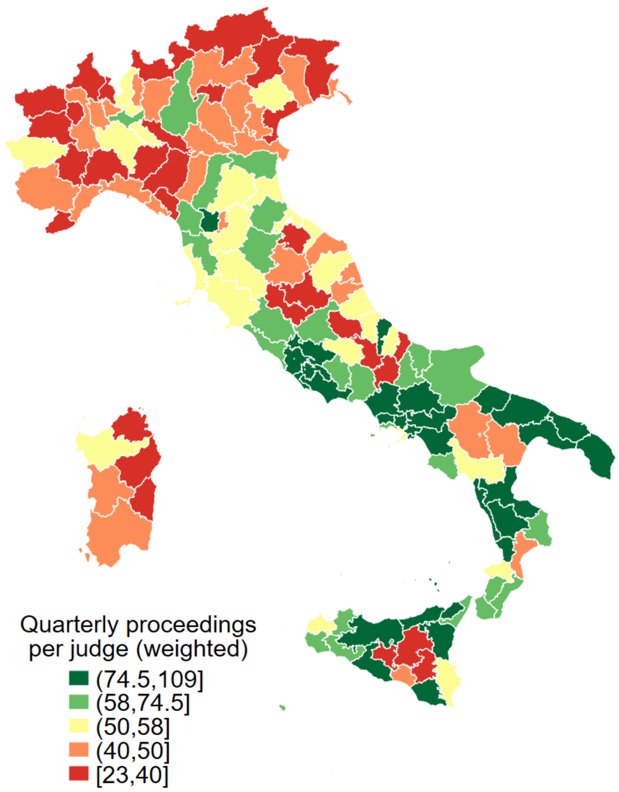
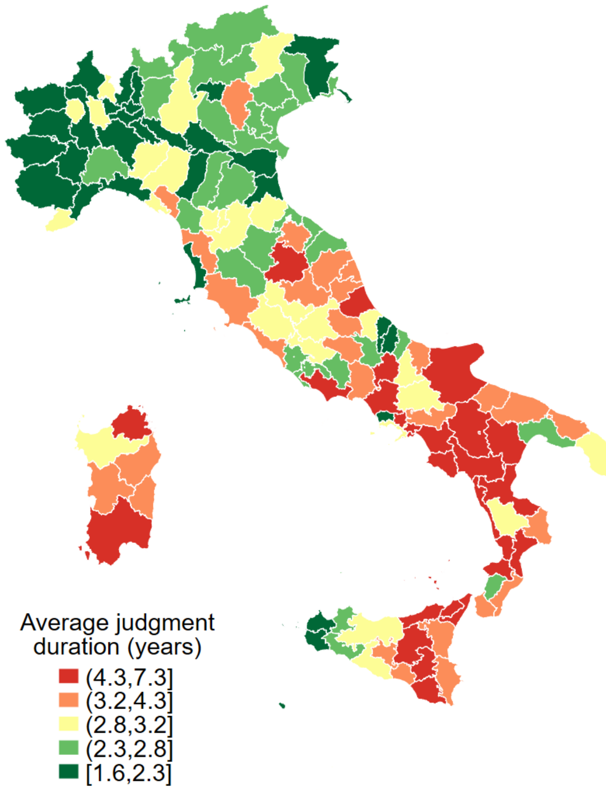


Figure D5: Average judicial productivity across courts



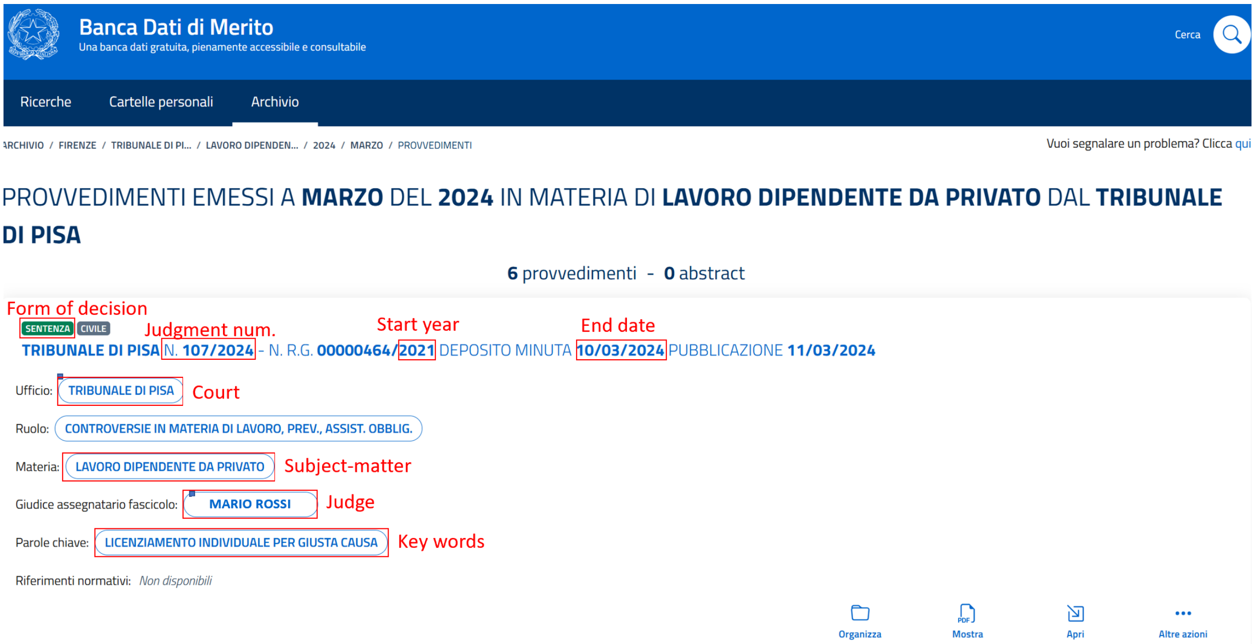
Notes: The figure shows the average number of complexity-weighted first-instance proceedings completed per judge per quarter in Italian courts between 2016 and 2025. Section 3 describes the construction of the productivity measure and the procedure used to weight proceedings by complexity.

Figure D6: Average duration of first-instance judgments.



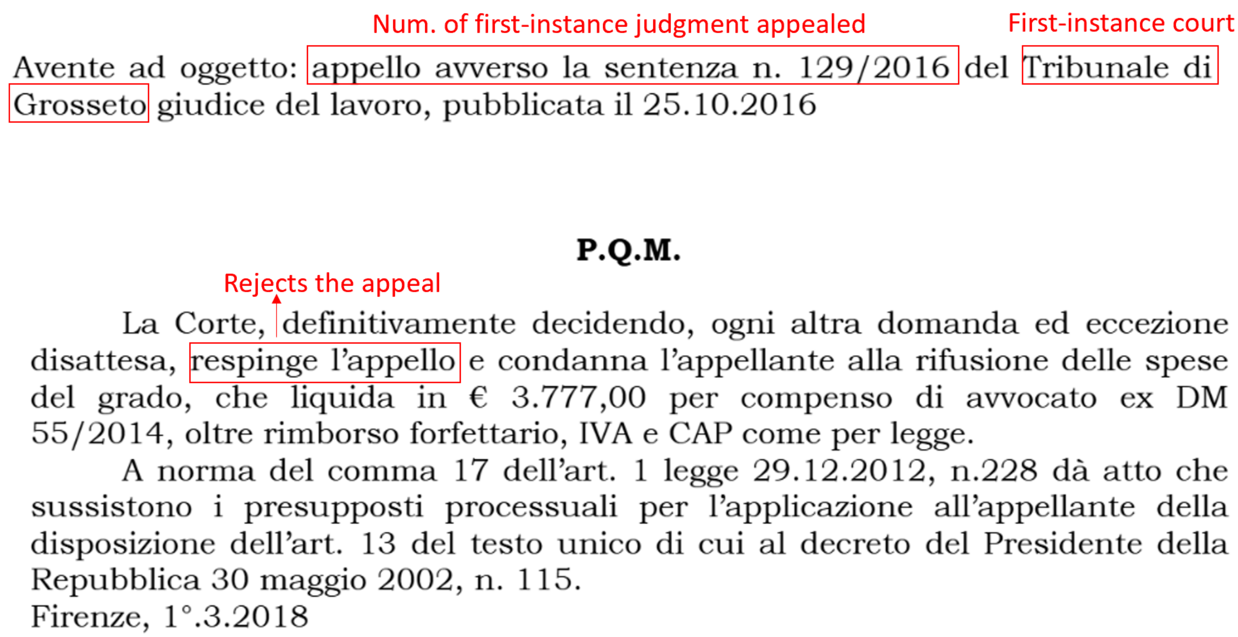
Notes: The figure shows the average duration, measured in years, of first-instance judgments (*sentenze*) completed in Italian courts between 2016 and 2025.

Figure D7: Web scraping information on first-instance civil proceedings and appeals.



Notes: The figure is a screenshot of a typical page from the online database *Banca Dati di Merito* (Ministero della Giustizia, 2026a). It displays the form of decision (first-instance judgment, decree, order, or judgment appeal), the judgment number, the starting year, the ending date, the court, the judge's full name, the subject matter, and the case keywords. I replaced the name of the judge with a pseudonym to protect the judge's privacy.

Figure D8: Web scraping additional information on civil appeals.



Avente ad oggetto: appello avverso la sentenza n. 129/2016 del Tribunale di Grosseto giudice del lavoro, pubblicata il 25.10.2016

P.Q.M.

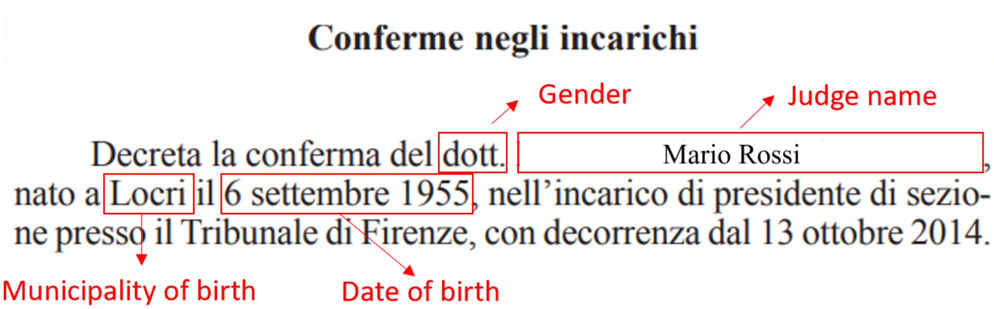
La Corte, definitivamente decidendo, ogni altra domanda ed eccezione disattesa, respinge l'appello e condanna l'appellante alla rifusione delle spese del grado, che liquida in € 3.777,00 per compenso di avvocato ex DM 55/2014, oltre rimborso forfettario, IVA e CAP come per legge.

A norma del comma 17 dell'art. 1 legge 29.12.2012, n.228 dà atto che sussistono i presupposti processuali per l'applicazione all'appellante della disposizione dell'art. 13 del testo unico di cui al decreto del Presidente della Repubblica 30 maggio 2002, n. 115.

Firenze, 1°.3.2018

Notes: The figure is a screenshot of a typical civil appeal document downloaded from the online database Banca Dati di Merito (Ministero della Giustizia, 2026a). It reports the number of the first-instance judgment under appeal, the court of first instance, and the outcome of the appeal.

Figure D9: Web scraping judges' demographics.

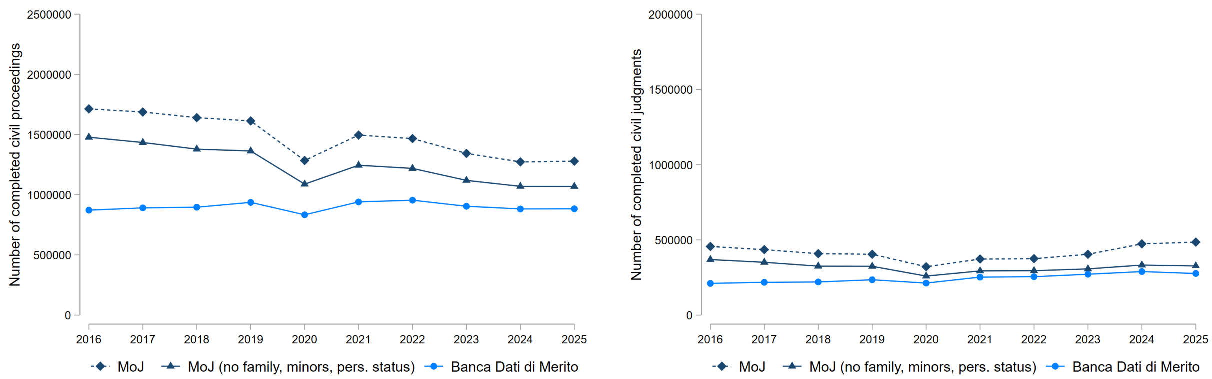


Conferme negli incarichi

Decreta la conferma del dott. Mario Rossi, nato a Locri il 6 settembre 1955, nell'incarico di presidente di sezione presso il Tribunale di Firenze, con decorrenza dal 13 ottobre 2014.

Notes: The figure presents a typical paragraph from a Ministry of Justice bulletin (Ministero della Giustizia, 2025a), which lists the judge's full name, gender, date of birth, and place of birth. I replaced the name of the judge with a pseudonym to protect the judge's privacy.

Figure D10: Number of judgments and all proceedings in 2016-2025

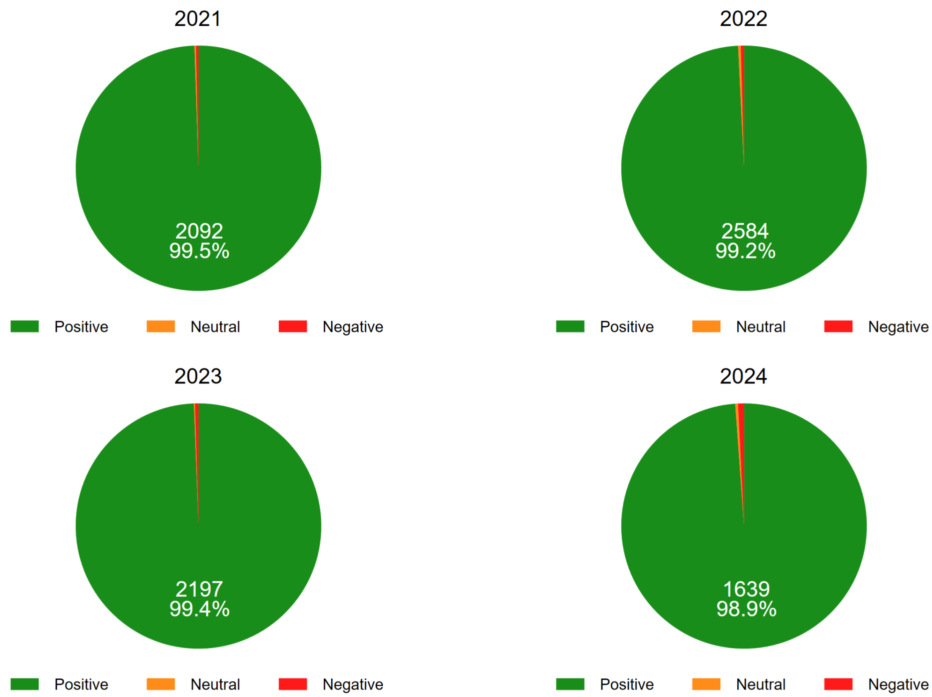


(a) All proceedings

(b) Judgments only

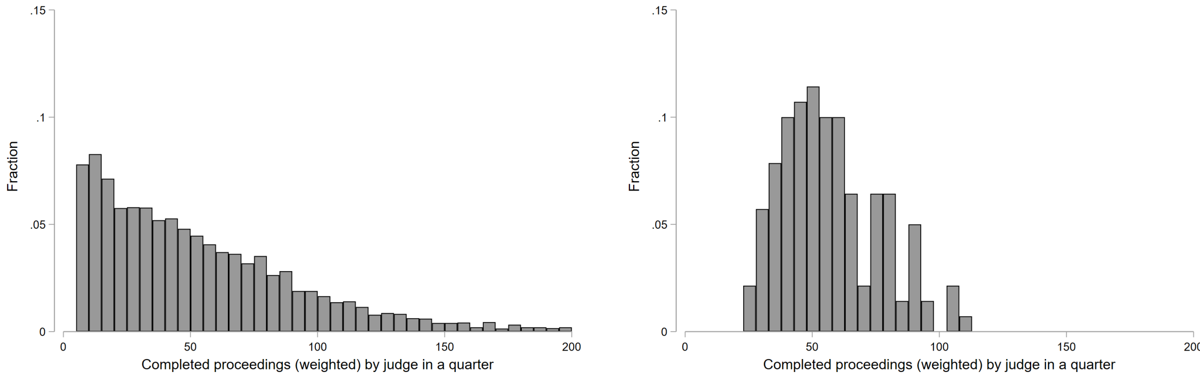
Notes: The figure compares the aggregate number of proceedings (panel a) and judgments (panel b) between 2016 and 2025 obtained through web scraping from the *Banca Dati di Merito* (light blue) with the corresponding aggregates reported by the Ministry of Justice (navy blue). Ministry figures are shown both excluding and including cases involving family matters, minors, and personal status (solid and dashed lines, respectively).

Figure D11: Judicial performance evaluations



Notes: This figure shows the outcomes of judicial performance evaluations between 2021 and 2024: positive evaluations are shown in green, neutral evaluations in orange, and negative evaluations in red. The white labels report the total number of positive evaluations and their share of all evaluations. The data are from the Ministry of Justice and are reported in Negri (2026).

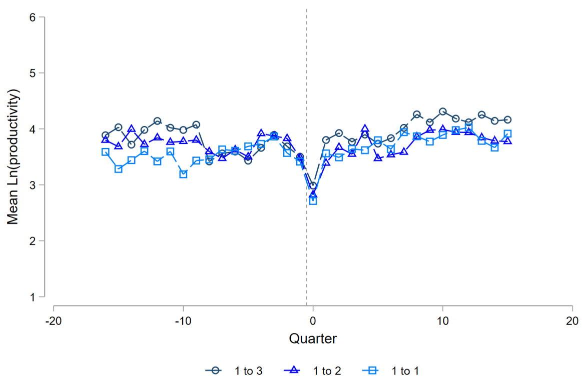
Figure D12: The distribution of productivity across judges and courts



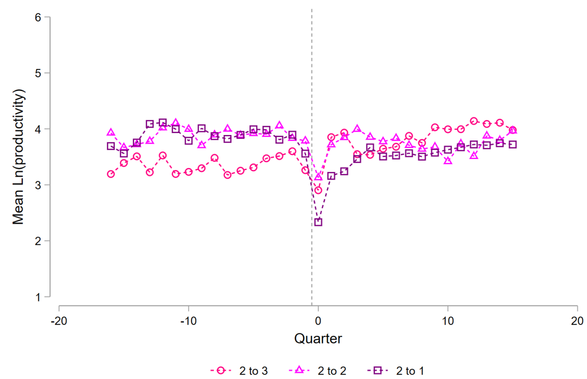
(a) Judges **(b)** Courts

Notes: Panel (a) shows the distribution of the number of proceedings (weighted by their complexity) completed by a judge in a quarter, averaged at the judge level. Panel (b) shows the distribution of the average number of proceedings (weighted by their complexity) completed by a judge in a quarter, averaged at the court level.

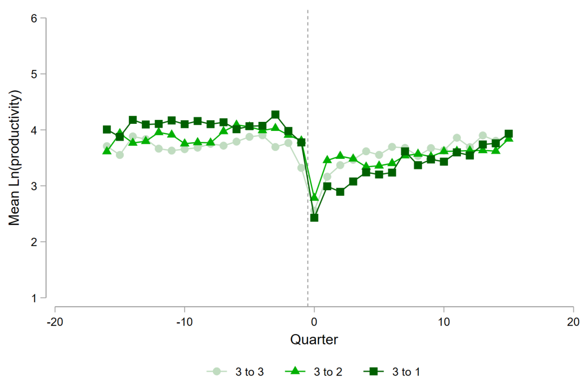
Figure D13: Average log productivity for judges experiencing a long-term change in court, by tercile of origin and destination court FE



(a) 1^{st} tercile of origin



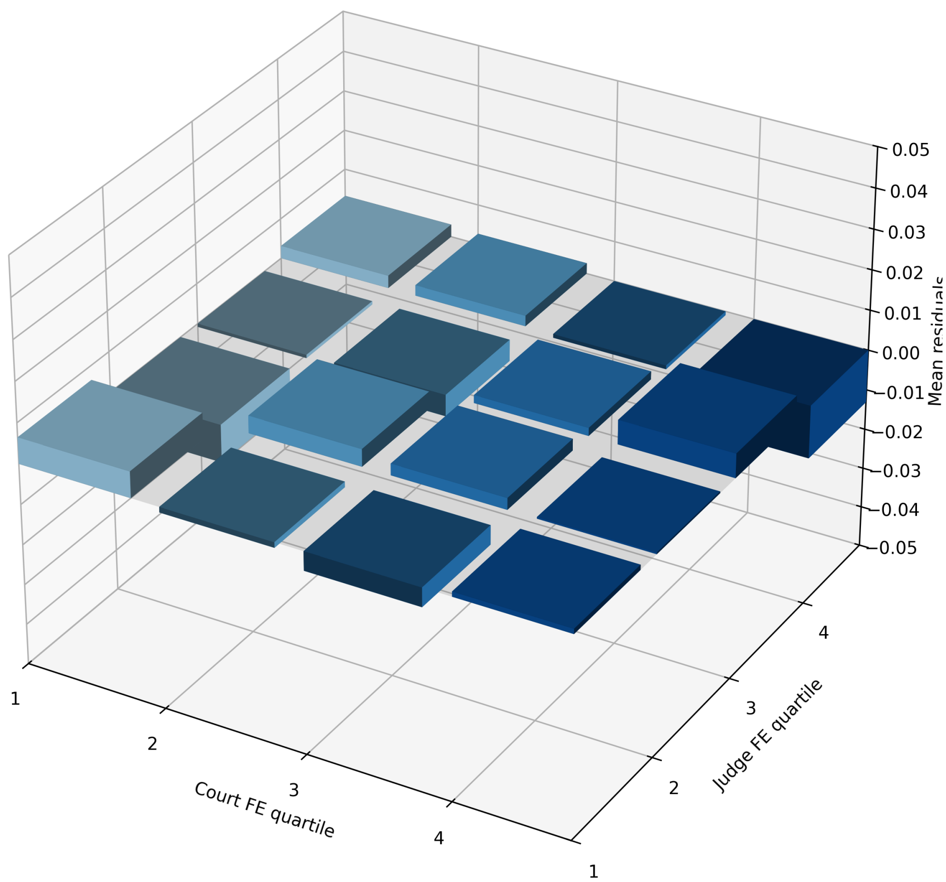
(b) 2^{nd} tercile of origin



(c) 3^{rd} tercile of origin

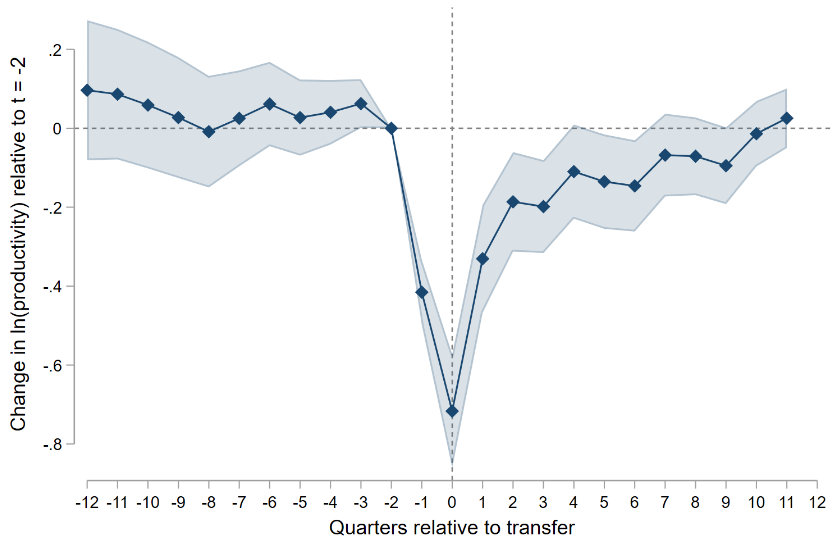
Notes: This figure plots the average log productivity of judges two years before and after a long-term change in court. A long-term change is defined as a transfer in which a judge spends at least one year in both the outgoing and incoming courts. The figure plots six types of long-term changes, by terciles of origin and destination fixed effects. The horizontal axis reports the distance in quarters relative to the long-term change in court.

Figure D14: Mean residuals, by quartiles of judge and court effects.



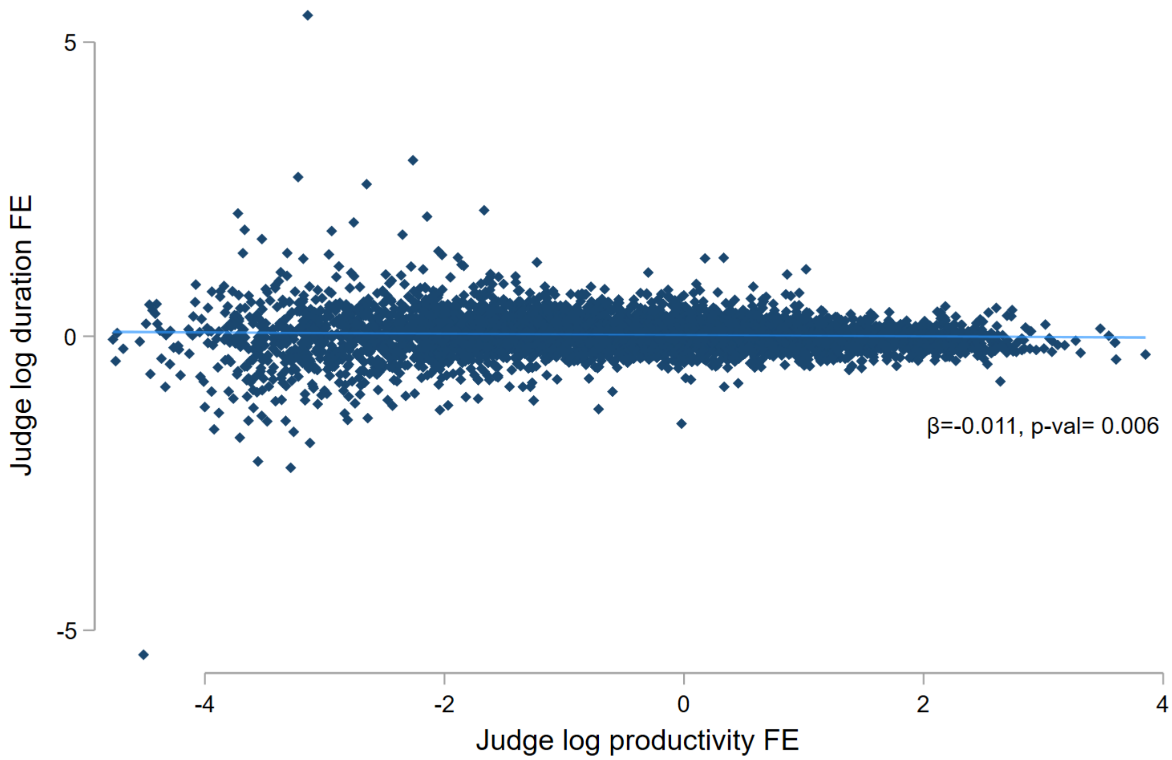
Notes: This figure shows mean residuals from model (2) with cells defined by the combination of quartiles of estimated judge effects and quartiles of estimated court effects. The sample is restricted to judges that completed at least 5 proceedings in a quarter.

Figure D15: The productivity costs of transfers, *unweighted productivity*



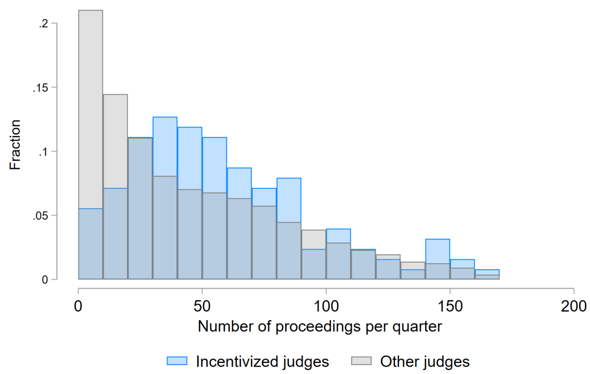
Notes: The figure reports the estimated coefficients δ_{T+s} from Equation (5), using the logarithm of unweighted quarterly productivity as the dependent variable. The estimation sample is restricted to the three years before and after a transfer to another court. The second quarter before the transfer serves as the omitted reference period. Standard errors are clustered at the judge level, and shaded areas indicate 95-percent confidence intervals.

Figure D16: Scatterplot of log productivity and log duration judge fixed effects

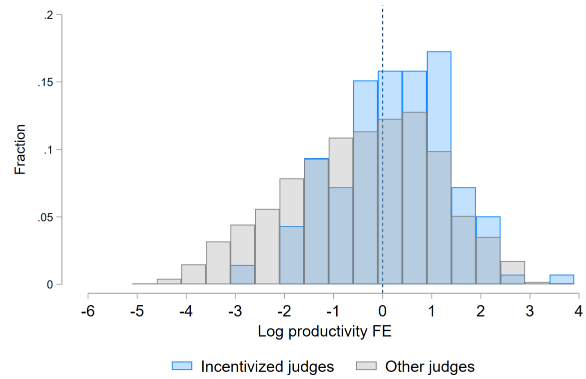


Notes: This scatterplot shows the judge fixed effects using log duration as the dependent variable and the judge fixed effects using log productivity as the dependent variable in Equation 2. Each dot is a judge. The line represents the linear fit and the shaded area its 95-percent confidence interval. β denotes the slope coefficient of the linear fit, and *p-val* reports its p-value.

Figure D17: Distribution of ex-ante productivity: incentivized and non-incentivized judges.

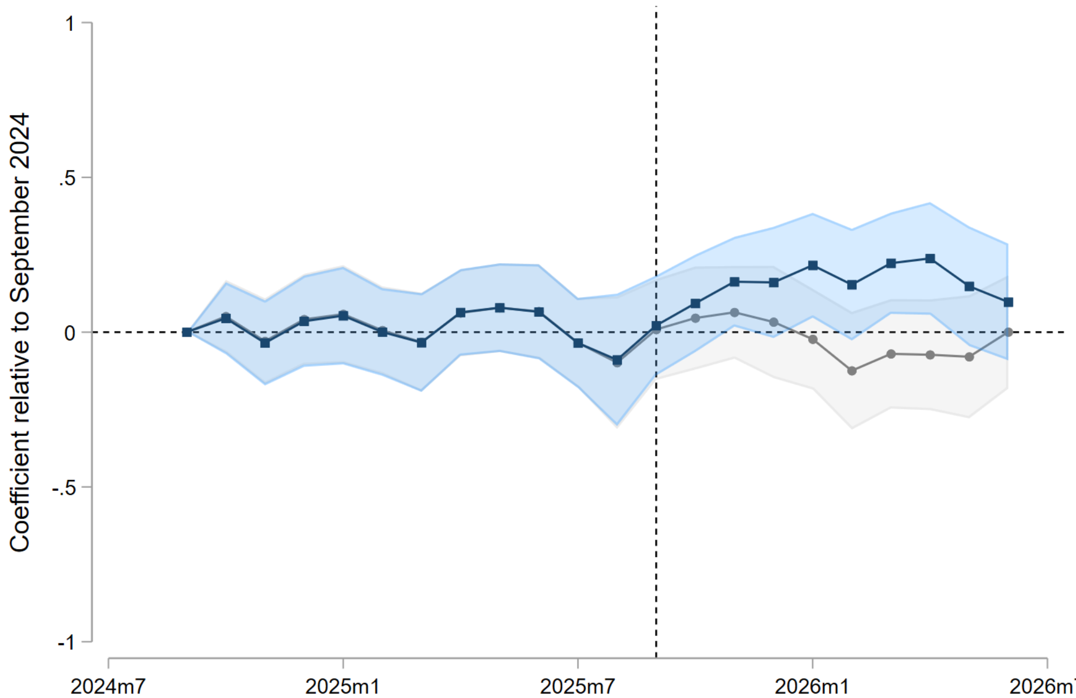


(a) Number of proceedings



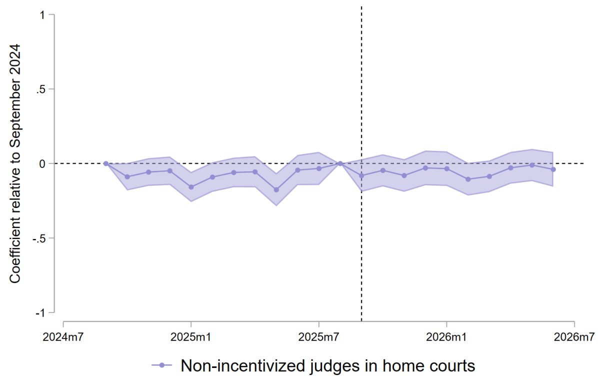
(b) Log productivity FE

Figure D18: The effects of remote work incentives on incentivized judges' total and in-person *unweighted* productivity.

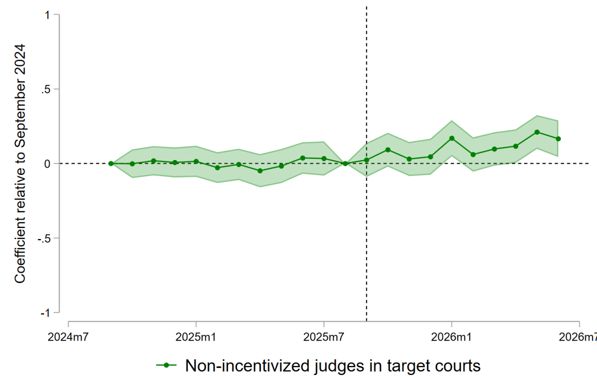


Notes: The figure plots the event-study coefficients β_{T+s} from Equation (8), with September 2024 as the omitted reference month. The blue line shows estimates for log total proceedings completed per month (in-person and remote), while the gray line shows estimates for log in-person proceedings only. Shaded areas denote 95-percent confidence intervals.

Figure D19: Spillover effects on non-incentivized judges



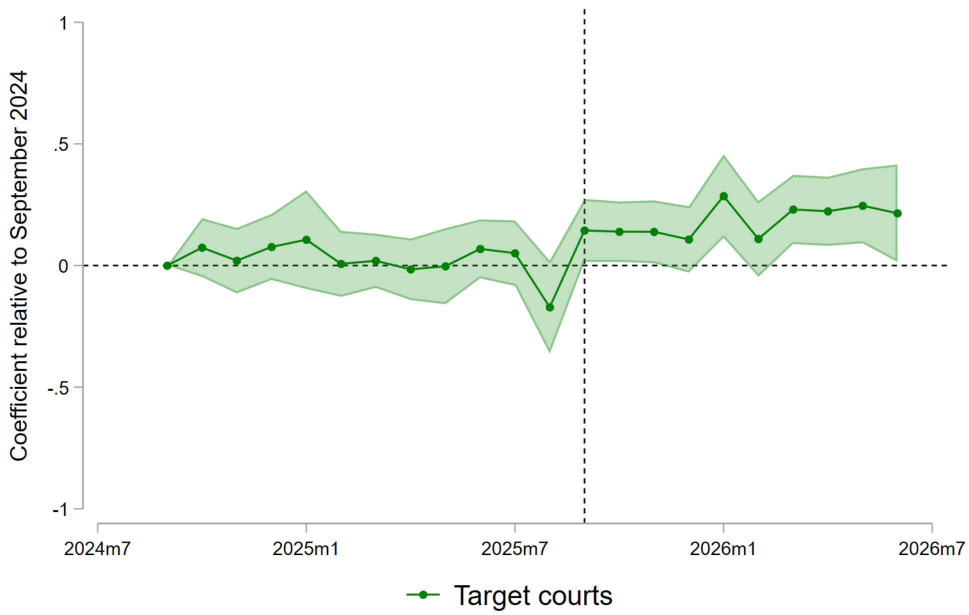
(a) Home courts



(b) Target courts

Notes: The figures plot the event-study coefficients β_{T+s} from Equation (8), replacing the treatment group with non-incentivized judges in home courts (panel a) and target courts (panel b) using log-complexity weighted productivity as the dependent variable with September 2024 as the omitted reference month. The control group consists of non-incentivized judges in control courts. All other judges (including incentivized judges) are dropped from the sample. The solid lines show estimates for log complexity-weighted proceedings completed per month. Shaded areas denote 95-percent confidence intervals.

Figure D20: The effects of remote work incentives on log monthly *unweighted* proceedings completed by target courts.



Notes: The figure plots the event study coefficients, γ_{T+s}, from Equation (10), using July 2025 as the omitted reference month. The green line reports the point estimates for the log of unweighted total proceedings completed per month by judges working for the court, including both judges working on site and those working remotely from other courts. The shaded area represents 95-percent confidence intervals.

B Additional tables

Table C1: Allocation of remote-work judges to target courts

(1) Ranking	(2) Court	(3) DT	(4) Variation DT	(5) Planned RW judges	(6) Applicant RW judges	(7) In-sample RW judges
1	Trieste	1303	63%	16	.	3
2	Venezia	1290	147%	66	.	16
3	L'Aquila	1173	118%	12	.	2
4	Vallo Della Lucania	1103	-18%	3	.	2
5	Isernia	1003	31%	4	.	1
6	Cagliari	918	16%	21	.	7
7	Enna	756	-10%	4	.	1
8	Lanusei	726	-4%	1	.	1
9	Lagonegro	720	-29%	2	.	1
10	Catanzaro	701	-16%	10	.	2
11	Potenza	685	-22%	7	.	1
12	Gela	649	11%	5	.	3
13	Velletri	635	15%	23	.	6
14	Termini Imerese	634	-16%	7	.	1
15	Oristano	626	-1%	3	.	1
16	Nola	613	-20%	13	.	4
17	Lamezia Terme	611	-36%	1	.	1
18	Brindisi	609	-14%	9	.	3
19	Belluno	606	61%	2	.	0
20	Paola	578	-31%	1	.	1
21	Siracusa	557	-15%	8	.	2
22	Bologna	553	4%	24	.	1
23	Cassino	549	-29%	3	.	0
24	Lecce	548	-2%	32	.	9
25	Perugia	545	-19%	4	.	1
26	Latina	539	-14%	9	.	5
27	Brescia	535	-1%	20	.	6
28	Avezzano	519	4%	4	.	0
29	Nuoro	518	-16%	1	.	0
30	Firenze	513	-3%	19	.	10
31	Teramo	508	-31%	2	.	0
32	Urbino	501	8%	1	.	4
33	Civitavecchia	500	-19%	5	.	4
34	Matera	499	-28%	1	.	1
35	Caltanissetta	499	-1%	5	.	1
36	Bari	498	-16%	27	.	5
37	Salerno	487	-36%	3	.	1
38	Pisa	486	-28%	2	.	0
39	Forli	480	-13%	2	.	0
40	Genova	471	13%	21	.	9
41	Locri	470	-20%	5	.	2
42	Massa	467	-5%	3	.	1
43	Trento	467	0%	3	.	2
44	Sciacca	466	-4%	3	.	0
45	Avellino	465	-24%	5	.	3
46	Agrigento	465	-17%	6	.	1
47	Foggia	462	-32%	5	.	0
48	Napoli	461	-22%	67	.	14
	Total			500	212	139

Notes: This table reports the list of target courts for remote-work judges and the allocation of remote positions under the incentive scheme. The ranking reflects the severity of case backlogs, based on disposition times as of June 2025 and their variation relative to 2019 levels. DT indicates the court disposition time (in days) on June 30, 2025. *Variation* DT indicates the percentage change in disposition time from 2019 to 2025, which should have reached -40% by June 2026 to reach the PNRR target. Column (5) indicates the number of judges who could potentially work remotely for each court under the scheme (Consiglio Superiore della Magistratura, 2025). Column (6) indicates the number of judges that applied for the incentive scheme (data by target court is not publicly available). Column (7) indicates the number of incentivized judges that I can identify in my sample for each target court.

Table C2: Number of judges identified as incentivized: actual and placebo periods

Incentive period	**Judges identified as incentivized**
September 2025-June 2026	139
Placebo periods	**False positive**
September 2024-June 2025	1
September 2023-June 2024	2
September 2022-June 2023	4
September 2021-June 2022	3
September 2020-June 2021	5
September 2019-June 2020	10
September 2018-June 2019	7
September 2017-June 2018	3
Average	4.38

Notes: In each year t, judges are classified as incentivized if they: complete proceedings only for a non-target court from September of year t-1 to August of year t; complete proceedings for both a home and a target court from September of year t to June of year t+1; and do not complete any proceedings for any target court from July to December of year t+1.

Table C3: Judge characteristics

	(1) Full sample	(2) Movers	(3) Incentivized
A. Demographics			
Female	0.58	0.57	0.51
Age	48.98	46.28	44.37
B. Macroareas of birth			
South	0.40	0.41	0.44
Islands	0.13	0.12	0.13
Center	0.19	0.20	0.15
North-East	0.11	0.10	0.10
North-West	0.16	0.17	0.17
Abroad	0.01	0.00	0.01
C. Number of judges			
With observed characteristics	3573	1195	115
Total	6242	1463	139

Note: The table reports the averages statistics of judge characteristics. The statistics are computed over: the full sample of judges in column (1); the subsample of movers in column (2); the subsample of incentivized judges in column (3). Movers are the judges that work in at least two courts over my sample period. Age is measured in years, all remaining variables in panels A and B are shares.

Table C4: Court characteristics

A. Labor	
Civil judges	18.52
Court assistants	16.92
Court clerks	74.42
Court officers	6.25
N	959
B. Capital	
Court offices ('000 m^2)	12.78
Court archives or libraries ('000 m^2)	5.29
Courtrooms ('000 m^2)	2.70
N	1370
C. Technology	
Digital filing rate (acts)	0.44
Digital filing rate (judgments)	0.52
N	700
D. Demand and output	
Specialization (HHI by matter)	0.09
Incoming proceedings ('000)	10.10
Pending proceedings ('000)	10.08
Resolved proceedings ('000)	10.57
Resolved proceedings with judgment ('000)	2.96
N	1400

Note: The table reports the averages of court characteristics. The level of observation is the court-year pair. Data on labor is available for 137 active courts from 2017 to 2023, excluding three courts in Trentino Alto Adige. Data on capital is a cross-section available only for the 137 active courts in 2021, excluding three courts in Trentino Alto Adige. Data on technology is available for the 140 active courts in 2016-2020. Data on demand and output is available for the 140 active courts from 2016 to 2024. Panel B shows surcace areas, which are measured in thousands of square meters. *Specialization* is the sum of the squares of the subject-matter shares of the backlog proceedings in each court-year. The digital filing rates are the ratio between acts (judgments) that are digitally filed over total acts (judgments). Data on proceedings are expressed as counts in thousands.

Table C5: Productivity cost of transfers

	Ln(unweighted proceedings)	Ln(weighted proceedings)
Transfer - 12	0.0965	-0.00645
	(1.07)	(-0.07)
Transfer - 11	0.0863	-0.00633
	(1.02)	(-0.07)
Transfer - 10	0.0588	-0.0341
	(0.72)	(-0.39)
Transfer - 9	0.0271	-0.0632
	(0.35)	(-0.77)
Transfer - 8	-0.00863	-0.118
	(-0.12)	(-1.55)
Transfer - 7	0.0252	-0.0525
	(0.41)	(-0.79)
Transfer - 6	0.0614	-0.0250
	(1.13)	(-0.44)
Transfer - 5	0.0271	-0.0196
	(0.55)	(-0.39)
Transfer - 4	0.0406	0.00774
	(0.97)	(0.17)
Transfer - 3	0.0625*	0.0393
	(1.98)	(1.20)
Transfer - 1	-0.415***	-0.389***
	(-9.92)	(-8.88)
Transfer	-0.717***	-0.898***
	(-9.80)	(-11.62)
Transfer + 1	-0.331***	-0.451***
	(-4.74)	(-6.17)
Transfer + 2	-0.186**	-0.251***
	(-2.90)	(-3.64)
Transfer + 3	-0.198**	-0.233***
	(-3.30)	(-3.71)
Transfer + 4	-0.110	-0.173**
	(-1.81)	(-2.66)
Transfer + 5	-0.135*	-0.165**
	(-2.22)	(-2.68)
Transfer + 6	-0.146*	-0.184**
	(-2.48)	(-3.09)
Transfer + 7	-0.0679	-0.0782
	(-1.27)	(-1.47)
Transfer + 8	-0.0710	-0.0913
	(-1.41)	(-1.80)
Transfer + 9	-0.0949	-0.103*
	(-1.91)	(-2.00)
Transfer + 10	-0.0138	-0.0191
	(-0.33)	(-0.42)
Transfer + 11	0.0254	0.00488
	(0.66)	(0.11)
Constant	3.859***	3.901***
	(115.19)	(109.80)
N	9714	9714
R-squared	0.594	0.571
Judge FE	YES	YES
Time FE	YES	YES
Court FE	YES	YES

Notes: This table reports changes in the logarithm of quarterly judicial productivity from three years before to three years after a transfer to another court. The second quarter before the transfer is omitted. Standard errors are clustered at the judge level.

Table C6: Judge productivity and reversals on appeal

	Reversal	Reversal, conditional on appeal	Reversal, conditional on appeal
	(1)	(2)	(3)
Log productivity FE	-0.003	-0.003	-0.007
	(0.007)	(0.007)	(0.005)
N	29959	29959	29950
Mean	0.484	0.484	0.484
R-squared	0.071	0.071	0.191
Time FE	YES	YES	YES
Type FE	YES	YES	YES
Court FE	YES	YES	YES
Appeal judge FE	NO	NO	YES

Notes: This table reports estimates from regressions of an indicator for whether a first instance decision is reversed on appeal on judge productivity, measured by the log productivity fixed effects estimated in Equation (2). Column (1) considers all first instance decisions, regardless of whether they are appealed. Columns (2) and (3) restrict the sample to decisions that are appealed. All specifications include fixed effects for decision month, case type (keyword), and first instance court. Column (3) additionally includes fixed effects for the president presiding over the appeal. Regressions are at the proceeding level. Standard errors are clustered at the judge level. The sample is restricted to courts matched with corresponding courts of appeal.

Table C7: Analysis of variance of log duration of proceedings

	(1)	(2)	(3)	(4)	(5)	(6)
N	9491104	9491104	9491104	9491104	9491104	9491104
R-squared	0.064	0.636	0.651	0.671	0.672	0.673
Adjusted R-squared	0.064	0.635	0.651	0.671	0.672	0.673
P-value courts			0.000		0.000	
P-value judges				0.000	0.000	
Time FE	YES	YES	YES	YES	YES	YES
Type FE	NO	YES	YES	YES	YES	YES
Court FE	NO	NO	YES	NO	YES	NO
Judge FE	NO	NO	NO	YES	YES	NO
Judge-by-Court FE	NO	NO	NO	NO	NO	YES

Notes: This table analyzes how much of the variance in log duration is explained by the judge, court, type, and time components in sample of proceedings. Regressions are at the proceeding level. N represents the number of judgments. The p-value tests the null hypothesis that judge effects and/or court effects are jointly zero.

Table C8: Variance-covariance decomposition of log duration

	(1) (Component)	(2) (Share)
Var($Ln(D_{ijt})$)	1.1289	100.00 %
Var(Judge)	0.0295	2.62 %
Var(Court)	0.0294	2.60 %
Var(Month-keyword)	0.7250	64.22 %
Var(Residual)	0.3083	27.31 %
Cov(Judge, Court)	-0.0077	-1.36 %
Cov(Judge, Month-keyword)	0.0068	1.20 %
Cov(Court, Month-keyword)	0.0067	1.20 %
N	9491104	

Notes: This table reports the variance-covariance decomposition of log duration in the largest connected set. The model includes judge, court, and month-by-keyword fixed effects. Covariances are multiplied by 2 in the shares.

Table C9: Judge productivity and case duration

	Log duration (1)	Log duration FE (2)
Log productivity FE	-0.035***	-0.011***
	(0.004)	(0.004)
N	9457112	5742
Mean	5.859	0.025
R-squared	0.652	0.003
Time FE	YES	NO
Type FE	YES	NO
Court FE	YES	NO

Notes: This table examines the association between judge productivity and case duration. Column (1) reports estimates from a regression of the log number of days between proceeding registration and the decision date on judge productivity, measured by the log productivity fixed effects estimated in Equation (2). Column (2) uses the log duration fixed effects estimated in Equation (7) as the dependent variable. The regression in column (1) is estimated at the proceeding level and controls for time, type and court fixed effects, while the regression in column (2) is estimated at the judge level. Standard errors are clustered at the judge level.

Table C10: The effect of remote-work incentives on the productivity of incentivized judges (unweighted)

	Proceedings (1)	Ln(Proceedings) (2)	In-person proceedings (3)	Ln(in-person proceedings) (4)
Post*Incentivized	3.708**	0.164***	-2.859*	-0.029
judge	(1.644)	(0.055)	(1.640)	(0.060)
N	399842	399842	399842	399147
Mean	23.623	2.505	23.578	2.504
R-squared	0.540	0.543	0.540	0.543
Time FE	YES	YES	YES	YES
Judge FE	YES	YES	YES	YES
Court FE	YES	YES	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the effect of the incentives on the productivity of judges participating in the incentive scheme. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2); the number of proceedings completed in-personly (column 3); the logarithm of the number of proceedings completed in-personly (column 4). All proceedings are weighted by their complexity. All specifications include fixed effects for decision month, home court and judge fixed effect. In all regressions we control for the interaction between each one of the twelve months and the indicator for incentivized judges. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme.

Table C11: Spillover effects of remote-work incentives on the productivity of non-incentivized judges

	Proceedings (1)	Ln(Proceedings) (2)
Post*target court	6.129***	0.175***
	(1.880)	(0.043)
Post*home court	1.052	-0.017
	(0.733)	(0.041)
N	383498	383498
Mean	23.174	2.533
R-squared	0.412	0.448
Time FE	YES	YES
Judge FE	YES	YES
Court FE	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the spillover effects of the incentives on the productivity of non-incentivized judges in target and home courts. Judges in control courts are the control group. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2). All proceedings are weighted by their complexity. All specifications include fixed effects for decision month, judge, and first instance court. In all regressions we control for the interaction between each one of the twelve months and the indicator for target courts and for home courts. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme.

Table C12: Spillover effects of remote-work incentives on the productivity of non-incentivized judges, unweighted

	Proceedings (1)	Ln(Proceedings) (2)
Post*target court	2.659***	0.082**
	(1.018)	(0.041)
Post*home court	-0.127	-0.049
	(0.868)	(0.040)
N	383498	383498
Mean	23.419	2.494
R-squared	0.537	0.545
Time FE	YES	YES
Judge FE	YES	YES
Court FE	YES	YES

Notes: his table shows the estimate of a difference-in-difference estimate of the spillover effects of the incentives on the productivity of non-incentivized judges in target and home courts. Judges in control courts are the control group. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2). All specifications include fixed effects for decision month, judge, and first instance court. In all regressions we control for the interaction between each one of the twelve months and the indicator for target courts and for home courts. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme.

Table C13: The effect of remote-work incentives on the productivity of incentivized judges, using only control-court judges as the control group

	Proceedings (1)	Ln(Proceedings) (2)	In-person proceedings (3)	Ln(in-person proceedings) (4)
Post*Incentivized judge	9.182***	0.285***	1.201	0.081
	(2.085)	(0.072)	(1.824)	(0.072)
N	75247	75247	75247	74551
Mean	20.419	2.436	20.163	2.429
R-squared	0.548	0.439	0.551	0.442
Time FE	YES	YES	YES	YES
Judge FE	YES	YES	YES	YES
Court FE	YES	YES	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the effect of the incentives on the productivity of judges participating in the incentive scheme. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2); the number of proceedings completed in-personly (column 3); the logarithm of the number of proceedings completed in-personly (column 4). All proceedings are weighted by their complexity. All specifications include fixed effects for decision month, home court and judge fixed effect. In all regressions we control for the interaction between each one of the twelve months and the indicator for incentivized judges. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme. We drop home-court judges and use only control-court judges as the control group.

Table C14: The effect of remote-work incentives on the productivity of incentivized judges (unweighted), using only control-court judges as the control group

	Proceedings (1)	Ln(Proceedings) (2)	In-person proceedings (3)	Ln(in-person proceedings) (4)
Post*Incentivized judge	4.372**	0.145**	0.189	0.019
	(1.957)	(0.066)	(1.816)	(0.064)
N	75247	75247	75247	74551
Mean	22.106	2.478	21.863	2.473
R-squared	0.595	0.518	0.598	0.522
Time FE	YES	YES	YES	YES
Judge FE	YES	YES	YES	YES
Court FE	YES	YES	YES	YES

Notes: This table shows the estimate of a difference-in-difference estimate of the effect of the incentives on the productivity of judges participating in the incentive scheme. The dependent variables are: the number of completed proceedings (column 1); the logarithm of the number of completed proceedings (column 2); the number of proceedings completed in-personly (column 3); the logarithm of the number of proceedings completed in-personly (column 4). All proceedings are weighted by their complexity. All specifications include fixed effects for decision month, home court and judge fixed effect. In all regressions we control for the interaction between each one of the twelve months and the indicator for incentivized judges. Standard errors are clustered at the judge level. Mean is the average of the dependent variable before the beginning of the incentive scheme. We drop home-court judges and use only control-court judges as the control group.