Remote Work Incentives and State Capacity

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Abstract

Leveraging data on 9 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. The incentive scheme aimed to reduce case backlogs in target courts with the longest disposition times by incentivizing judges in other courts to adjudicate cases remotely for them. Exploiting the rotation of judges across courts, I estimate that judges account for 60 percent of the variation in judicial productivity, compared with 11 percent explained by courts. More productive judges also deliver higher-quality decisions, as measured by lower reversal rates on appeal, yet received no higher compensation prior to the incentive scheme. The remote work incentive increased judges' completed proceedings by 20 percent and their complexity-weighted productivity by 37 percent. In turn, remote adjudication increased productivity in target courts by 29 percent and reduced their backlog and disposition times by 4 and 5 percent within nine months. These findings show that remote work can reduce spatial inequality in state capacity without requiring costly worker relocation.

1 Introduction

State capacity is a key determinant of economic growth but remains highly uneven within countries, reflecting past investments in legal and fiscal institutions (Besley and Persson, 2009). Reallocating public resources toward lagging areas is often constrained by the costs of worker relocation: compensation incentives to employees, parallel staffing, facility investment (Schmutz and Sidibé, 2019; Freitas, 2025). 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 spatial inequality in state capacity by allowing governments to reallocate public-sector work towards disadvantaged areas without requiring costly worker relocation (Bloom et al., 2015; Barrero et al., 2023).

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I thank Antonio Accetturo, Mario Cannella, Federico Fornasari, Sauro Mocetti, Lucia Rizzica, Giacomo Rodano, Giacomo Roma, Viola Salvestrini, and seminar participants at the Bank of Italy for their valuable comments. The views expressed in this paper are those of the author and do not necessarily reflect those of the Bank of Italy.

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 *home courts* to adjudicate cases remotely for *target courts* facing the most severe backlogs. By decoupling the location of service provision from the location of workers, the reform allowed judicial capacity to be reallocated toward lagging jurisdictions without relocating judges.

I web-scraped data on 9 million civil proceedings in Italy between 2016 and 2026, containing information on judges, courts, case characteristics, filing and disposition dates, and appellate outcomes from an online platform provided by the Ministry of Justice (Ministero della Giustizia, 2026a).[1] I combine an AKM decomposition of judicial productivity with an event-study analysis of a remote work incentive scheme introduced between September 2025 and June 2026 (Abowd et al., 1999).

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.

Judges' productivity declines sharply in the quarter following a transfer to another court and gradually recovers over time. This productivity dip is observed even when judges move from the least to the most productive courts. This finding provides new evidence that transfers of public-sector workers impose not only monetary costs but also productivity costs on governments. More broadly, they suggest that remote work may help reallocate state capacity without the performance losses associated with worker relocation.

Second, I investigate the determinants of first-instance judges' productivity. The Ministry of Justice provided data on court characteristics, while I collected information on judges' characteristics and reconstructed their compensation histories from Ministry of Justice bulletins between 2007 and 2025 (Ministero della Giustizia, 2026b).[2] Regressing judge fixed

[1] To link first-instance proceedings to their corresponding appeals, I submitted freedom of information requests to all courts of appeal. 17 of the 29 courts kindly provided the requested data.

[2] I was able to retrieve information for 58 percent of the judges in my sample, accounting for 76 percent

effects in log productivity on judge and court characteristics, I find that the factors most strongly associated with productivity are judicial specialization and court backlog. The positive correlation between backlog and productivity explains why target courts, despite facing the largest backlogs, have been recently more productive than average. This 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.

Prior to the introduction of the incentive scheme, I find no significant correlation between productivity and judges' 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). Consistent with evidence from other public-sector settings (Fenizia, 2022; Deserranno et al., 2025), I find no evidence of a quantity–quality trade-off: more productive judges are no more likely to have their decisions reversed on appeal and, if anything, exhibit slightly lower reversal rates.

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 their 2019 level (Presidenza del Consiglio dei Ministri, 2025).[3] The program targeted 48 courts with the longest disposition times and was designed to involve up to 500 judges drawn from a pool of 2,609 eligible candidates, although only 212 ultimately participated (Consiglio Superiore della Magistratura, 2025; Negri, 2025). Participating 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 undertake a second batch of

of all proceedings.

[3]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.

50 cases and double the reward. 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 130 incentivized first-instance judges. The remaining 82 participants belonged to courts outside my sample, such as courts of appeal and juvenile courts, or were serving on secondment.

Judges who participated in the incentive scheme were positively selected: the median participant was ex-ante more productive than 67 percent of all other judges. Despite this positive selection, productivity trends remained parallel between incentivized and non-incentivized judges until 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 5 cases from a baseline of 24, implying a 20 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, concentrated in the final months of the program. This reduction was therefore more than offset by an average of 8 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 37 percent, indicating that the cases assigned by target courts were more complex than average. Using non-target courts without incentivized judges as control courts, I find no evidence of 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 20 percent and complexity-weighted productivity by 29 percent relative to control courts. The judicial capacity provided by remote judges reduced backlog in target courts by 4 percent and disposition time by 5 percent within nine months. To the best of my knowledge, these findings provide the first causal evidence that remote work incentives can reduce spatial disparities in state capacity.

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). A

related literature 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 remote work incentive scheme, I show that performance incentives are taken up by workers with higher ex-ante productivity, whether reflecting greater skills or stronger intrinsic motivation. At the same time, incentives induce a partial reallocation of effort away from unrewarded tasks, highlighting both the selection and crowding-out effects of performance pay.

The paper also contributes to the literature on remote work. Existing evidence focuses primarily on the effects of remote work on worker productivity, labor supply, job satisfaction, promotions, turnover, and absenteeism (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). My results suggest that remote work can help organizations overcome geographic mismatches between the location of workers and the location of demand, provided that institutions create incentives for workers to direct effort toward locations where it is most needed.[4]

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.

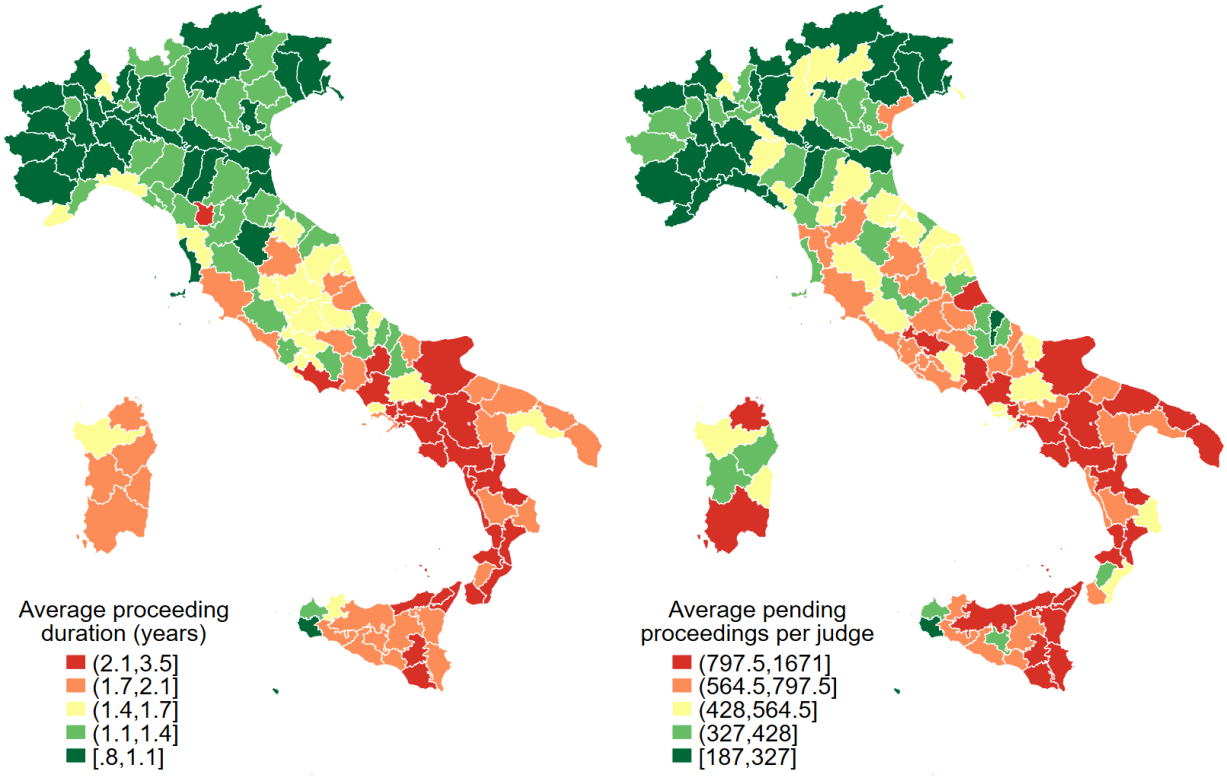
The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the remote work incentive scheme. Section 3 presents the data and Section 4 examines the components and determinants of judicial productivity prior to the introduction of the incentive scheme. Section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

[4]This paper also contributes to the literature on policies aimed at improving judicial performance. Existing work has examined 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% of GDP (Figure D1), Italy has the second slowest civil justice system in Europe (Figure D2).[5] 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%.

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.

[5]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).

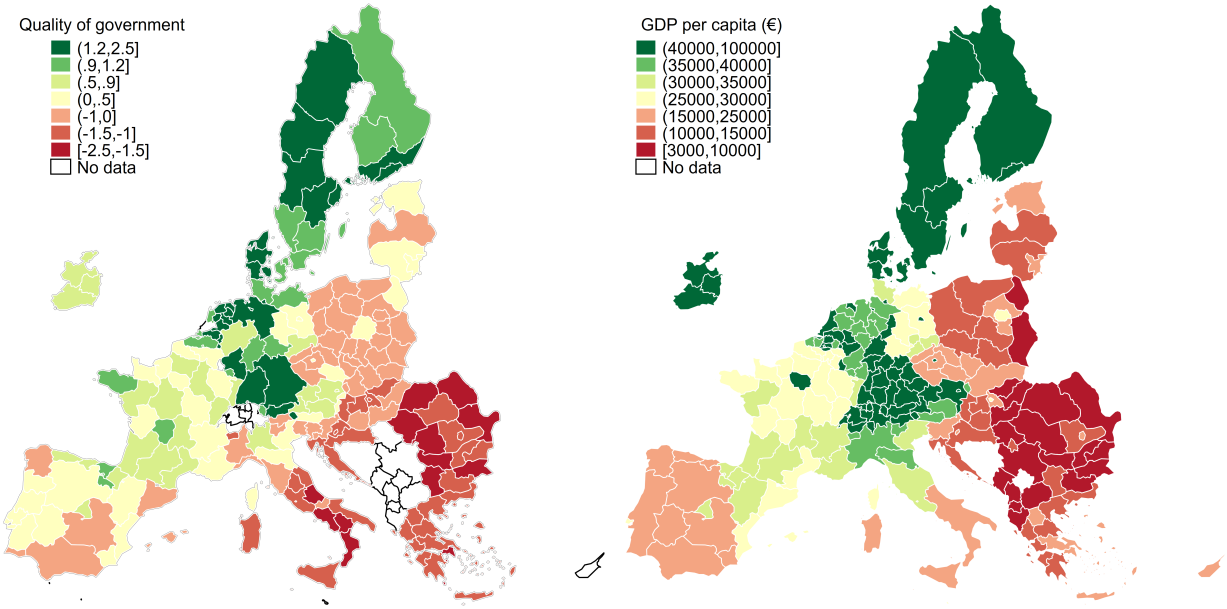
The average slowness of the Italian justice system hides large spatial disparities in judicial capacity. Figure 1a shows that first-instance proceedings take longer in southern courts, with an average duration of 2.1 years compared to 1.3 years in the rest of Italy (+62 percent). Focusing on judgments, the average time rises to 3.9 years in the South, relative to 2.8 years elsewhere (+39 percent; Figure D6).[6] Figure 1b shows that southern courts also face larger backlogs, averaging 1235 pending proceedings per judge compared to 855 in the rest of Italy (+44 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). 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 is characterized by longer completion times for public procurement (Baltrunaite et al., 2021), lower quality public transportation (Mocetti and Roma, 2021), slower emergency and rescue services (Miglino and Smurra, 2025), lower levels of digitalization among local public administrations (Ciapanna et al., 2025). Since reunification, East Germany has had weaker fiscal-administrative capacity than West Germany, with partial convergence facilitated by the secondment of Western civil servants (Dwenger and Gumpert, 2025). In the US, judicial performance varies substantially across state supreme courts (Ash and MacLeod, 2021). Postal delivery errors and bureaucratic productivity was largely heterogeneous across U.S. cities during the Gilded Age (Aneja and Xu, 2024).

Figure 2 reveals pronounced within-country heterogeneity in state capacity across European Union regions. These differences closely mirror regional economic disparities. High-income regions, such as Northern Italy, Northern Spain and Madrid, and the former West Germany, exhibit higher scores on the European Quality of Government Index than lower-income regions such as Southern Italy, Southern Spain, and the former East Germany. Consistent with this pattern, the correlation between regional GDP per capita and the Quality of Government Index is 0.68. Overall, state capacity appears to be weaker in historically disadvantaged regions, potentially exacerbating existing spatial economic inequalities.

[6]Proceedings are of three types: judgments (*sentenze*), orders (*ordinanze*), and decrees (*decreti*).

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) displays GDP per capita (in €) across European NUTS2 regions in 2017 (Eurostat, 2026a,b).

2.1 Remote work incentives

As part of 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). The PNRR target was a 40 percent reduction by 30 June 2026 in the average disposition time compared with 2019. The High Council of the Judiciary identified 48 target courts most in need of support and established the maximum number of judges to be assigned remotely to each, based on two criteria: progress toward the PNRR objectives and the average case disposition time.

The overall program was capped at 500 volunteer judges. 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. 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 proceedings 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.[7] 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.

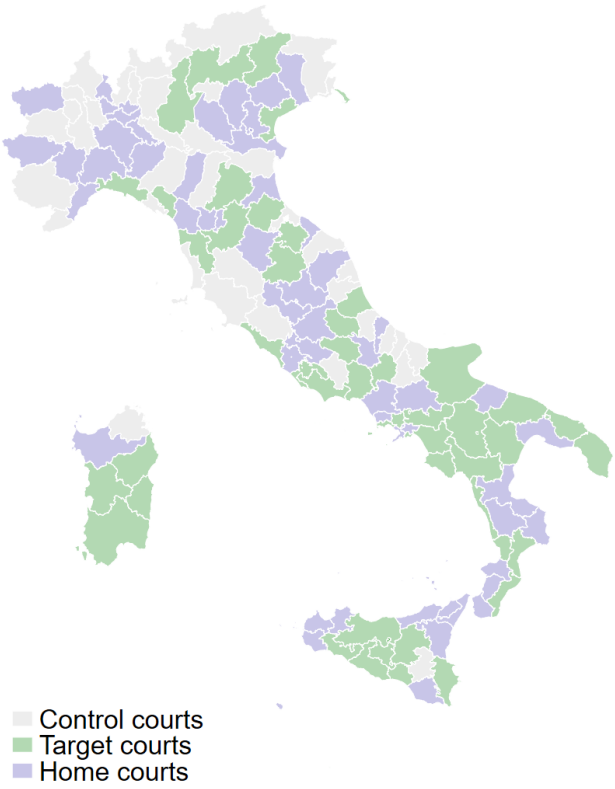
Completion of the 50 assigned cases entitled the judge to a lump sum payment equal to three times the monthly salary of an ordinary judge with three years of seniority. This corresponded to a net incentive of about 9,515 euros (190 euros per proceeding).[8] In addition, the judge received 0.16 seniority 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, doubling or even tripling the total incentive payment. Lump sum payments and seniority points were awarded only upon completion of the *entire* assigned caseload of 50 or 100 cases.

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). Only 212 applications were received but the judge allocations remained proportional to the distribution originally planned (Negri, 2025).

[7] 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.

[8] 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 bracket (43 percent).

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 130 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 130 incentivized first-instance judges. The remaining 82 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 of the incentivized judges. Courts that are neither target courts nor home courts of any incentivized judge are defined as control courts. 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.[9]

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.[10] The dataset covers all civil subject matters, except for family law, minors, and personal status (D'Alessandro et al., 2024; Ministero della Giustizia, 2026a).[11] This allowed me to construct a dataset at first-instance proceeding level that links each first-instance ruling to the corresponding judge and court, enabling the tracking of judges across Italian first-instance civil 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 key words. The key word categories are very fine (e.g. insolvency proceedings, eviction for non-payment of rent contract, individual dismissals for just cause) and include 874 different classifications, a more granular specification than subject matters (71 different classifications). Accounting for the type of proceeding is essential to avoid conflating differences in judges' productivity with differences in the variety of cases judges adjudicate. Figure D10 shows that the data downloaded from the *Banca Dati di Merito* closely replicates 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 assess the quality of first-instance judgments, I linked them with the corresponding appeal through FOIA requests to all Italian Courts of Appeal. 17 of the 29 courts kindly provided a unique identifier (*ruolo–numero di registro*) for the universe of appeal proceedings initiated between

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

[10] 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.

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

2016 and 2025 and linked each appeal to the corresponding first-instance proceeding via its unique identifier. To recover appellate outcomes, I web-scraped decisions from the Courts of Appeal from *Banca Dati di Merito* and classified whether the original judgment was upheld or overturned.[12] 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 professional and honorary judges, as well as administrative staff (clerks, assistants, officers), 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, archives and libraries 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 proxy for the degree of court digitization (Cugno et al., 2022; Cannella et al., 2024).

Finally, I collected all official bulletins published by the Ministry of Justice between 2007 and 2025, from which I extracted the gender, date of birth, and place of birth of 58 percent of the judges in the sample, corresponding to 76 percent of the proceedings in the dataset (Ministero della Giustizia, 2025a). Figure D9 provides an example of the demographic information available for a judge 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, 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).

[12] So far, I have downloaded data for three courts of appeal: Ancona, Brescia and Bologna.

3.1 Summary statistics

Judge characteristics. Table C2 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 6,125 first-instance judges, of whom 1,438 (23 percent) are movers and 130 (2.1 percent) are incentivized.

The majority of judges (55 percent) are born in Southern Italy or the islands, 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 slightly 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 on average 4.5 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 58 percent of judges in the full sample, 82 percent of movers, and 83 percent of incentivized judges.

Table 1: Sample structure

	(1) Full sample	(2) Target courts	(3) Home courts	(4) Control courts
# Judges	6125	2666	3130	1129
# Incentivized judges	130	0	130	0
# Movers	1428	749	898	353
# 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	9317769	3207720	4897089	1212960
# judgments	2553573	949439	1321988	282146
# orders	1039070	381964	527282	129824
# decrees	5725126	1876317	3047819	800990
# Courts of Appeal	3	3	2	3
# Appeals	18335	4969	2696	10670
# Accepted appeals	8844	2301	1435	5108
First-instance duration (years)	1.64	1.92	1.56	1.22
Judgment duration	3.30	3.79	3.07	2.73
Order duration	1.97	2.16	1.96	1.45
Decree duration	0.84	0.93	0.84	0.66
Appeal duration	2.50	2.10	2.86	2.60

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.

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.[13] 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.

So far, I have web scraped data on 9.3 million first-instance proceedings completed between January 2016 and April 2026. Of these, 2.6 million resulted in judgments, 1 million in orders, and 5.7 million in decrees. I have also collected data on approximately 20,000

[13]Twenty-five courts were abolished between 2013 and 2014 (Mocetti et al., 2025). Proceedings initiated in these courts are assigned to the courts into which they were incorporated.

appeals from three Courts of Appeal (Ancona, Brescia, and Bologna). Slightly less than half of these appeals were accepted, either fully or partially, resulting in the reversal of the first-instance decision.

On average, first-instance proceedings last 1.6 years. Judgments are the most time-consuming, with an average duration of 3.3 years, followed by orders (2.0 years) and decrees (0.8 years). Target courts are those facing the greatest challenges in terms of backlog and case disposition. 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.

Productivity. Let type k denote a unique combination of proceeding form (judgment, order, or decree) and key words (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).[14] Also, all specifications include quarter or month fixed effects to absorb any variation in the number of working days across periods.

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

[14] 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.

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 considerably more across individual judges than across courts (Figures D13a–D13b). Judges at the 25th percentile of the individual-level distribution complete 14 proceedings per quarter, compared with 72 for judges at the 75th 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-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 effect specification at the judge-quarter level (Abowd et al., 1999):

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

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.[15] τ_t represents quarter fixed effects and accounts for seasonality and common shocks. 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 intro-

[15]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.

duced. Column 1 includes only time fixed effects and yields an adjusted R^2 of 0.01. 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 0.05), but rises substantially when judge fixed effects are added (to 0.48). Column 4 shows that introducing court fixed effects after controlling for judge fixed effects explains virtually 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.

Table 2: Analysis of variance of log productivity

	(1)	(2)	(3)	(4)	(5)
N	143850	143850	143850	143850	143850
R-squared	0.014	0.055	0.504	0.511	0.534
Adjusted R-squared	0.014	0.053	0.483	0.489	0.510
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. N represents the number of judgments. 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). The variance of log productivity can be decomposed as:

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

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).[16]

[16]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

Table 3: Variance-covariance decomposition of the number of proceedings completed per month (weighted)

	(1) (Component)	(2) (Share)
Var(P_{jt})	2.3776	100.00 %
Var(Judge)	1.4339	60.31 %
Var(Court)	0.2635	11.08 %
Var(Quarter)	0.0982	4.13 %
Var(Residual)	1.2106	50.92 %
Cov(Judge, Court)	-0.2703	-11.37 %
Cov(Judge, Quarter)	-0.0423	-1.78 %
Cov(Court, Quarter)	-0.0017	-0.07 %
N	143850	

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

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 the evidence in Fenizia (2022), I also observe a negative covariance between bureaucrat (judge) and office (court) effects. More productive judges tend to be assigned to less productive courts, while less productive judges are more likely to work in courts with higher average productivity.

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

the sample size (Andrews et al., 2008).

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.

Validity checks. 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 (γ_j) productivity. That is, more productive judges sorting into more productvie courts would not violate the identifying assumptions. Likewise, if more productive judges were systematically sent to southern or northern courts, this would not represent a threat to the identification strategy.

As shown in Card et al. (2013), the identifying assumptions would be violated in three main ways. First, if judges sort into courts based on their comparative advantage. Second, if faster judges are assigned to courts experiencing a trend in productivity over time, such that more productive judges disproportionately move to courts that are becoming less productive. Third, if judges are reallocated in response to transitory negative shocks affecting court performance, leading to endogenous mobility.

Endogenous mobility associated with court-specific trends in log intervention time might exaggerate the relevance of judges' time-invariant heterogeneity in explaining productivity. Following Card et al. (2013), I assume that the error term in Equation (2) is composed by three random effects:

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

$\eta_{jc(j,t)}$ is an idiosyncratic productivity premium or penalty 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.[17]

[17]The assumption 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.

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 ($\psi_{destination} - \psi_{origin}$). The average decrease in judges' log productivity for the first tercile is actually *larger* in magnitude than the productivity gain for the third tercile. This pattern 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 between judges and courts play a relatively minor role in this setting, validating the two-way fixed effect specification. 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 (falling) productivity were systematically assigned to higher- (lower-) 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 a decline in productivity prior to a court transfer across all three terciles, likely reflecting the reduction in case completion associated with the handover of pending cases before a judge leaves a court.

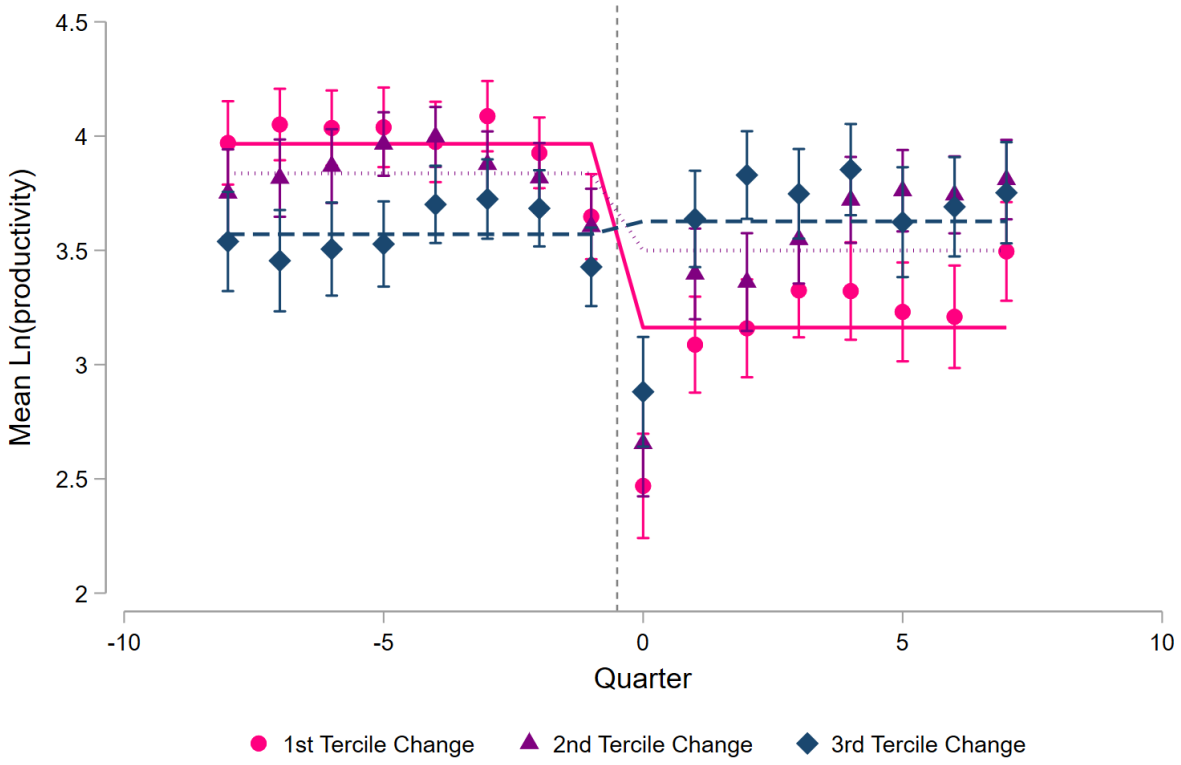


Figure 4: Average log productivity for judges experiencing a long-term change in court, by tercile of changes in court FE.

Notes: This figure plots the average log productivity and the associated 95-percent confidence intervals two years before and after a long-term change in court.The figure plots three types of long-term changes, by terciles of the change of court fixed effects: (1) an increase in court quality (diamonds), (2) a decrease (circles), and (3) no change (triangles). The horizontal axis reports the distance in quarters relative to the long-term change in court.

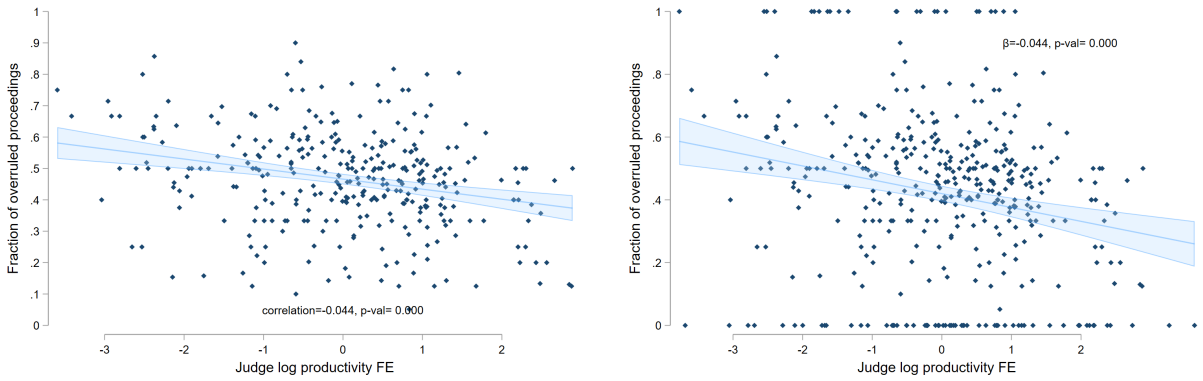
A third potential source of endogeneity is the possibility that judges are reallocated in response to transitory negative or positive shocks in their performance ε_{it} (Card et al., 2013). There is some decrease in productivity in the quarter before a court change, but we also observe a substantially stronger drop in the quarter after the event (the court change). 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 the *pre*-event selection dynamics (Ashenfelter, 1978). The

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

4.2 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 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. Specifically, are more productive judges more likely to have their decisions overturned on appeal?

Figure 5 provides no evidence of a trade-off between the quantity and quality of first-instance judicial decisions. The figure shows a small negative correlation (-0.04) 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.



(a) Reversal, unconditional **(b)** Reversal, conditional on appeal

Figure 5: Correlation between judge log productivity and reversal 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:

$$r_{ijt} = \hat{\alpha_j} + \phi_{c(j,t)} + \xi_t + \omega_{k(i)} + \nu_{ijt} \tag{5}$$

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-instace court fixed effects. $\omega_{k(i)}$ are fixed effects for the key word 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 results are reported in Table C4. Consistent with Figure 5b, the estimated relationship between productivity and the probability of reversal is negative, although the coefficient is not statistically different from zero. 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. This finding is consistent with evidence from other areas of the public sector showing that higher individual performance is associ-

ated with better outcomes. For example, Best et al. (2023) find that more effective public procurers not only obtain lower prices but also achieve higher spending quality.

4.3 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).

This raises the question of whether the highest-paid judges are also the most productive. Even in the absence of performance-based incentives, efficiency wage theory predicts that higher wages could increase productivity. By raising the cost of job loss, higher pay can reduce turnover and deter shirking when monitoring is imperfect (Salop, 1979; Shapiro and Stiglitz, 1984); in a gift–exchange framework, it can also elicit greater effort as workers reciprocate with improved performance (Akerlof, 1982).

Although I do not observe individual judges' wages, I observe 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 in a stepwise manner with age until retirement. Figure D12 shows that productivity, measured as the average number of complexity-weighted proceedings completed per quarter, follows an inverted U-shaped profile, peaking around age 45 for men and age 50 for women. As a result, the correlation between productivity and predicted wages is only 0.02. A similar pattern emerges when productivity is measured using the judge fixed effects estimated from Equation (2). Plotting these fixed effects against predicted average annual net earnings reveals only a weak, slightly concave relationship, with a correlation of 0.032. Overall, the evidence suggests that, prior

to the introduction of the incentive scheme, more productive judges were not rewarded with higher pay.

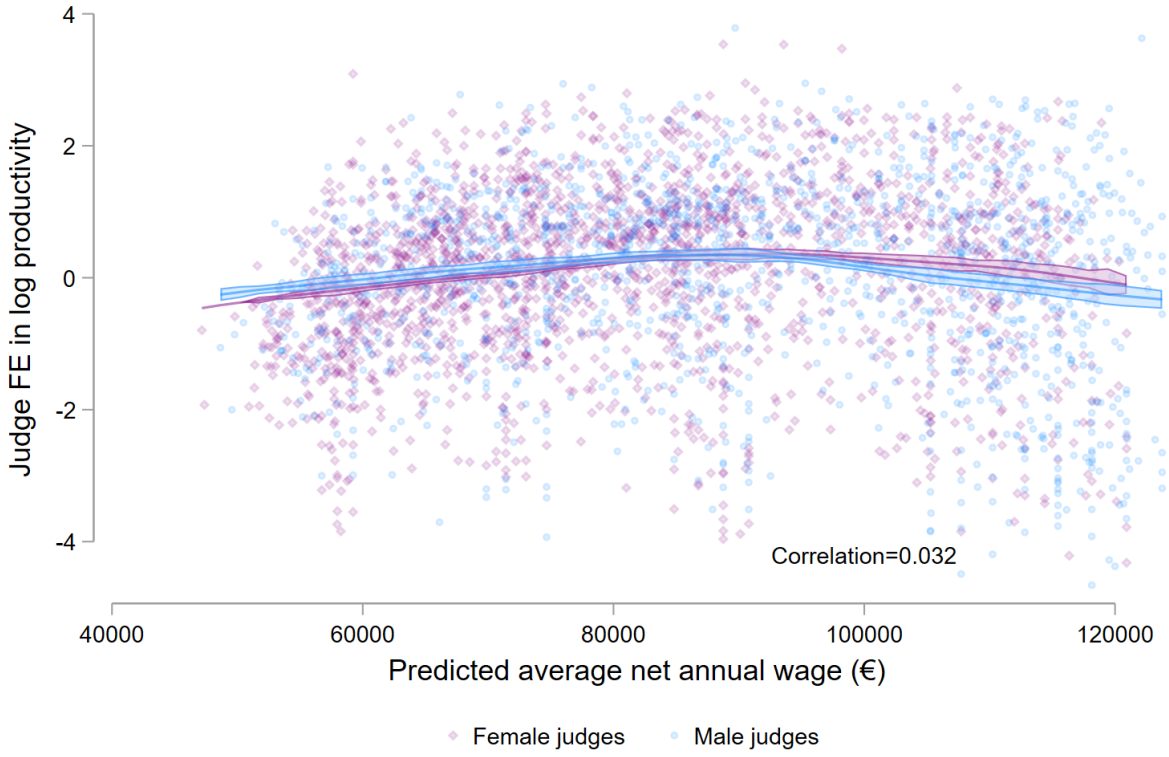


Figure 6: Correlation between wages and judge FE in log productivity

Notes: This figure shows how the judge fixed effects in log productivity varies by judges' predicted wages, separately for female and male judge. 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 the career at year 25 and passed each of the seven professional valuations every four years (Dipartimento della Funzione Pubblica, 2025; Area Democratica per la Giustizia, 2025).

4.4 Productivity increases in specialization and demand.

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 7 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.

Court characteristics include labor inputs (the numbers of auxiliary staff, clerks, and administrative officials), capital inputs (the surface area of offices, archives, and courtrooms), technological adoption (the share of judgments submitted digitally), and demand (the numbers of incoming and pending proceedings). Judge characteristics include gender, average age, and subject-matter specialization. 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.[18]

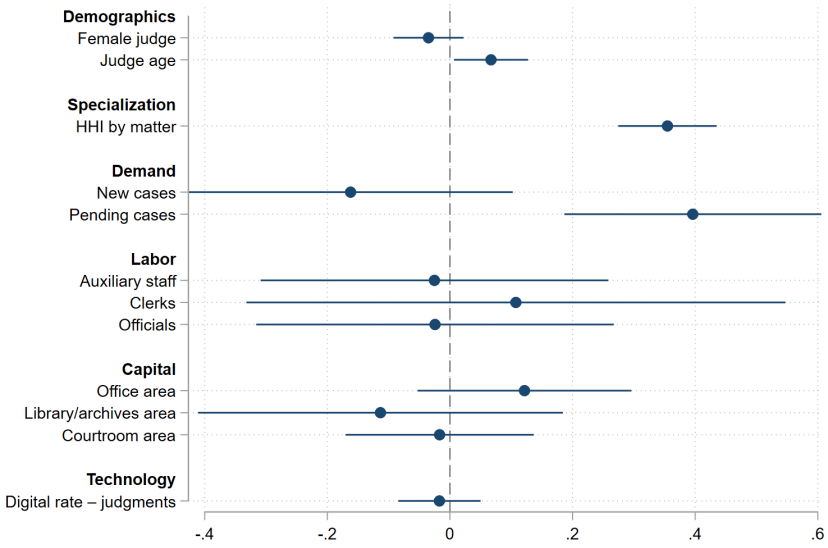


Figure 7: Predictors of judges' productivity fixed effects

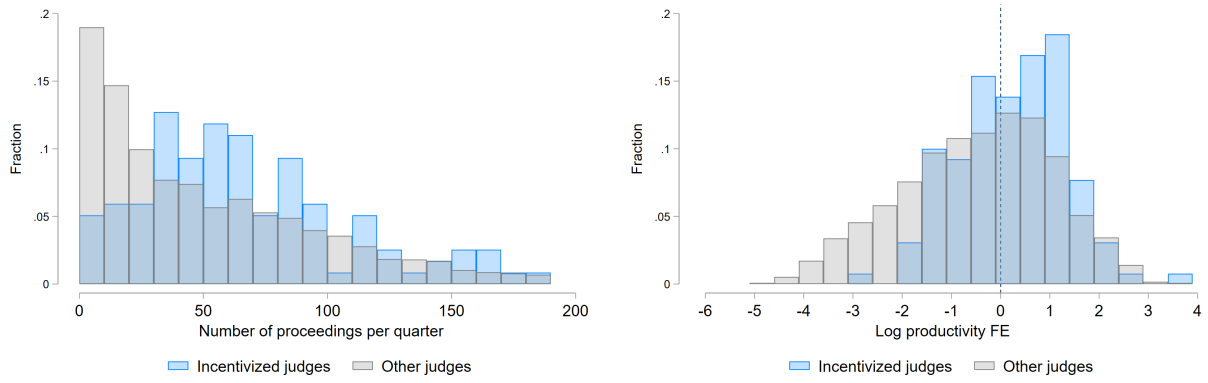
Notes: The figure shows the coefficients of a multivariate regression of judge fixed effects of log productivity on a set of predictors. Judge fixed effects are estimated as in Equation (2). Labor, capital, technology and demand covariates refer to the court in which the judge is working. Demographics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

[18]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.

Although the evidence is correlational, Figure 7 suggests that specialization and backlog are relevant drivers of judges' productivity. 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. This result is consistent with theoretical predictions and empirical evidence indicating that 'task juggling' is detrimental to judicial productivity (Coviello et al., 2014, 2015). The positive correlation between backlog and productivity explains why target courts, despite facing the largest backlogs, are more productive on average (Figure D5). This 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.

4.5 Incentivized judges were ex-ante more productive.

Figure 8: Distribution of ex-ante productivity: incentivized and non-incentivized judges.



(a) Number of proceedings

(b) Log productivity FE

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 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

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

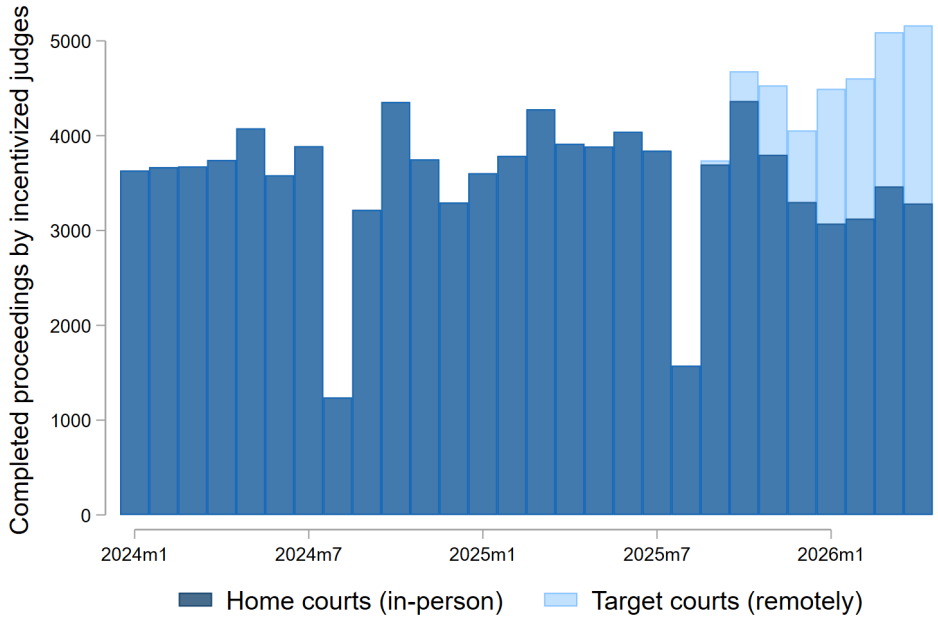
Prior to the introduction of the incentive scheme, judges that later participated in the remote work incentive scheme were already more productive than their peers (Figure 8). The median participant ranked above approximately two thirds of non-participating judges in the pre-incentive productivity distribution. 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.

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.

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:

$$ln(P_{jt}) = \sum_{s=-12}^{9} \beta_{T+s} I_j + \alpha_j + \psi_{c(j,t)} + \tau_t + \epsilon_{jt}, \text{ for } t \in [T-12, T+9] \tag{6}$$

where T denotes September 2025, the month in which judges applied to participate in the incentive scheme. I_j equal to 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 $s \in [1,9]$ the coefficients test for dynamic treatment effects during the incentive period, for $s \in [-12,-1]$ the coefficients indirectly test for the validity of the assumption of parallel trends between incentivized and non-incentivized judges. In the main

specifications, we use all non-incentivized judges as the control group.

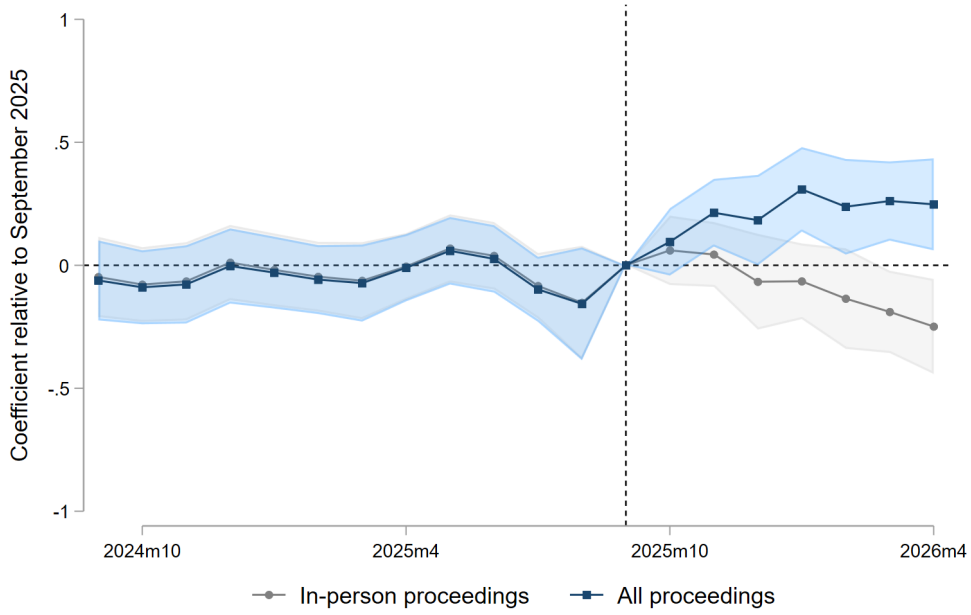


Figure 10: The effects of remote work incentives on incentivized judges' total and in-person productivity.

Notes: The figure plots the event-study coefficients β_{T+s} from Equation (6), with September 2025 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.

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 and stabilize at around one-third above pre-treatment levels. 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

D15).

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:

$$ln(P_{jt}) = \beta I_j \cdot Post_t + \alpha_j + \psi_{c(j,t)} + \tau_t + \sum_{s=1}^{12} \delta_s I_j \cdot 1\{m = s\} + \epsilon_{jt} \tag{7}$$

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).[19] 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	9.289***	0.366***	-4.582**	-0.017
	(2.018)	(0.067)	(1.889)	(0.069)
N	393130	393130	393130	392572
Mean	23.475	2.543	23.434	2.542
R-squared	0.427	0.448	0.427	0.448
Time FE	YES	YES	YES	YES
Type 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, court of home 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 5 additional proceedings per month relative to a pre-treatment average of 24 (column 1 of Table C5),

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

implying an increase in unweighted productivity of approximately 20 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 8 remote proceedings per month more than offset the decline in in-person activity, resulting in a net increase in productivity.

The effects are even larger when productivity is weighted by case complexity (Table 4). Incentivized judges completed the equivalent of approximately 9 additional complexity-weighted proceedings per month relative to a pre-treatment average of 24, corresponding to a 37 percent increase in weighted productivity. Although judges completed almost 5 fewer in-person complexity-weighted proceedings per month, this effect is statistically different from zero only in the linear and not in the log specification. When the control group is 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 C8-C9).

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 (7) 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 D16a-D16b 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.[20]

Tables C6–C7 confirm the absence of adverse effects of remote-work incentives on judges who did not participate in the scheme. The positive effect estimated for judges in target courts is likely driven by the presence of 82 incentivized judges from outside the first-instance

[20] 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 C6-C7.

court system who started assisting target courts through the incentive scheme (see Section 2.1).

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 number of proceedings completed by target courts.

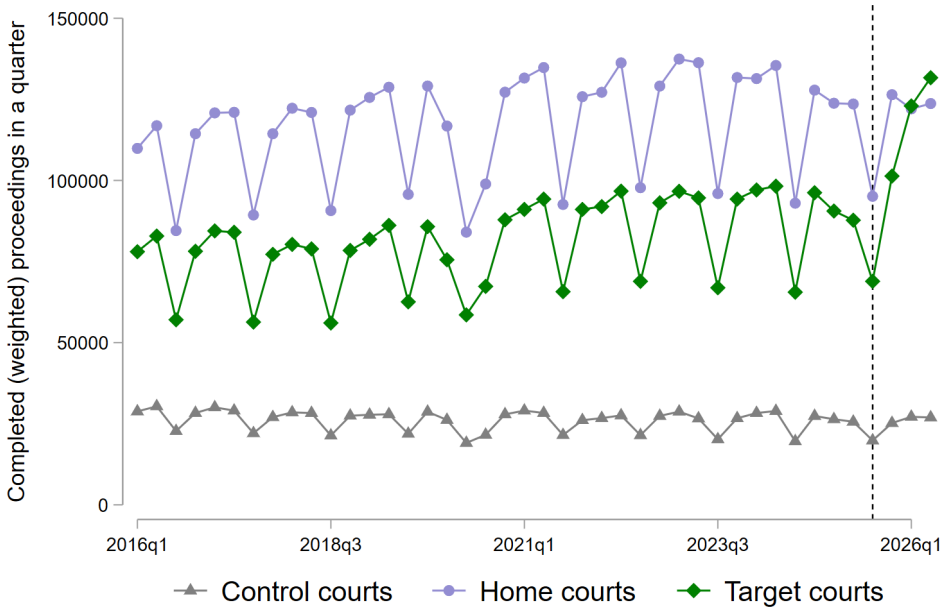


Figure 11: Completed proceedings in target, home, and control courts before and after the introduction of remote work incentives.

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 among target courts after the introduction of the incentive scheme, with the number of complexity-weighted proceedings completed

exceeding that of 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 using court-month level data:

$$ln(P_{ct}) = \sum_{s=-12}^{9} \gamma_{T+s} \, Target_c + \theta_c + \pi_t + v_{ct}, \text{ for } t \in [T-12, T+9] \tag{8}$$

$$ln(P_{ct}) = \gamma \, Target_c \cdot Post_t + \theta_c + \pi_t + \sum_{s=1}^{12} \rho_s \, Target_c \cdot 1\{m = s\} + \kappa \, Target_c \cdot t + v_{ct} \tag{9}$$

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 130 incentivized judges identified in my sample, as well as by the additional 82 incentivized judges assigned from non-first-instance courts. In the difference-in-difference 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, D17, and Table 5 show that the incentive scheme significantly increased the output of target courts during its nine months of operation. The number of proceedings completed each month in a target court increased by 51 relative to a pre-treatment average of 532, while complexity-weighted productivity rose by 16 percent. Given 48 target courts and a nine-month implementation period, the estimates imply that the remote work incentive scheme generated 22 thousand additional completed proceedings.

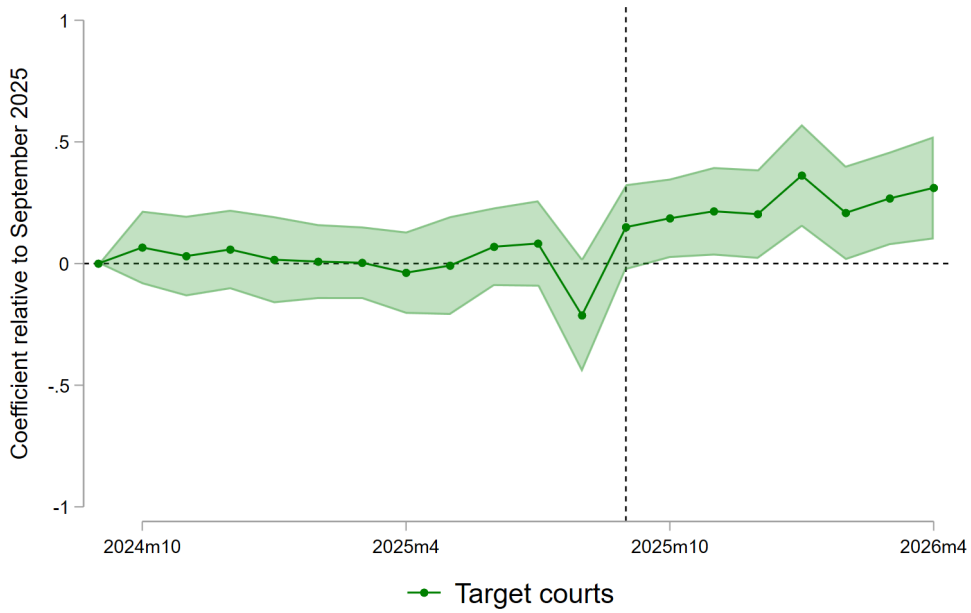


Figure 12: The effects of remote work incentives on log monthly complexity-weighted proceedings completed by target courts.

Notes: The figure plots the event study coefficients, γ_{T+s}, from Equation (8), using September 2024 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 550 thousand pending proceedings in target courts. The additional output generated by remote work incentives therefore reduced the national backlog by 1.8 percent and the backlog of target courts by 4 percent within nine months.[21] In 2025, Italian first-instance civil courts completed 1.28 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 1.7 percent nationally and by 4.7 percent in target courts within nine months.

[21]I exclude proceedings before justices of the peace, as well as property and insolvency enforcement proceedings, because they are not included in my dataset. The total backlog in 2025, including these categories, was 1.53 million proceedings.

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

	Unweighted		Weighted	
	Proceedings	Ln(Proceedings)	Proceedings	Ln(Proceedings)
	(1)	(2)	(3)	(4)
Post*Target court	51.308**	0.063*	131.677**	0.157***
	(25.775)	(0.035)	(57.993)	(0.047)
N	10540	10540	10540	10540
Mean	532.199	5.850	560.386	5.888
R-squared	0.908	0.908	0.877	0.895
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.

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, eliminating the entire backlog through the incentive scheme would cost €561 million. 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 four potential concerns.

First, incentivized judges may substitute away from their regular duties, reducing the number of proceedings completed in person in their home courts. The event-study estimates

indicate some decline in in-person activity. However, this reduction is less than half the increase in remotely completed proceedings, implying a substantial net gain in productivity. Second, reallocating judges' effort toward target courts could reduce productivity of non-incentivized judges in both home and target courts. For example, non-participating judges might have to absorb additional administrative tasks or coordinate with remotely assigned colleagues. Yet, the event-study finds no evidence of such spillover effects.

Third, rewarding judges solely for the number of proceedings completed could reduce decision quality, increasing appeals and shifting congestion to higher courts. The absence of an ex-ante positive 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. Fourth, the scheme's impact is constrained by limited take-up: only 212 judges participated. Even if every participant completed 100 remote proceedings over nine months, clearing the current backlog would take 54 years.

These considerations raise the question of how remote work incentives could be redesigned to achieve larger gains while preserving quality and in-person productivity. A reasonable approach would be to implement a more generous 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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A Additional figures

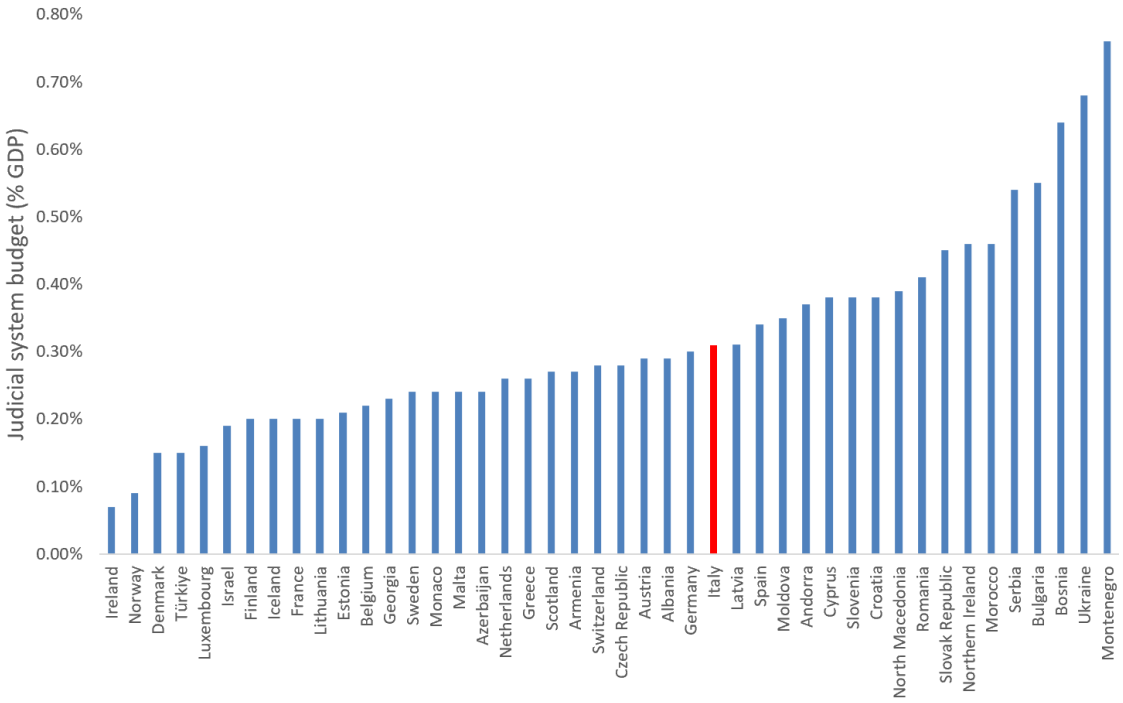


Figure D1: Judicial budget as a share of GDP. Notes: Shares are expressed in percent of GDP. Source: CEPEJ (2024).

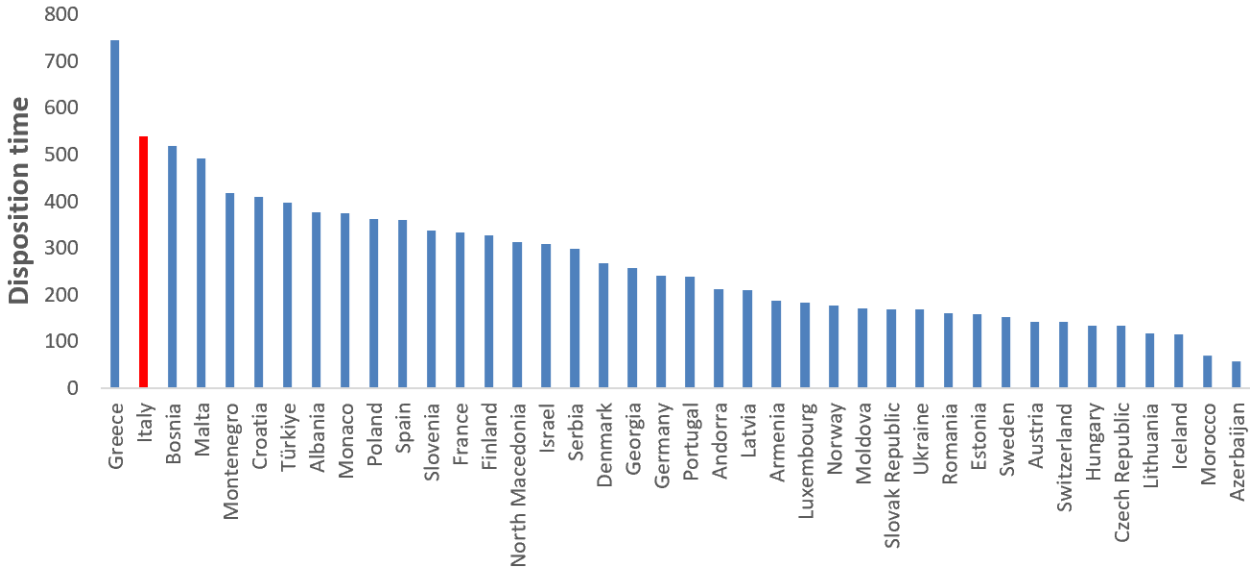


Figure D2: Disposition time in civil and commercial cases across Europe. Notes: Higher values indicate slower case processing. Source: CEPEJ (2024).

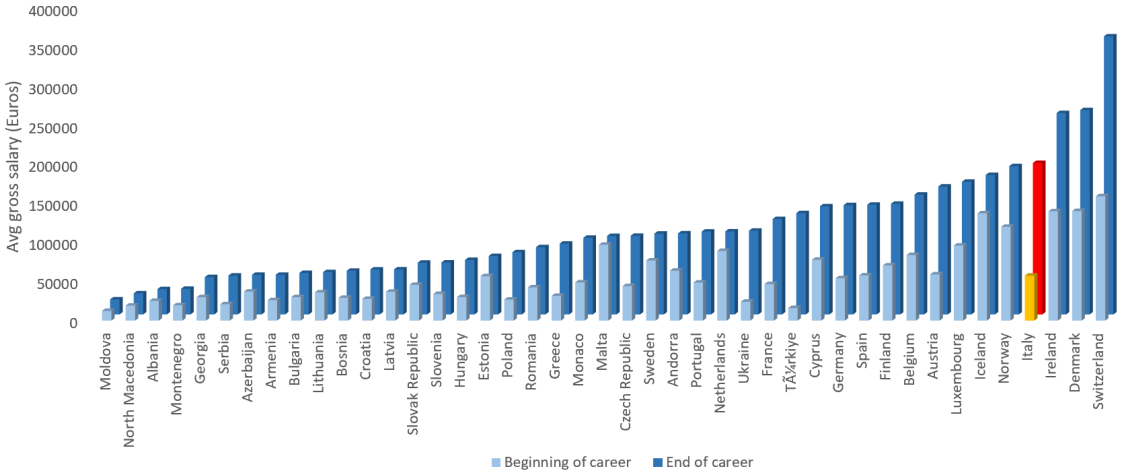


Figure D3: Gross annual salaries of judges at the end of career. Notes: Nominal gross pay, latest year available. Source: CEPEJ (2024).

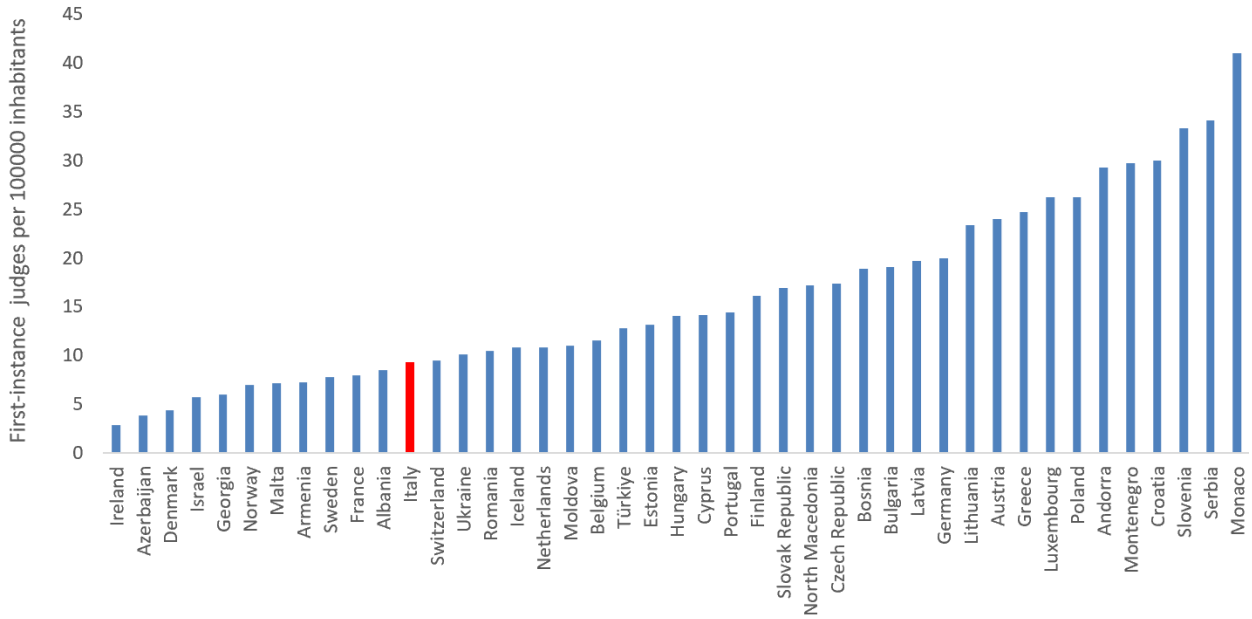
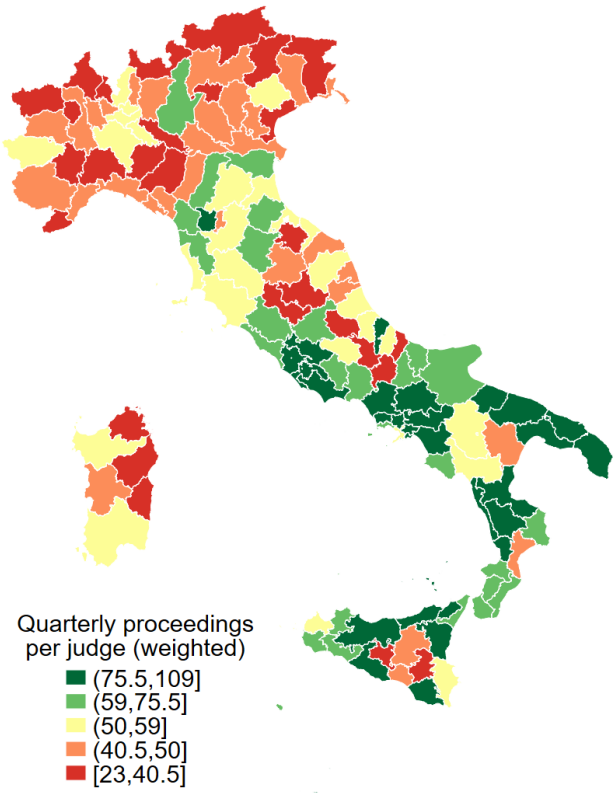


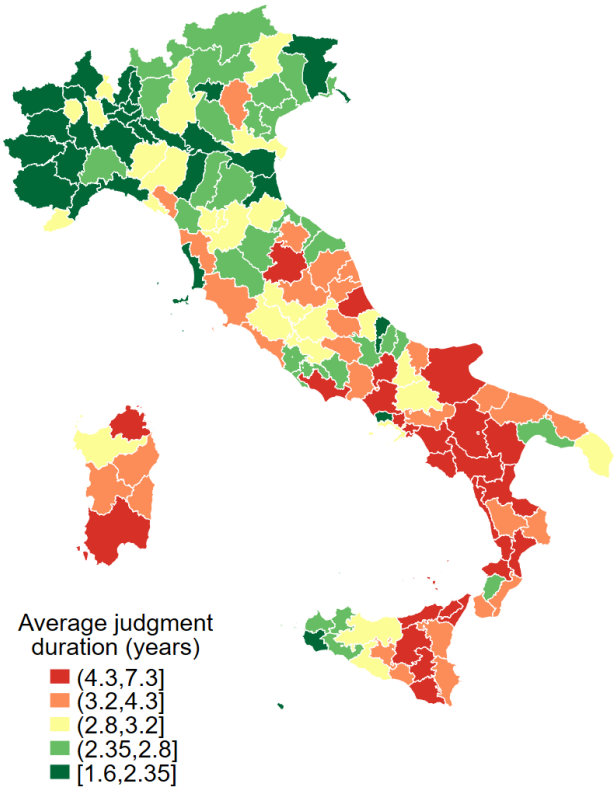
Figure D4: Number of professional judges per 100,000 inhabitants. Source: CEPEJ (2024) (CEPEJ, 2024).

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.

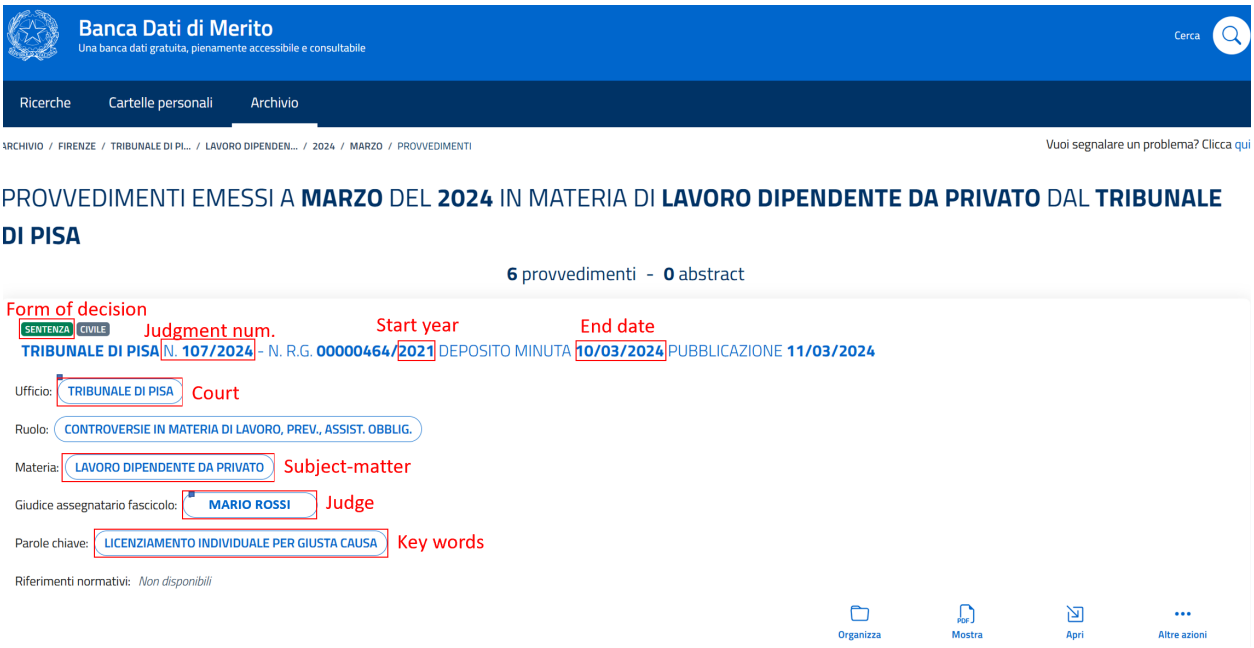


Figure D7: Webscraping information on first-instance civil proceedings and appeals.

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 key words. I replaced the name of the judge with a pseudonym to protect the judge's privacy.

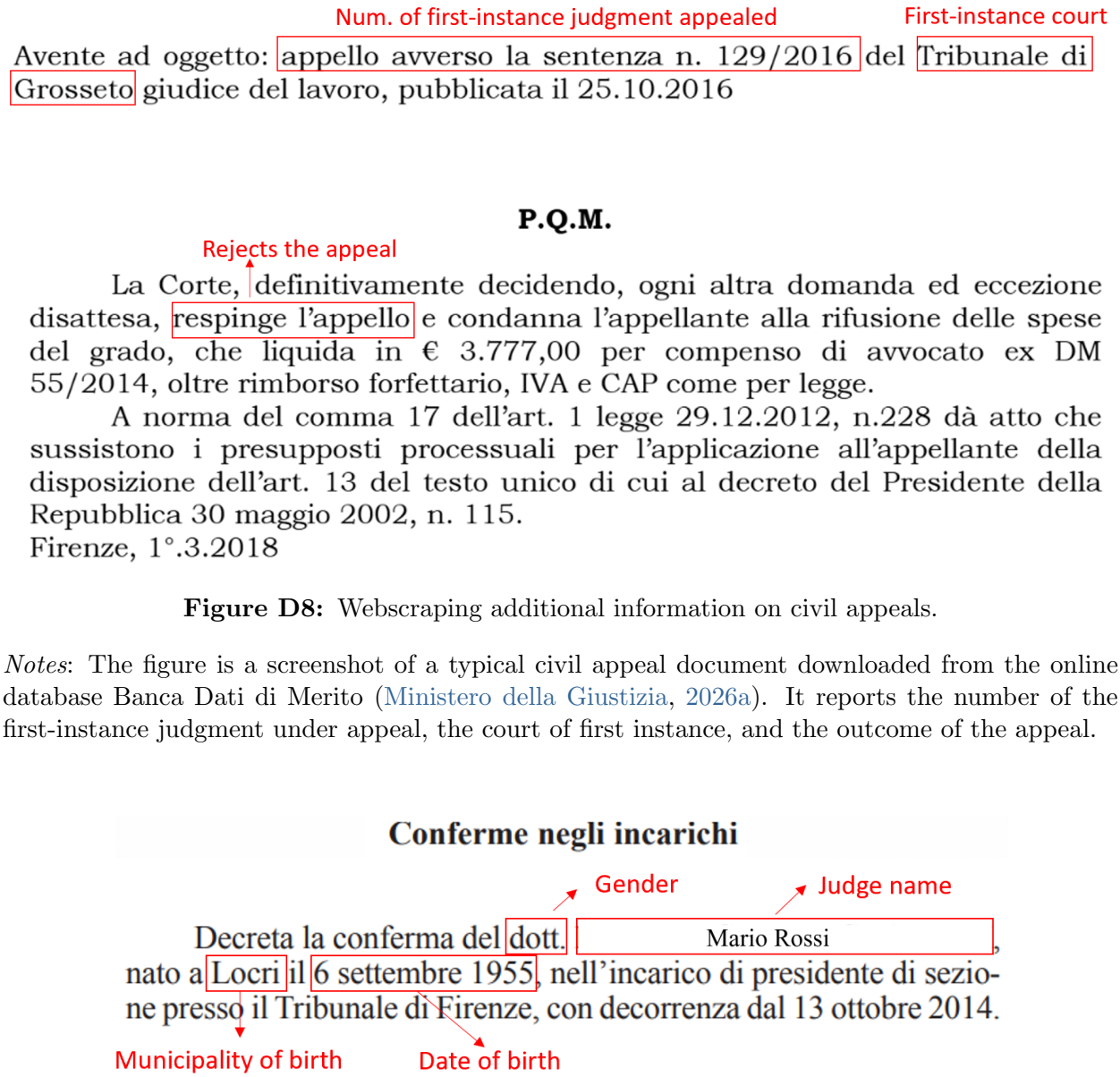


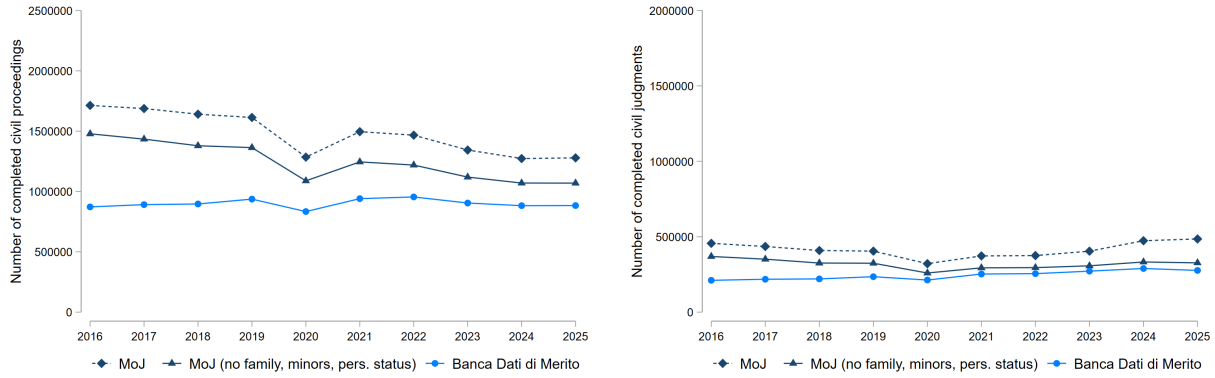
Figure D8: Webscraping additional information on civil appeals.

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: Webscraping judges' demographics.

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).

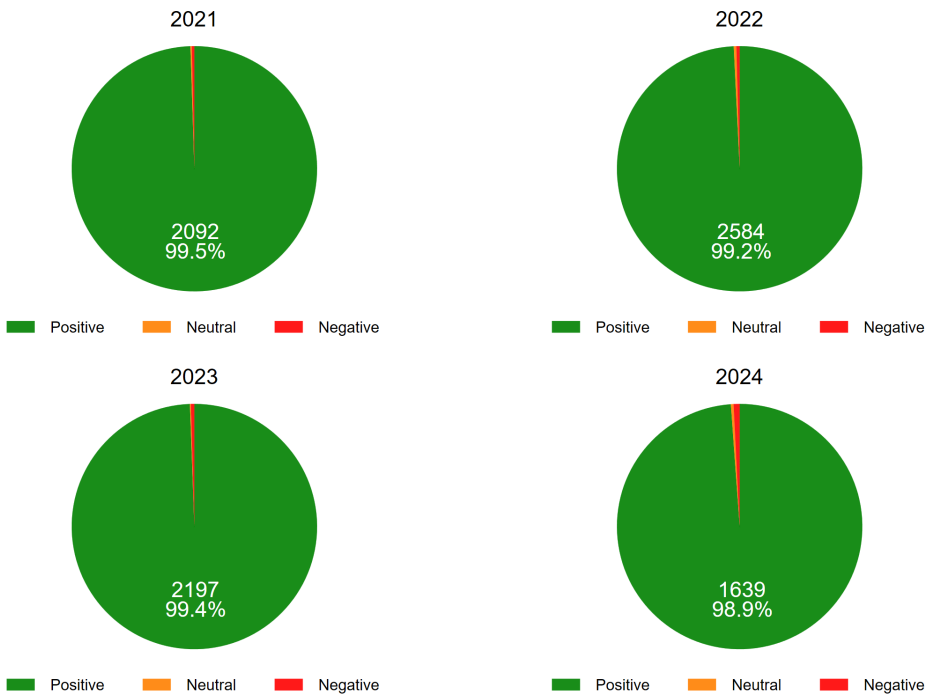


Figure D11: Judicial performance evaluations

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 is from the Ministry of Justice and it is reported in Negri (2026).

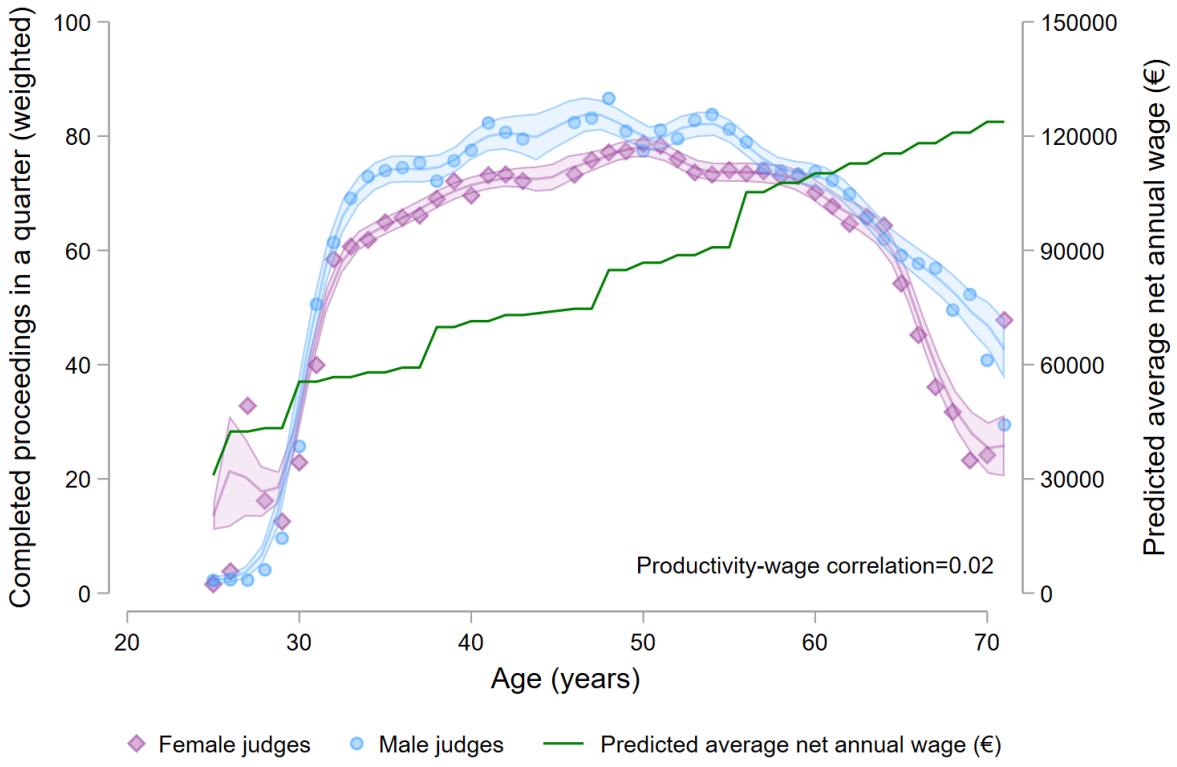
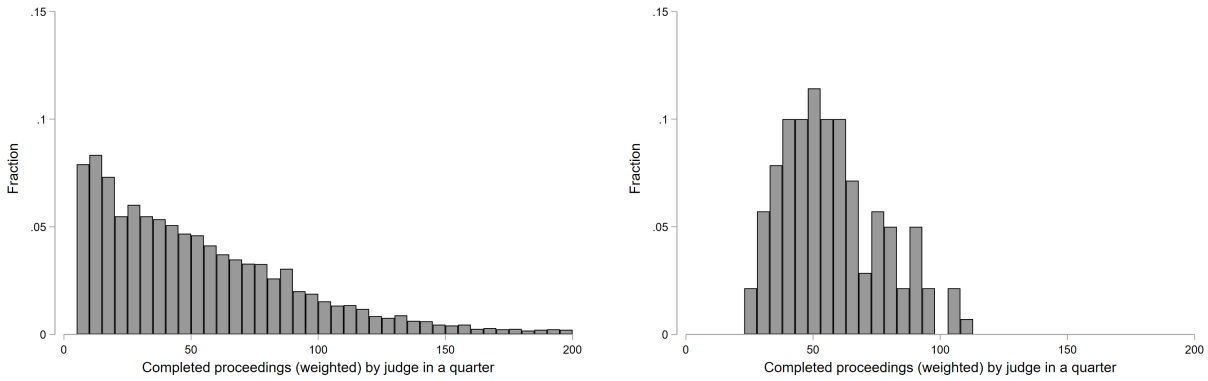


Figure D12: How average number of judgments per year varies by judges' age and income

Notes: This figure shows how the average number of judgments per year varies by judges' age and gross compensation, separately for female and male judges. Each bin is the average number of judgments per year completed by all female (in violet) and male (in blue) judges by year of age. The shaded areas represent the 95 percent confidence intervals of a kernel-weighted local polynomial regression of number of judgments per year on age. The green line represents the contractual gross compensation paid by the government for an ordinary judge, assuming the judge started the career at year 25 and passed each of the seven professional valuations every four years (Dipartimento della Funzione Pubblica, 2025; Area Democratica per la Giustizia, 2025).

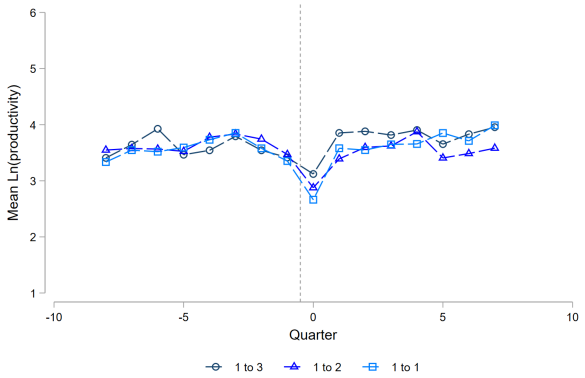


(a) Judges **(b)** Courts

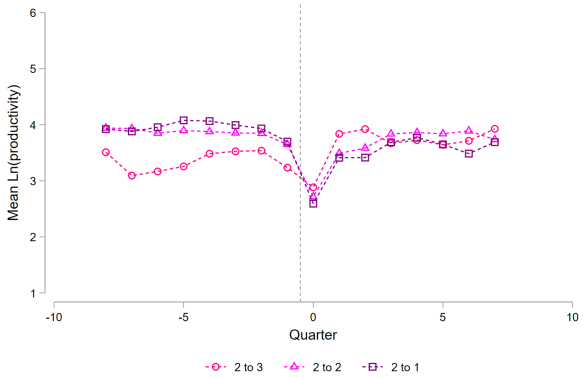
Figure D13: The distribution of productivity across judges and courts

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.

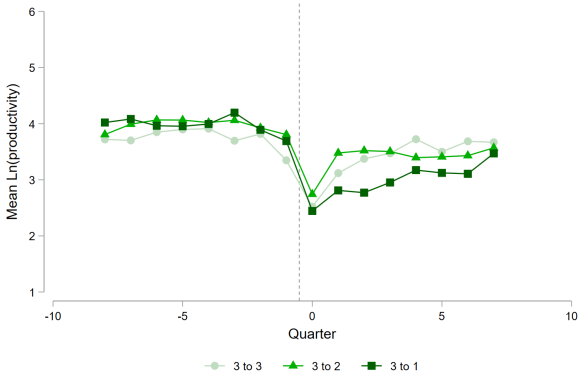
Figure D14: Average log productivity for judges experiencing a long-term change in court, by tercile of origin and destination court FE



(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. 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.

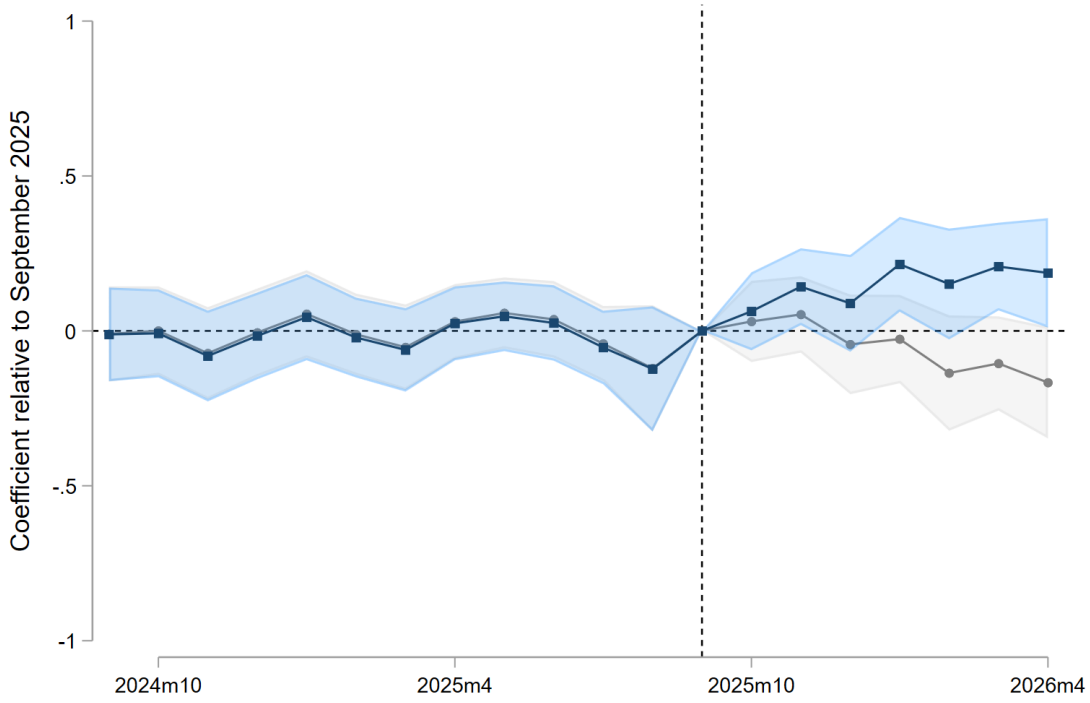
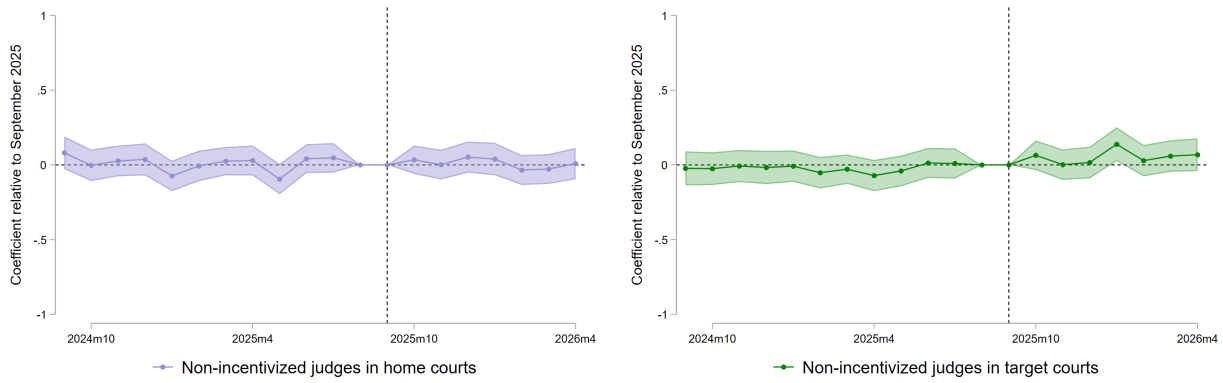


Figure D15: The effects of remote work incentives on incentivized judges' total and in-person *unweighted* productivity.

Notes: The figure plots the event-study coefficients β_{T+s} from Equation (6), with September 2025 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.



(a) Home courts **(b)** Target courts

Figure D16: Spillover effects on non-incentivized judges

Notes: The figures plot the event-study coefficients β_{T+s} from Equation (6), 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 2025 as the omitted reference month. The control group is composed by 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.

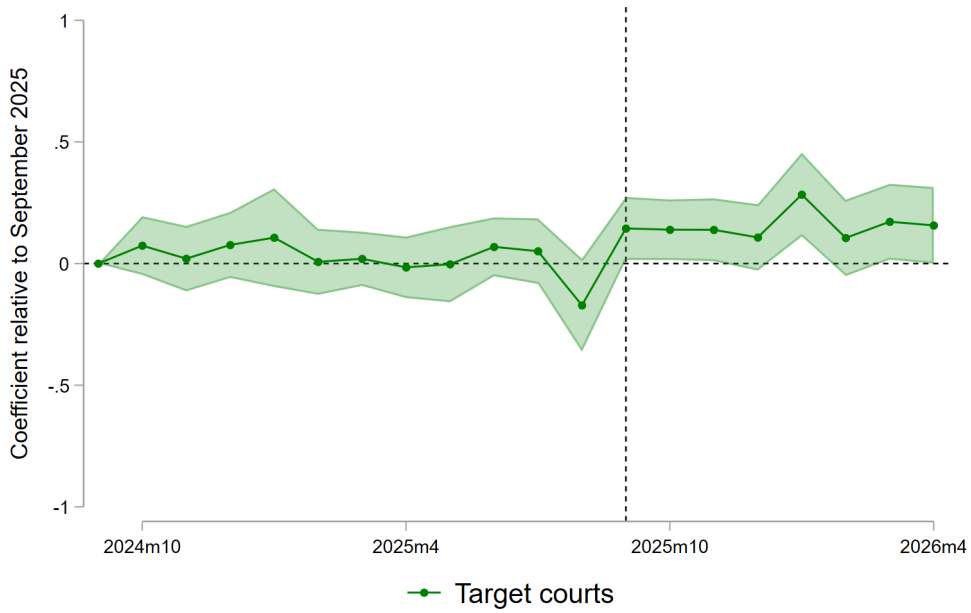


Figure D17: The effects of remote work incentives on log monthly *unweighted* proceedings completed by target courts.

Notes: The figure plots the event study coefficients, γ_{T+s}, from Equation (8), using September 2024 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	.	2
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	.	2
13	Velletri	635	15%	23	.	6
14	Termini Imerese	634	-16%	7	.	1
15	Oristano	626	-1%	3	.	1
16	Nola	613	-20%	13	.	3
17	Lamezia Terme	611	-36%	1	.	1
18	Brindisi	609	-14%	9	.	4
19	Belluno	606	61%	2	.	0
20	Paola	578	-31%	1	.	0
21	Siracusa	557	-15%	8	.	2
22	Bologna	553	4%	24	.	1
23	Cassino	549	-29%	3	.	0
24	Lecce	548	-2%	32	.	8
25	Perugia	545	-19%	4	.	0
26	Latina	539	-14%	9	.	5
27	Brescia	535	-1%	20	.	5
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	.	8
41	Locri	470	-20%	5	.	2
42	Massa	467	-5%	3	.	1
43	Trento	467	0%	3	.	1
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	.	13
	Total			500	212	130

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: Judge characteristics

	(1) Full sample	(2) Movers	(3) Incentivized
A. Demographics			
Female	0.58	0.57	0.51
Age	48.92	46.24	44.52
B. Macroareas of birth			
South	0.40	0.40	0.44
Islands	0.13	0.12	0.14
Center	0.19	0.20	0.15
North-East	0.11	0.10	0.08
North-West	0.16	0.17	0.18
Abroad	0.01	0.00	0.01
C. Number of judges			
With observed characteristics	3561	1178	108
Total	6125	1438	130

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.

Table C3: Court characteristics

	(1) Full sample	(2) Largest connected set
A. Labor		
Civil judges	18.52	18.52
Court assistants	16.92	16.92
Court clerks	74.42	74.42
Court officers	6.25	6.25
N	959	959
B. Capital		
Court offices	12.78	12.78
Court archives or libraries	5.29	5.29
Courtrooms	2.70	2.70
N	1370	1370
C. Technology		
Digital filing rate (acts)	0.44	0.44
Digital filing rate (judgments)	0.52	0.52
N	700	1370
D. Demand and output		
Incoming proceedings	10.10	10.10
Pending proceedings	10.08	10.08
Subject-matter homogeneity	0.09	0.09
Resolved proceedings	10.57	10.57
Resolved proceedings with judgment	2.96	2.96
N	1400	1400

Note: The table reports the averages of court characteristics. The statistics are computed over the full sample of courts in column (1) and over the largest connected set of courts in column (2). 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. The surface measures are in square meters. *Subject-matter homogeneity* 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). All other variables are count variables.

Table C4: Correlation between judge log productivity and reversal of first-instance decisions

	Reversal	Reversal, conditional on appeal	Reversal, conditional on appeal
	(1)	(2)	(3)
Log productivity FE	-0.004	-0.004	-0.007
	(0.008)	(0.008)	(0.008)
N	18246	18246	18230
Mean	0.483	0.483	0.483
R-squared	0.073	0.073	0.188
Time FE	YES	YES	YES
Type FE	YES	YES	YES
Court FE	YES	YES	YES
Appeal judge FE	NO	NO	YES

Notes: This table examines the correlation between judge productivity (measured as log productivity fixed effects) and the probability that a first instance decision is reversed on appeal. 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. Standard errors are clustered at the judge level. The sample is restricted to courts matched with corresponding courts of appeal.

Table C5: 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 judge	4.739***	0.199***	-3.154*	-0.054
	(1.670)	(0.058)	(1.647)	(0.062)
N	393130	393130	393130	392572
Mean	23.626	2.505	23.588	2.504
R-squared	0.543	0.543	0.543	0.543
Time FE	YES	YES	YES	YES
Type 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, court of home 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 C6: The effect of remote-work incentives on the productivity of incentivized judges

	Proceedings (1)	Ln(Proceedings) (2)
Post*target court	5.975***	0.161***
	(1.966)	(0.045)
Post*home court	0.782	-0.025
	(0.758)	(0.043)
N	377916	377916
Mean	23.258	2.536
R-squared	0.422	0.449
Time FE	YES	YES
Type FE	YES	YES
Court FE	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, case type (keyword), and first instance court. Column (3) additionally includes 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 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 C7: The effect of remote-work incentives on the productivity of incentivized judges, unweighted

	Proceedings (1)	Ln(Proceedings) (2)
Post*target court	2.383**	0.076*
	(1.008)	(0.042)
Post*home court	-0.302	-0.048
	(0.896)	(0.041)
N	377916	377916
Mean	23.443	2.495
R-squared	0.541	0.546
Time FE	YES	YES
Type FE	YES	YES
Court FE	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, case type (keyword), and first instance court. Column (3) additionally includes 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 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 C8: The effect of remote-work incentives on the productivity of incentivized judges

	Proceedings	Ln(Proceedings)	In-person proceedings	Ln(in-person proceedings)
	(1)	(2)	(3)	(4)
Post*Incentivized	11.958***	0.371***	1.410	0.097
judge	(2.249)	(0.078)	(1.913)	(0.077)
N	73200	73200	73200	72638
Mean	20.286	2.430	20.064	2.425
R-squared	0.549	0.439	0.551	0.441
Time FE	YES	YES	YES	YES
Type 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, court of home 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 C9: The effect of remote-work incentives on the productivity of incentivized judges, unweighted

	Proceedings	Ln(Proceedings)	In-person proceedings	Ln(in-person proceedings)
	(1)	(2)	(3)	(4)
Post*Incentivized	5.963***	0.209***	0.177	0.037
judge	(1.999)	(0.068)	(1.819)	(0.066)
N	73200	73200	73200	72638
Mean	21.967	2.474	21.763	2.469
R-squared	0.599	0.518	0.601	0.521
Time FE	YES	YES	YES	YES
Type 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, court of home 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.