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Effect of Business Uncertainty On Turnover

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Abstract

We document a causal relationship between business uncertainty and workforce management at the firm level, by leveraging litigation-generated quasi-experimental variation in business uncertainty. The causal effects of business uncertainty on turnover, hiring and separations are of the expected negative direction, and of sizable magnitude. These consequences are stronger among firms that operate in sectors in which business uncertainty is intrinsically higher, and can be attributed to the effect of regulation induced business risk on normal operations. In particular, employee turnover, hiring and separations are ratcheted down to reduce the risk of additional wrongful termination lawsuits.

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1 Introduction

When firms perceive that their business environment has become temporarily more uncertain, they are expected to become temporarily more cautious in hiring and firing. This caution, according to a leading theoretical model (Bernanke, 1983), reflects the option value of waiting in the presence of hiring and firing costs: if a lot of uncertainty about the business environment will be dispelled soon, it makes no sense for firms to incur the labor adjustment costs now. A different explanation is based on the idea that the actions of hiring and firing entail business risk, and the firm could be especially wary of taking on that risk when it is subjected to an especially uncertain business environment.

A large empirical macro literature has sought to estimate the effect of uncertainty on firm behavior including hiring, firing, and turnover: see Bloom (2014) for a review. This literature has relied primarily on three identification strategies: statistically-constructed time-varying measures of aggregate uncertainty;[1] calibrated structural models;[2] and unforeseen spikes in uncertainty caused by natural disasters or terrorist events, paired with shift-share cross-sectoral variation.[3] These identification strategies suffer from two limitations. First, identifying based on economy-wide or industry-wide shocks, raises the concern that firms might be reacting to the behavior of other firms and consumers, rather than to changes in their own beliefs about uncertainty. Second, parsing out the role of *expectations* about the economy from the *realizations* of economic shocks, is challenging: Berger, Dew-Becker and Giglio (2020) argue that the latter, not the former, account for observed firm responses.[4] To our knowledge the causal relationship between uncertainty and turnover has not yet been examined using firm-level variation in uncertainty.

From an identification perspective, the ideal variation would have the following properties. First, variation in uncertainty (i.e., in the second moment of forecasts) should be distinguishable from variation in ex-post realizations of said uncertainty. Second, the

[1] E.g., Ramey and Ramey (1995).

[2] E.g., Bloom (2009), Leduc and Liu (2016), and Bloom et al. (2018).

[3] E.g., Baker, Bloom and Davis (2016).

[4] This identification concern is especially salient because spikes in macroeconomic uncertainty tend to coincide with recessions, i.e., with systematically bad shocks. See, e.g., Bloom (2009), Jurado, Ludvigson and Ng (2015), Bloom et al. (2018).

variation in uncertainty should be at the firm level, and be uncorrelated with aggregate uncertainty, to avoid general equilibrium confounding effects. Third, it should be possible to sharply date the beginning and end of the uncertainty window. This is because the theory predicts a sharp rebound once the uncertainty is resolved and, also, because option value theory predicts that permanent increases in uncertainty may produce qualitatively different outcomes from temporary ones.[5]

In this paper we leverage a source of variation in uncertainty that meets the above desiderata. The uncertainty concerns the damages that will be assessed on firms that are defendants in wrongful employees' termination trials. The shock to the uncertainty is represented by the trial's duration. Due to a peculiarity of Italian law, if a firm with more than 15 employees is found guilty of wrongfully terminating an employee, the firm must hire back the worker without prejudice *and pay back all the wages and social security contributions accrued during the trial*.[6] The latter provision implies that the uncertainty associated with the trial outcome is effectively increasing in the time it takes the judge to decide the case.

Specifically, the uncertainty shock that we will exploit consists in being randomly assigned to a slow judge as opposed to a fast one. This shock has several advantages related to the above discussion. First, because we can study firm behavior *during* vs *after* the trial, we can measure the effect of a shock on uncertainty (by looking at firm behavior during the trial) separately from the effect of the ex-post realizations of said uncertainty (which we can measure by comparing the behavior of winner vs loser firms after the trial). Second, being randomly assigned to a slower judge is a shock that has no general equilibrium effects. Third, it is possible to sharply date the beginning and end of the uncertainty spike.

We find that, during a trial, firms slow down their turnover, hiring and separations. This effect is sharper for firms that are assigned to a systematically slower judge. The differential effect of having been assigned to a slower judge vanishes when the trial is over, irrespective of whether the firm wins or loses the trial. Moreover, we also find that firm-

[5]See, e.g., Figure 9 in Bloom (2009), where a permanent increase in uncertainty causes an increase, rather than a decrease in employment.

[6]The entire analysis of this paper will therefore be focused on firms with more than 15 employees.

level productivity (as measured by value added) suffers when a firm is assigned to a slower judge. However, we find no statistically significant effects on sales and profitability.

Additionally, we find that, within our universe of firms with more than 15 employees, the effects are concentrated in firms with fewer than 50 workers, which makes sense because the uncertainty created by a single labor trial is less salient in larger firms. Moreover, we find that the effects are limited to firms in high-volatility sectors. This correlation, if interpreted causally, would mean that the idiosyncratic shocks to the firm's expectations have a greater impact when the general economic situation is more volatile. Finally, we find no effect on investment: variation in judge assignment does not cause firms to alter their investment decisions. We discuss several possible explanations for this finding.

We finally compare two theories based on their ability to accommodate the above facts: option value theory, and a theory based on the firm's risk aversion. We conclude that the latter seems to be more consistent with our findings.

Our identification strategy has advantages and limitations. The advantages have been mentioned above. Among the limitations are the concern that the exclusion restriction might fail due to other characteristics of the judge, other than the speed of decision, that affect firm outcomes. Reassuringly, though, we show that a salient characteristic (whether the randomly assigned judge is pro-worker or pro-business as measured by her previous decisions) has no effects on hiring, separation, and turnover. Also, the source of the identifying variation is conceptually related to workforce management and, therefore, quite different from the TFP shock that is at the core of the macroeconomic uncertainty literature. Finally, in our setting both the mean and the variance of the assessed damages are larger if the trial is longer (this is established formally in Proposition 1 below), meaning that the first and second moments of these damages co-move. Therefore, our natural experiment does not allow us to separately identify the effect of a mean preserving increases of uncertainty and the effect of a change in expectations, so that we interpret our evidence as speaking about the effect of the joint exogenous variation in the first and second moments of the shock. This is, however, not necessarily a disadvantage, given that pure mean preserving shocks are rare in reality, and the co-movement of first and second moments of shocks that we study is an interesting and frequently observed characteristic of real shocks hitting firms, as shown in the literature cited in footnote 4.

The paper proceeds as follows. After a brief review of the related literature in the next section, Section 3 describes the institutional setting and the data. Section 4 provides descriptive evidence of the effects studied in this paper. Section 5 presents the econometric framework that allows us to go beyond the descriptive evidence and Sections 6 discusses its identification. Sections 7 and 8 lay out the empirical findings. Section 9 connects the findings to two theoretical frameworks. Section 10 concludes.

2 Related literature

Our paper contributes to the literature on the effects of macroeconomic uncertainty on firm behavior. In addition to the above-mentioned literature, Baker, Bloom and Davis (2016) develop an index of economic policy uncertainty based on newspaper coverage frequency of events like the Gulf Wars I and II, the 9/11 attacks, the failure of Lehman Brothers, etc., and use it to study the effect of uncertainty on US firms. Using stock return volatility as a proxy, Bloom, Bond and Van Reenen (2007) study the effect of macroeconomic uncertainty on a sample of UK firms. Using survey data on managers expectations as a proxy, Guiso and Parigi (1999); Bontempi, Golinelli and Parigi (2010); Fiori and Scoccianti (2021) empirically study the effect of uncertainty in a sample of Italian firms. Di Maggio et al. (2022) build a firm-level measure of uncertainty based on the realized volatility of abnormal stock returns and document a significant pass-through of uncertainty shocks from firms to their employees. None of these studies leverages quasi-experimental exogenous variation in uncertainty such as we use in this study.

The next most closely related literature is that which relies on judge-based variation for identification. Two papers deserve particular mention: Gianfreda and Vallanti (2017) and Cahuc et al. (2021). Neither paper has data on firm-level hiring and termination (and thus turnover), which are key dependent variables for us. Based on what might be called "systemic variation", Gianfreda and Vallanti (2017) identify persistent changes in jurisdiction-level averages in trial duration, which may drive the firm to put in place precautionary changes *directed at avoiding being sued*. In contrast, our estimates, are based on "unforeseeable variation:" our identifying variation (being assigned to a slow or fast judge) is not revealed to the firm until *after it has been sued*, and it has no predictive power for *future lawsuits*. Therefore, our estimates are net of precautionary or anticipatory behaviors on the firm's part. Cahuc et al. (2021), like us, leverages "unforeseeable variation:" the random allocation of judges to cases.[7] Their outcomes are: employment, sales, and firm survival, but, unlike this paper, they are measured *after the trial has ended*, that is, after the uncertainty has realized. In contrast, we find that firm-level responses occur *only while the trial is ongoing*, which we interpret as evidence that firms act upon their expectations about the risk associated with the trial.

Closer to our paper, some studies have estimated the effect of variation in judicial speed on various firm-level outcomes: export levels (Nunn, 2007); breaches of contract, investments, access to finance, debt repayment, employment, and revenues (Guiso, Sapienza and Zingales, 2004; Jappelli, Pagano and Bianco, 2005; Djankov et al., 2008; Fabbri, 2010; Chemin, 2012; Ponticelli and Alencar, 2016; Giacomelli and Menon, 2016; Kondylis and Stein, 2018; Schiantarelli, Stacchini and Strahan, 2020). Lichand and Soares (2014) look at the effect on entrepreneurship rates.

To some extent related to our identification strategy, some studies have implemented the random judge design to estimate the causal effects of judicial decisions on various outcomes. Bernstein, Colonnelli and Iverson (2019) focus on bankruptcy decisions and asset utilization. Kling (2006); Green and Winik (2010); Loeffler (2013); Nagin and Snodgrass (2013); Aizer and Doyle Jr (2015); Mueller-Smith (2015); Stevenson (2017); Harding et al. (2017); Arnold, Dobbie and Yang (2018); Norris (2018); Arteaga (2020); Bhuller et al. (2020); Dobbie, Goldin and Yang (2018) and Dobbie et al. (2018) focus on recidivism and employment. French and Song (2014) look at disability insurance cases and labor supply. Galasso and Schankerman (2015) examine patent rights' decisions and citations of patents. Within this literature of "judge-based identification," to the best of our knowledge, our paper is the first to focus on worker turnover as the outcome variable.

[7] In their setting, judges vary in the likelihood of deciding in favor of the plaintiff. In our setting, in contrast, judges vary in the speed of disposition, and this translates into a variation in the financial penalty for wrongful termination.

3 Institutional setting and the data

Our cases are decided by monocratic judges who specialize in labor law.[8] In our court, an administrator assigns cases to judges using a computer-based random assignment algorithm. After that, a specific assignment judge is tasked with making any tweaks that might be needed to cope with specific personnel issues, such as prolonged sick leaves.[9] Therefore, it behooves us to test for random allocation of cases to judges. We do this in Section 6.

There are many types of labor trials and we focus on trials for wrongful termination because the peculiarities of the penalty structure in these cases form the basis of our identification strategy. The penalties for wrongful termination are set by statute as a function of the number of employees.[10] If a firm with more than 15 employees is found guilty of wrongful termination, the termination is deemed null, meaning that the firm must pay all back wages accrued during the trial, and must hire back the worker without prejudice after the trial is over.[11,12,13] As explained in Section 9, this feature creates a

[8] Neither other judges nor a jury are involved in the disposition.

[9] Judges on a sick leave lasting longer than one week are excused from new assignments. Vacation periods do not represent an exemption. Judges in our court cannot reject assignments. In a few rare cases, some judges show prolonged periods of inactivity (many months): we drop these judges, and any judge with less than ten cases per year.

[10] See Law 604/66, which sets the damages for wrongful dismissals. The details of the labor law during our sample period are discussed in Ichino (1996), Ichino, Polo and Rettore (2003) and Garibaldi and Violante (2005). Gianfreda and Vallanti (2017, 2020) use the 15 employees threshold interacted with the duration of labor disputes in the districts.

[11] To compute the number of employees that are relevant for the threshold, we exclude fixed-term contracts and managers as stated in Italian law (Art. 18 of Law 300/1970). Each part-time employee is supposed to be counted pro-quota based on the fraction of hours worked relative to FTE, but this information is missing in the early years of our sample. Accordingly, we follow the literature (see, e.g., Card and Krueger (1994)) and count each part-time worker as 1/2 of a full-time worker.

[12] However, the firm and the worker may bargain around this requirement after the judge's disposition. Social security data indicate that among wrongful termination cases where the plaintiff is successful, the worker is actually hired back by the firm only 5% of the times.

[13] This peculiar feature of Italian labor law was ended by the so-called "Fornero reform:" for cases filed after June 28, 2012, the penalties for wrongful termination in firms with more than 15 employees were brought into line with those for firms with 15 employees or less (see footnote 14). Unfortunately, our court data end too soon after the Fornero reform came into effect for us to evaluate the effect of this reform. In fact, to avoid right-censoring, we are forced to drop all the cases filed after the Fornero reform. Nonetheless, we repeated our main analysis separately for cases ending before and after June 28, 2012, and the results are similar, although imprecisely estimated because of the few cases ending after June 28, 2012 (see Table A.13 in the Online Appendix.)

mechanical dependence between trial duration and penalties for the guilty firm.[14]

The analysis links several data sources. Information on court cases comes from confidential data from the Court of First Instance in Rome, Italy. This is the largest labor court of first instance in Europe.[15] The data contains information on case characteristics (type, number of plaintiffs and defendants, date of assignment, date of disposition, judge identifier) and, remarkably, some identifiers for defendants and plaintiffs. The availability of firm identifiers allowed us to match court outcomes with the social security (INPS) data described below.[16]

Most dispositions take the form of a ruling (64%) or of a settlement between the parties (11%). The rest of the dispositions are cases where a party withdraws its claim, or where the suit cannot be adjudicated owing to factual or procedural issues that become known after filing, or because exceptional circumstances arise.

The court data were extracted in 2014 and cover cases filed between January 1, 2001 and July 1, 2014. Because our empirical strategy relies on measuring the date of *disposition* of these cases, which we do not know for the cases that were filed late in this time window, we drop all cases filed after December 31, 2011. This procedure ensures that the date of disposition is known for essentially all retained cases, and leaves us with 398,078 trials assigned to 95 judges. We call this the "baseline sample."

The Italian Social Security Administration (INPS) generously linked this dataset with its own social security data. INPS data are matched employer-employee data that record the working histories of the Italian population, as well as firm balance-sheet information, over the period 1999-2017.[17] We asked INPS to restrict the match to trials in which a firm

[14]Just for completeness, in the case of firms with 15 or fewer employees that are found guilty of wrongful termination, the termination is allowed to stand but the firm is required to pay damages in an amount that is fixed by law at a proportion of the worker's wage. Therefore, for these smaller firms that are excluded from our analysis, there is no mechanical dependence between trial duration and penalties for the guilty firm. Similarly, the statutory penalties for labor cases that are different from wrongful termination are also unaffected by trial duration and are therefore not included in our analysis sample.

[15]See https://www.repubblica.it/2007/01/sezioni/cronaca/bolzoni-tribunale/tribunale-seconda-puntata/tribunale-seconda-puntata.html

[16]The identifiers are somewhat incomplete: names are almost always present, but there is no systematic information about whether a name corresponds to a person, a firm, or a government agency. But, occasionally, social security numbers are available. The parties' names and, when available, their social security number, were used by INPS to link the two datasets.

[17]The data was made available through the Visitinps Scholars program. Firm balance-sheet information

is a party – either plaintiff or defendant.[18] INPS was able to identify 27,839 such trials. Some firms appear as a party in more than one trial: to deal with the multiple-treatment problem, following Bhuller et al. (2020) we keep firms at their first trial.[19] This restriction reduces the sample to 13,785 trials, and the same number of unique firms. We call this the "firm-restricted" sample. We use both samples, the baseline one (398,078 trials) and the firm-restricted one (13,785 trials), to test for the random assignment of cases to judges. Random assignment is not rejected in either sample: see Section 6.

Our final "estimation sample" further restricts to the 1,147 firms that are defendants in wrongful termination cases and that employ more than 15 employees. This is because our identifying variation, which is the variation in damages associated to trial duration, is only present for wrongful termination cases in firms above 15 employees.

A key advantage of social security data is that it allows us to measure outcomes at the month-firm level. We compute monthly turnover as the sum of hires and separations normalized by the number of employees. Note that this information is not available from balance sheet data, but can be constructed with the INPS data. Other dependent variables – value added, earnings before interest, taxes, depreciation, and amortization (EBITDA), sales, and investments – are provided at the yearly frequency by Cerved data, which is matched to INPS archives. In the Online Appendix, Table A.2 displays summary statistics for the relevant variables in our estimation sample, while Figure A.1 plots the distribution of the trial duration D_i.

4 Descriptive evidence

Figure 1 provides descriptive evidence of the slowdown in turnover, hiring, and separations, during the trial, and that these effects are stronger in firms that are randomly matched

is obtained from Cerved and linked by INPS. INPS archives contain only firms employing at least one employee, whereas Cerved contains only firms required to file financial statements. We keep firms that are both in INPS and Cerved. At the time INPS made the match, Cerved data were not available after 2017.

[18]Not all disputes involve a firm. For example, there are many cases between individuals and public administrations or other entities.

[19]Table A.1 reports the distribution of the total number of trials experienced by a firm in this sample. 68 percent of firms experience only one trial, 17 percent experience two trials.

with a slower judge. The vertical axis in each panel of the figure measures the outcome of interest: turnover (panels a, b), hiring rate (panels c, d), and separation rate (panels e, f).

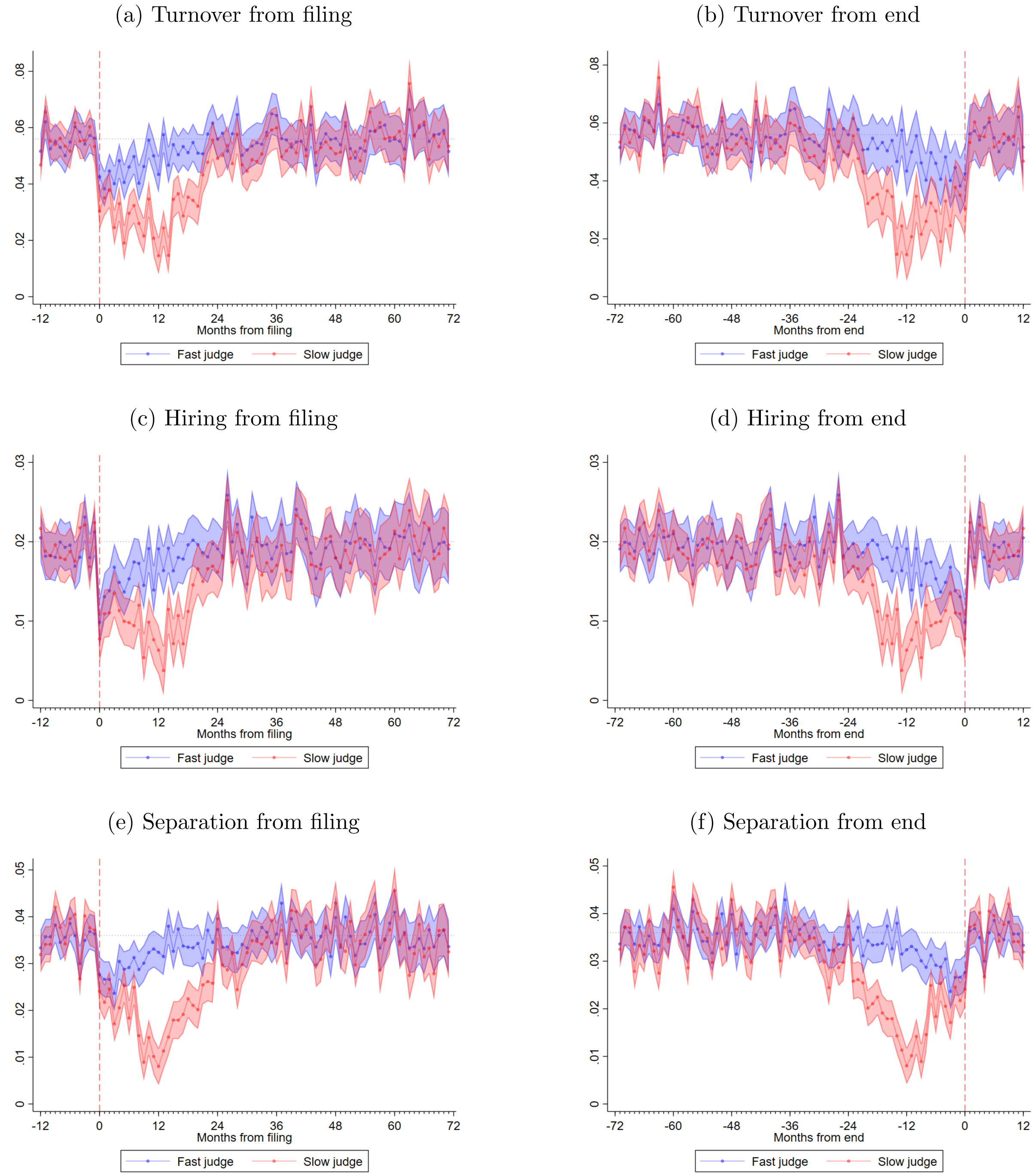
The blue and red lines represent the outcomes of interest for firms that were assigned to a "slow" or a "fast" judge, respectively. To code a firm i as being assigned to a slow or fast judge we first compute (in the baseline sample) the leave-out mean duration $\tilde{D}_{-i}$ as the average duration of all the cases assigned to the judge of case i during the calendar year in which case i is filed, excluding case i (this variable is later used as an instrument in our econometric framework; see Section 5). The case is coded as assigned to a slow judge if $\tilde{D}_{-i}$ exceeds the median leave-out mean duration across all judges, where the median is computed among all cases assigned in the same year as case i.

In the left-hand panels (a, c, e) of Figure 1, the zero on the x-axis (vertical dashed line) corresponds to the filing date, and the ticks represent months from filing date. These panels show that all firms experience a slowdown in turnover, hiring, and firing, during the trial, and that the effect is sharper in firms that are randomly matched with a slower judge, starting from the very first months after the trial is filed. The panels also indicate an absence of pre-trends regardless of whether firms are allocated to slow or fast judges.

In the right-hand panels (b, d, f) of Figure 1, the zero on the x-axis (vertical dashed line) corresponds to the disposition date, and the ticks represent months from that date. These panels show that, as soon as the trial ends, all firms return to the pre-trial levels of turnover, hiring, and firing, regardless of whether the firms were allocated to a slow or fast judge.

While the left-hand panels of Figure 1 can be interpreted causally because firms are randomly assigned to judges, the right-hand panels cannot because the duration of the trial need not be exogenous to the variables of interest. With this caveat, Figure 1 is consistent, at least *prima facie*, with Bernanke's (1983) option value theory: while subject to the uncertainty associated with the trial, firms are more cautious in hiring and firing; this effect disappears as soon as the uncertainty is resolved. The figure is also consistent with a risk-aversion based theory where firms are leery of taking on the additional risk entailed by hiring or firing a worker, while being subjected to litigation risk. We will

Figure 1: Turnover, hiring, and separation before, during, and after trial



Notes: The figure plots the monthly means of our workforce outcomes for firms assigned to a "slow" (red) or a "fast" (blue) judge, respectively. Panels a, c, and e consider firms' outcomes -12, to +72 months from the filing of trial i. Panels b, d, f consider firms' outcomes -72, to +12 months from the end of the trial i. To code a case/firm i as being assigned to a slow or fast judge we first compute (in the baseline sample) the leave out mean duration $\tilde{D}_{-i}$ as the average duration of all the cases assigned to the judge of case i during the calendar year in which case i is filed, excluding case i (this variable is later used as an instrument in our econometric framework; see Section 5). The case is coded as assigned to a slow judge if $\tilde{D}_i$ exceeds the median leave-out mean duration across all judges, where the median is computed among all cases assigned in the same year as case i. Blue and red shades indicate 95% confidence intervals computed using the monthly standard deviations.

confront these theories with our complete set of evidence in Section 9.

5 Digging deeper into the evidence

To dig deeper into the evidence presented in Figure 1, consider the following equation

$$Y_{i,t} = \beta_t \mathbb{D}_i + X_i \theta_t + \varepsilon_{i,t}, \tag{1}$$

that we would like to estimate separately for each period t (e.g., a month or a set of months), after the period $t_{0,i}$ in which case i is filed. $Y_{i,t}$ is an outcome variable (e.g., turnover, hiring, separations or value added) measured in period t for the firm involved in case i. X_i is a vector of time-invariant characteristics of the case. Let $t_{1,i}$ be the period in which the trial ends. The main regressor of interest, $\mathbb{D}_i$, is defined as:

$$\mathbb{D}_i = \begin{cases} E(D_i) & \text{if} \quad t_{0,i} < t < t_{1,i} \\ D_i & \text{if} \quad t_{1,i} \leq t \end{cases}. \tag{2}$$

In words, $\mathbb{D}_i$ is either the duration of trial i *expected* by the firm while the trial is ongoing – $E(D_i)$ – or the *actual* trial duration D_i that the firm observes after the case is decided by the judge or settled. We do not observe $E(D_i)$, but we assume that the firm has an unbiased expectation of the trial duration, for example based on information provided by the lawyer about the judge in charge of the case.[20] Therefore,

$$E(D_i) = D_i + \nu_i, \tag{3}$$

where ν_i is random noise. Substituting (2) and (3) in (1), the equation that we would like to estimate, separately in each period t, can be written as

$$Y_{i,t} = \beta_t D_i + X_i \theta_t + \eta_{i,t}, \tag{4}$$

where

$$\eta_{i,t} = \begin{cases} \varepsilon_{i,t} + \nu_i & \text{if} \quad t_{0,i} < t < t_{1,i} \\ \varepsilon_{i,t} & \text{if} \quad t_{1,i} \leq t \end{cases}.$$

The estimation of equation (4) is problematic because the unobservable characteristics of the trial and of the firm, captured by $\varepsilon_{i,t}$, are likely to be correlated with the actual

[20] At the end of Section 7 we provide evidence in support of this assumption.

trial duration D_i. So independently of whether (4) is estimated in a period before or after the end of the trial, the coefficient of interest β_t can be interpreted causally only if we can exploit for identification a source of exogenous variation of D_i. This source is offered by the random allocation of cases to judges in a context in which some judges are systematically slower than others. Exploiting these features we can use the average speed of a judge, in cases different than i, as a plausible instrument for trial duration D_i.

Specifically, a two-stage least squares (2SLS-LATE) estimation of β_t is achieved by pairing equation (4) with the following first-stage equation:

$$D_i = \gamma \tilde{D}_{-i} + X_i'\delta + \nu_i, \tag{5}$$

which is estimated once for all periods t. The instrument $\tilde{D}_{-i}$ has already been introduced in Section 4 and is computed as the average duration of all the cases assigned to the judge of case i during the calendar year in which case i is filed, excluding case i (leave-out mean).[21] The 2SLS-LATE estimate $\hat{\beta}_t$ represents the causal effect on period-t outcomes of one extra period of trial duration resulting from being randomly assigned to a slower judge.

Conveniently for estimation purposes, there is considerable variation in the instrument $\tilde{D}_{-i}$. The left-hand panel of the Figure A.2 in the Online Appendix plots the distribution of $\tilde{D}_{-i}$. Intuitively, variation in $\tilde{D}_{-i}$ can be interpreted as a measure of the variation in the speed of disposition of cases that is due just to the identity of judges. Indeed, since each judge is assigned around 460 cases per year, we expect each $\tilde{D}_{-i}$ to be approximately the same for all the cases of a given judge in a given year. Therefore, the dispersion in the left-hand panel of Figure A.2, is mostly a reflection of variations across judges. The histogram reveals a wide dispersion in speed across judges: the standard deviation is nine months, and a judge at the 90th percentile is about three-and-a-half times slower than a judge at the 10th percentile. The right-hand panel of Figure A.2 plots the residualized annual judge duration after conditioning on year of filing fixed effects. Though the distribution is compressed in the residualized figure, substantial variation remains, even within a year. This dispersion reveals the residual cross-judge difference in duration, which will be our

[21] The leave-out mean is computed in the "baseline sample" of 398,078 cases, using all past and future cases assigned to a given judge in a given year. In Table A.8 and A.9, we show robustness to alternative measures of $\tilde{D}_i$.

identifying variation. Following Abadie et al. (2017), we cluster standard errors at the judge level because this is the level at which the randomization takes place.

6 Validity of the identifications assumptions

Before presenting our results, in this section we first confirm empirically that the cases are randomly allocated to judges. Then, we discuss the assumptions that ensure the validity of the instrument $\tilde{D}_{-i}$ introduced in the previous sections.

6.1 Random allocation of cases to judges

We test for the random allocation of cases to judges using the following model:

$$\tilde{D}_{-i} = \zeta K_i + \eta A_i + \epsilon_i. \tag{6}$$

where A_i are year-of-assignment fixed effects and K_i are case characteristics. If cases are randomly allocated within a year, case characteristics K_i should not predict any judge characteristic, including average judge speed $\tilde{D}_{-i}$, so we expect ζ to equal zero. We estimate $\hat{\zeta}$ in two samples. First, in the "baseline sample" of all trials filed in the court (Table A.3 columns 1 and 2, in the Online Appendix), then in the "firm-restricted sample" of trials (Table A.3 columns 3 and 4, in the Online Appendix) in which at least one party, either plaintiff or defendant, is a firm that is linked to the social security archives. In both samples, the estimates $\hat{\zeta}$ are quantitatively small and only one is individually statistically significant at the 10% level, despite the variables K_i being highly predictive of case duration D_i (Tables A.4 and A.5, in the Online Appendix). This evidence confirms that cases are randomly assigned to judges, conditional on year of assignment.[22]

The power of the above tests would be reduced if firms involved in more than one trial were systematically more likely to be re-assigned to the same judge. In Figure A.3 in the Online Appendix) we report the p-values of the Chi-square tests of independence between the identities of the judges assigned to the first and second trial for firms that experience

[22]Coviello, Ichino and Persico (2019) have previously shown random allocation of cases to judges in the "baseline" sample.

at least two trials in the same month (top-left panel), same quarter (top-right panel), and same year (bottom-left panel). Since none of the p-values falls below 0.05, we conclude that judges are randomly assigned within the same firm across multiple litigations. This evidence further supports the presence of random assignment in our data.

6.2 Instrument relevance

Table 1 reports estimates of the first-stage equation (5) where we regress trial duration D_i on the instrument for judge speed $\tilde{D}_{-i}$, controlling for year of assignment, case type, number of parties, and lawyer characteristics, in the "baseline sample" ($N = 398{,}078$); and controlling for year of assignment, type of case, sector and age of the firm, in the "firm-restricted sample" ($N = 13{,}785$). In Panel A, we include year dummies but no other controls. The estimates are highly significant, suggesting that being assigned to a judge who is on average (leave-out mean) one month slower, increases trial duration by roughly 15 days.[23] Panel B of Table 1 controls for X_i. In both specifications, the F-statistics of the instrument are well above 10, indicating that $\tilde{D}_{-i}$ is a relevant predictor of trial duration D_i.

Incidentally, comparing Panels A and B of Table 1 provides an additional test of random assignment: if judges are randomly assigned, predetermined variables should not significantly change the estimates, because they should be uncorrelated with the instrument. As expected, the coefficient does not appreciably change when firm's characteristics and trial type controls are added in Panel B. This observation further supports our claim of random assignment.

6.3 Exclusion restrictions

Interpreting the IV estimates as measuring the causal effect of delayed justice requires an exclusion restriction: the judge should affect the firm's outcomes only through the trial duration channel and not directly in any other way. Our argument in favor of the exclusion restriction is greatly simplified by a key legal principle whereby the judge is required to

[23] By this calculation, being assigned to a judge who is one standard deviation (i.e., 14 months) slower increases trial duration by about 7 months, or about 43% of mean trial duration.

stay out of the entrepreneur's choices insofar as possible.[24] This principle means that the judge can only affect firm performance through two channels: *when* the decision is made (our preferred interpretation) and *what* the decision is (pro-plaintiff, or pro-defendant).

Table 1: First-stage: being assigned to a slow-judge

	Baseline sample (N = 398,078)	Firm-restricted sample (N = 13,785)
	(1)	(2)
	A. Year of Filing Fixed Effects	
Judge duration, $\tilde{D}_{-i}$	1.017***	0.528***
	(0.013)	(0.081)
F-statistic (instrument)	6,341	42
	B. Additional Controls	
Judge duration, $\tilde{D}_{-i}$	0.983***	0.525***
	(0.017)	(0.081)
F-statistic (instrument)	3,493	42
Dependent mean	19	17
Instrument mean	16	18

Notes: The table reports OLS estimates of trial duration (D_i) on $\tilde{D}_{-i}$. In Panel B, the additional controls are the type of case, the number of parties and the lawyers' characteristics in the "baseline sample"; and the type of case, the sector and age of the firm in the "firm-restricted sample". Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

To address the possibility that the second channel is confounding our estimates, we carry out two tests. First, we show that average judge speed $\tilde{D}_{-i}$ is uncorrelated with the probability that the judge to whom case i is assigned rules for the plaintiff.[25] For this purpose, a settlement is coded as a pro-defendant decision. This suggests that pro-plaintiff (or pro-defendant) bias is unlikely to confound the estimated effect of $\tilde{D}_{-i}$. The second test is, in spirit, a placebo test. It consists in repeating the analysis in Section 7 below, except using pro-plaintiff bias rather than judge speed as the explanatory variable. Specifically, we replace D_i with a dummy indicating that the plaintiff wins, and $\tilde{D}_{-i}$ with the leave-out-mean of the same dummy for the judge to whom case i is assigned. We find that pro-plaintiff bias has a small and statistically insignificant effect (see Tables A.6, and

[24]This is the principle of *insindacabilità delle scelte imprenditoriali* (entreprenurial choices cannot be questioned) established in Law n. 604 of July 15, 1966.

[25]We run model (1) using the same identification strategy as in Section 5, except that the outcome variable is a dummy indicating whether the plaintiff wins. We find a small and statistically insignificant effect (β =-0.001, standard error of 0.001).

A.7 in the Online Appendix). Overall, we believe that the case for the exclusion restriction is reasonably solid in our context.

6.4 Monotonicity

Imbens and Angrist (1994) show that a monotonicity assumption is required to identify local average treatment effects. Following Dobbie, Goldin and Yang (2018) and Bhuller et al. (2020), we implement two different tests to validate the monotonicity assumption in our setting.

The first testable implication of the monotonicity assumption is that the first-stage estimates should be non-negative in any sub-sample. For this test, we construct $\tilde{D}_{-i}$ using the full sample of available cases as before, but now we estimate the first stage on several sub-samples which are defined by the specific type of litigation, or by firm size. The results are reported in column 1 of Tables A.8 and A.9 in the Online Appendix. The first one of these table splits the sample by type of litigation; the second one, by firm characteristics. For all these sub-samples, the first-stage estimates are large, positive, and statistically different from zero, consistent with the monotonicity assumption.

The second testable implication of the monotonicity assumption is that judges should be slower for a specific case type (e.g., firing cases) if they are slower in other case types (e.g., all litigations except firing cases). To test this implication, we break the data into the same sub-samples as we did for the first test but, for each sub-sample, we compute $\tilde{D}_{-i}$ outside of the sub-sample. For example, for the firing cases sub-sample, we compute $\tilde{D}_{-i}$ on all cases except firing cases. Column 2 of Tables A.8 and A.9 in the Online Appendix lists the first-stage estimates using this reverse-sample instrument that excludes own-type cases. The first-stage estimates are all positive and statistically different from zero (the first-stage F-statistics obtained as the square of the t-tests are larger than 10), suggesting that judges who are slower for one case type are also slower for all case types.

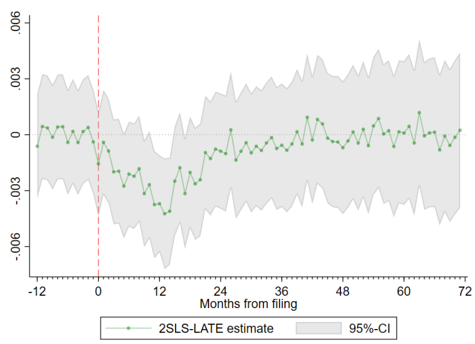
In sum, both tests fail to reject the monotonicity assumption.

7 Core findings

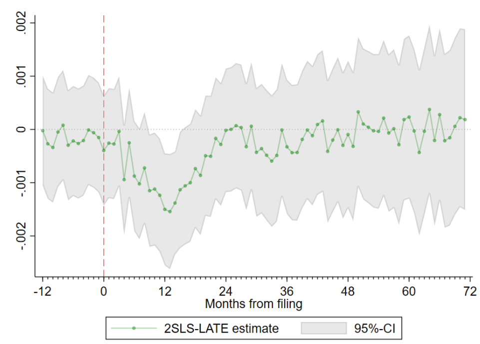
Figure 2 reports estimates of β_t in equation (4), obtained separately for each month t in the interval going from 12 months before the case is filed and 72 months after. The estimation sample consists of all firing litigations involving firms with more than 15 employees (see Section 3). The outcomes in the four panels are turnover, hiring, separation and employment. As expected, before the beginning of the trial the causal effect of an additional month of trial duration, induced by an assignment to a slower judge, is null on all outcomes. Immediately after the beginning of the trial, instead, there is clear evidence that the causal effect of an increase of trial duration is a reduction of turnover, hiring and separation. Employment is largely unaffected.

Figure 2: Dynamic effects on workforce management

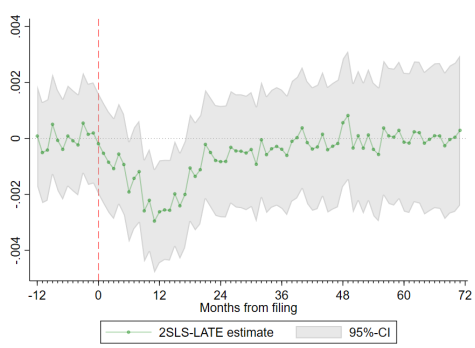
(a) Turnover



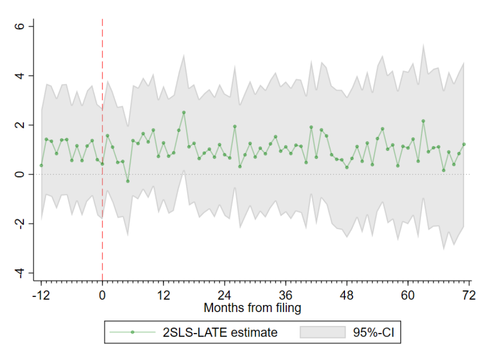
(b) Hiring



(c) Separation



(d) Employment



Note: The figure plots the 2SLS-LATE estimated coefficients obtained from month-by-month regressions of equation (1) [-12,72] months before and after the case was filed. The estimation sample consists of all firing litigations involving firms with more than 15 employees (see Section 3). *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. Gray shades indicate 95% confidence intervals.

With the passage of time after the filing of cases, the negative causal effect of trial duration first increases in absolute value and then decreases. After about 24 months from the beginning of the trial, when about 90% of the firms are done with the litigation, the causal effect goes back to being approximately null on all outcomes.

Table 2: Delayed justice reduces turnover, hiring and separations.

	sub-sample – firing trials (Employees > 15) (N = 1,147)			
Months from filing:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
Turnover	-0.003**	-0.003**	-0.0005	0.056
	(0.001)	(0.001)	(0.001)	
Hires	-0.001**	-0.001**	-0.0002	0.020
	(0.0004)	(0.0004)	(0.0004)	
Separations	-0.002**	-0.002**	-0.0003	0.036
	(0.0009)	(0.0009)	(0.0008)	
Employment	1.26	1.71	1.39	468
	(1.15)	(1.24)	(1.32)	

Notes: The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials D_i on the workforce outcomes Y_{it}, discussed in equation (1) and instrumented with the leave-out mean $\tilde{D}_{-i}$. In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from filing the case. *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration D_i is 15 and 14 months, respectively. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 24. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 reports similar results with a coarser time aggregation, and after standardizing variables in a way that allows for an easier quantitative assessment of the effects. Focusing on the first row, column 1 shows that within the first 12-month period from the case filing (i.e., while many firms are still in litigation), a 14-month increase in the duration of trials (one standard deviation) causes a decrease in turnover by 0.04 percentage points, or approximately 75%.[26] Column 2 replicates this estimate looking at turnover in the 13-24 month window after case filing and confirms the previous results, both qualitatively and

[26] 75%=14*0.03/0.056.

quantitatively. The effects essentially disappear in column 3, which looks at months 25-36 from filing (i.e., at a time when, as already mentioned, 90% of the firms are done with litigation).

Moreover, the absence of effects continues after the end of the trial, as shown in Table A.10 of the Online Appendix, which replicates the estimates of Table 2 for the first, second, and third twelve-month periods after the end of the trial. This indicates that, unlike Gianfreda and Vallanti (2017), we are not picking up anticipatory adjustments to future litigation, which makes sense because being assigned to a slow or fast judge does not predict the duration of any future lawsuit. Moreover, this null effect differentiate our results from those of Cahuc et al. (2021), who find post-trial differences.

The next rows in Table 2 break down the effect of trial duration on turnover into a hiring and a separation effect. We find that hirings and separations contribute essentially equally to the estimates of the first row: they are reduced by 70% and 78%, respectively. The final row in Table 2 shows that employment is not affected by trial duration: the estimated coefficient is quantitatively small and not statistically different from zero at all distances from filing.

The fact that after the beginning of the trial firms assigned to a slow judge begin immediately to behave differently than firms assigned to a fast judge indicates that the firm must be able to acquire some information about the speed of the judge to which it has been assigned. It is reasonable to suppose that this information comes from the firm's attorney. To assess if this hypothesis is supported by the data, we replicate Figure 2 in a split sample: high vs low experience firm attorney. Experience is measured by attorney age, or by the number of cases that a given attorney is seen to handle in our baseline sample. Regardless of how experience is measured, Figure A.4 in the Online Appendix is overall supportive of the hypothesis that firms which are represented by more-experienced attorneys are able to more-quickly latch on to the judge's speed. Indeed, we see that the effects on turnover, hiring, and separations, emerge more quickly in firms that are represented by more-experienced attorneys. Unfortunately, statistical power is insufficient to achieve statistically significant differences in outcomes at the monthly level. In addition, lawyers are chosen by the firms, so the figure does not admit a causal interpretation.

Nevertheless, we feel that Figure A.4 is somewhat persuasive of the hypothesis that firms acquire from lawyers the information on the speed of the judge to whom they are assigned.

8 Linking to the macro literature

The macro literature is concerned with a number of features of firm responses to uncertainty shocks, including: whether expectations about uncertainty or realizations of said uncertainty cause firms to respond; whether a more risky environment amplifies firm response; whether small or large firms are more responsive to uncertainty; and the effect of uncertainty on productivity and investment. This section examines these issues.

8.1 Judge ruling and firm actions when uncertainty is resolved

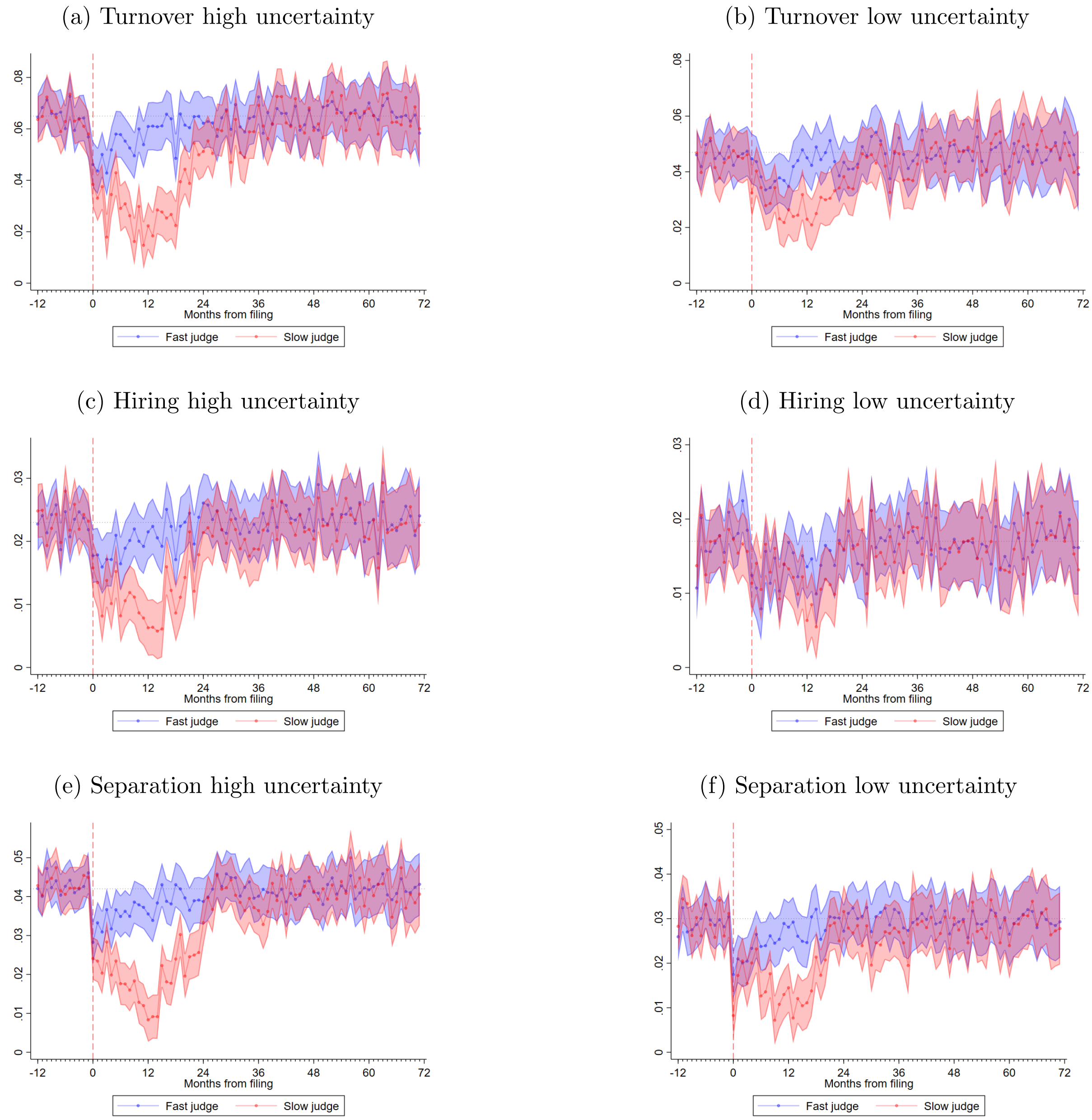
In our setting, being assigned to a systematically pro-worker judge is more likely to result in a worse ex-post realization from the firm's perspective. Fig A.5 in the Online Appendix shows that the ex-post realization does not make a difference: after the trial is over, firms go back to the pre-trial level of hiring and firing regardless of whether the decision they experienced was systematically better (because they were assigned to a pro-firm judge) or worse (because they were assigned to a pro-worker judge) for them.

One can read Figure A.5 as indicating that firms respond to uncertainty while it is ongoing, but they do not respond differentially after the uncertainty has been resolved. This observation may be interesting within the context of Berger et al.'s (2020) critique of the empirical literature that attributes to the firms' expectations about uncertainty those effects that are, instead, attributable to the firms' response to the realizations of said uncertainty. Figure A.5 does not seem to support Berger et al's (2020) critique.

8.2 Heterogeneity by background risk

A theory based on risk aversion on the firm's side suggests that firms which operate in riskier environments should be more wary of taking on additional risk and, therefore, to hire and fire.

Figure 3: Heterogeneous effects by background risk



Note: The figure plots the sample means of our workforce outcomes for firms that were assigned to "slow" (red) and "fast" (blue) judge, respectively. To code a case/firm i as being assigned to a slow or fast judge we first compute (in the baseline sample) the leave out mean duration $\tilde{D}_{-i}$ as the average duration of all the cases assigned to the judge of case i during the calendar year in which case i is filed, excluding case i (this variable is the instrument in our econometric framework; see Section 5). The case is coded as assigned to a slow judge if $\tilde{D}_i$ exceeds the median leave-out mean duration across all judges, where the median is computed among all cases assigned in the same year as case i. Panels a, c, and e (resp., b, d, f) consider cases assigned to firms operating in high uncertainty sectors (resp., low uncertainty). Uncertainty is measured using the Bank of Italy survey (INVID) that elicits business managers' subjective distribution over one-year-ahead sales growth and it is operationalized as the median difference between the minimum and maximum forecasted growth by each manager. *Turnover* is the sum of hires and separations normalized by the pre-trial workforce of the firm, e.g., the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separated normalized by the pre-trial number of workers of the firm, e.g., the number of employees in the 12 months before the trial starts. Gray shades indicate 95% confidence intervals computed using the cross-judges monthly standard deviation.

To test this prediction, we proxy for uncertainty using a survey which elicits business managers' subjective distribution over one-year-ahead sales growth. Risk is operationalized as the median difference between the minimum and maximum forecasted growth by each manager.[27] We classify sectors in which the average difference is larger than the median as "high background risk" sectors, the others as "low background risk." Figure 3 show that the effects of having longer trials arise mainly in firms operating in sectors with high background risk (left-hand panel); the effects are smaller and not statistically significant in sectors with low background risk (right-hand panel). Table A.11 in the Online Appendix lead to similar conclusions, by replicating the estimates of Table 2 after splitting the sample as in Figure 3.

8.3 Heterogeneity by firm size

We also split the sample by pre-trial level of employment. We call "small" those firms that employ at least 15 employees (which are therefore subject to the firing regulation) and no more than 50 (which is the median in the estimation sample). The rest we call "large firms." Table A.12 in the Online Appendix indicates that, indeed, the effects of having longer trials are mainly concentrated among small firms (Panel A); the effects are smaller and not statistically significant among large firms (Panel B).

8.4 Productivity and investment

When faced with additional risk such as that created by being randomly assigned to a slow judge, firms should be negatively affected. However, the slowdown in hiring and firing documented in Sections 4 and 7 is a coping mechanism to attenuate the negative impact of the uncertainty. Accordingly, we expect firms to be somewhat negatively affected, but not overwhelmingly so.

While the already commented Table 2 supports the first part of the above statement, Table 3 shows that our results are broadly consistent also with the second part. Specifically,

[27]Similar data has been used in Guiso and Parigi (1999) that look at the impact of uncertainty on investments. The properties of this measure of risk are discussed in Bontempi, Golinelli and Parigi (2010) and Fiori and Scoccianti (2021).

this Table indicates that a 14-month increase in the duration of trials (one standard deviation) causes a decrease in value added by 1,265 Euros, or approximately 1% with the effect being statistically significant at the 5% level. The table also indicates that duration of trials cause a reduction in EBITDA and sales, albeit not statistically significant. Overall, we interpret these estimates as evidence that firms are somewhat negatively affected, but not overwhelmingly so.

Table 3: Delayed justice reduces value added.

	sub-sample – firing trials (Employees > 15) (N = 1,147)			
Year from filing:	1 (1)	2 (2)	3 (3)	Dep.Var. mean (4)
Value added	-1,265** (575)	-1,145** (545)	-464 (580)	1,886K
EBITDA	-287 (179)	-268 (177)	-123 (176)	428K
Sales	-4,789 (3,192)	-4,976 (3,110)	-2,020 (3,108)	8,392K
Investments	-179 (336)	-269 (342)	-235 (359)	179K

Notes: The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials (D_i) on firms' balance sheet outcomes (Y_{it}) discussed in equation (1), and instrumented with the variable judge duration $\tilde{D}_{-i}$. In columns 1-3, the dependent variables are measured the 1^{st}, 2^{nd}, or the 3^{rd} year from filing the case as balance sheet outcomes are only available at the yearly level. *Value added* is the value added expressed in 2014 Euros. *EBITDA* are earnings before interest, taxes, depreciation, and amortization. *Sales* are the sales in 2014 euros. *Investments* are the investments in 2014 euros and are obtained as the change in the physical capital stock. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration (D_i) is 15 and 14 months, respectively. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 24. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Investment, in the last row of Table 3, is estimated to decrease but, again, this effect is not statistically significant. This finding could be explained by several non-mutually exclusive considerations. First, capital investment is famously lumpy. Second, we find that employment levels are not affected by differential expectations about trial duration, so if capital is in fixed proportion to labor, there would be no need to adjust investment levels as expectations about trial duration change. Third, our identification comes from a firm-specific shock: it is possible that investment responds to industry-level shocks more so than to firm-specific ones. Finally, investments are obtained as the change in the annual physical capital stock, as they are not directly observed in our balance sheet data, which introduces measurement error in our investment data.

9 Conceptual framework

This section presents two theories that can explain why and how trials affect firm labor choices. Section 9.1 establishes a preliminary result: that longer trials create more risk in the mathematical sense of stochastic dominance. In Section 9.2 we discuss option value theory. In Section 9.3 we discuss a theory based on risk aversion. In Section 9.4 we compare and contrast the two theories in light of the empirical findings of the preceding sections.

9.1 Slower judges create more risk

A worker (she) produces a positive revenue R and is paid a wage w. When $R - w < 0$, it makes economic sense for the firm to terminate the worker. However, the law restricts the circumstances under which termination is lawful. (For example, a worker may lawfully be terminated if R is low due to *unjustified* absences, but not due to *justified* ones.) The set of circumstances under which it is lawful to terminate a worker is ambiguous. A terminated employee may sue to get the termination to be ruled unlawful. Once a terminated worker sues for wrongful termination, she is randomly assigned to a judge. Judges vary systematically in the speed with which they reach a decision. With probability $(1 - p)$ the firm is found innocent and is required to pay nothing. With probability p the firm is found guilty. We assume that the legal interest rate and the firm's interest rate coincide and for simplicity of exposition, we assume that both are zero.

Let t_0 and t_1 be the months in which a trial start and ends, respectively. According to Italian law, if the firm has more than 15 employees, the penalty depends on trial duration. Indeed, in addition to hiring back the worker without prejudice, the guilty firm must disgorge the unpaid wages accrued during the trial. Thus, after a trial lasting $t_1 - t_0$ months, the guilty firm owes the worker $(t_1 - t_0)w$.[28] This means that the guilty firm prefers trials to be short: longer trials are more costly for the guilty firm.

Before knowing the judge's disposition, a firm that is sued for wrongful termination faces a binary random outcome. If found not guilty (probability $1 - p$), the firm pays out zero regardless of trial duration. If found guilty (probability p), the firm pays $(t_1 - t_0)w$. This random variable has mean $(t_1 - t_0)pw$ and variance $(t_1 - t_0)^2 p(1 - p)w^2$, both of which are increasing in the trial duration $(t_1 - t_0)$. Therefore we have proved the following result.

Proposition 1. ***(effect of duration on risk profile of trial outcome)*** *A firm with more than 15 employees that is assigned to a slower judge faces a random shock (trial outcome) with lower mean and higher variance, than a firm which is assigned to a faster judge.*

This proposition implies that for firms with more than 15 employees, longer trials result in a financial risk profile that is first- and second-order stochastically dominated by shorter trials. Proposition 1 implies that being assigned to a systematically slower judge has three effects: first, the trial lasts longer; second, expected damages are larger, in expectation; third, while the trial lasts, uncertainty, as measured by the variance of the future damages, is larger.

We conclude with a few observations about the model. First, we assumed that p, the probability that the firm is found guilty by judge j, is independent of judge j's speed. This assumption is supported empirically in Section 6. Second, we assumed that a wrongly terminated worker will be rehired, which is inefficient. However, it is not important that the worker be re-hired in equilibrium, provided that firm can bribe the worker to "go

[28] Technically, the obligation to pay back the wages arises because the termination is deemed null.

away;" that is, to give up the right to be re-hired in exchange for a suitable financial compensation.

9.2 Option value theory

Option value theory is mathematically complex. This section gives a simple setting in which the main implications can be understood with minimal effort.

Consider a firm that is exposed to a stochastic business environment. In period $t = 1, 2, ...$ the state of the business environment is $s = H, L$. In the "good" state H, it is optimal for the firm to employ l_H workers; in "bad" state L, it is optimal to employ $l_L < l_H$ workers. Let π denote the probability that the state s changes value from one period to the next. For π sufficiently close to zero, meaning that the state of the economy is very persistent and therefore uncertainty is low, if the firm is sufficiently patient, it is optimal for the firm to immediately adjust its employment level to l_s as soon as the state s changes, even if it must incur firing and hiring costs. This is because the costs are amortized over the long period of time, in expectation, during which the state stays the same. This setting, in our context, corresponds to the firm before or after the trial.

Now suppose the firm enters a temporary phase of elevated uncertainty starting at time t_0 (in our context, this would be the start of the trial). We model this elevated uncertainty by introducing a one-time event (which, in our context, would be the end of the trial) that happens at period t_1 and, in that period only, alters the transition probability as follows:

$$Pr[s_{t_1+1} = L | s_{t_1} = H] = p.$$

This equation means that, if the firm was in state H in period t_1, then with probability p it will find itself in state L in the following period. Other than this modification, the transition probability is governed by π.[29] The stopping time t_1 is stochastic. We assume that p is not close to zero, hence $p > \pi$, meaning that the negative shock is much more likely to occur than the "natural" transition of the economy from a high to a low state. Intuitively, this implies that between t_0 and t_1, uncertainty is elevated. In our context,

[29] For convenience, we assume that the firm does not experience any negative shock at time t_1 if it is already in state L. At the cost of adding a third element to the state space, i.e., a state worse than L, one could relax this assumption with little change to the model's predictions.

p represents the probability that the firm is found guilty, which we refer to as a negative shock.

For any $(t_1 - t_0)$, if the hiring and firing costs are sufficiently high, the firm will not expand its workforce during the phase of elevated uncertainty, that is, for t between t_0 and t_1. This is because, even if the state transitions from L to H during that phase is possible, there is an high probability of transitioning back to L soon, so paying the hiring (and subsequent firing) costs does not make sense. Hence, option value theory predicts that, during a temporary phase of elevated uncertainty, firms will hold off on adjusting production factors. The logic for this response is to avoid repeatedly paying adjustment costs.

Option value theory predicts that increasing uncertainty increases the returns to inaction. In our context, option value theory yields several sharp predictions. First, since the uncertainty has negative mean (elevated probability of going from a high to a low state, but not vice versa), upward adjustment is stifled (from l_L to l_H), but not lower adjustment.

Second, as soon as the uncertainty is resolved, all the firms resume their "normal" adaptation of employment to state. This means that all the firms that had transitioned from L to H between t_0 and t_1, but were afraid to adjust their employment upward for fear of being hit by the shock, immediately increase their employment at time $t_1 + 1$.

The third prediction is that firms that were hit by the negative shock find themselves in state L at time $t_1 + 1$, i.e., after the uncertainty is resolved. These firms start out from a lower employment level than firms that were not hit by the shock. These observations are formally stated in the following proposition.

Proposition 2. ***(effect of trial length within option value theory)*** *Suppose the firm's hiring decision are governed by option value theory. Then, while the trial is ongoing, upward labor adjustments may be reduced, but not downward ones. As soon as uncertainty is resolved, firms that were found innocent are expected to increase employment more, and to quickly achieve a higher employment level, than those that were found guilty.*

9.3 Risk aversion theory

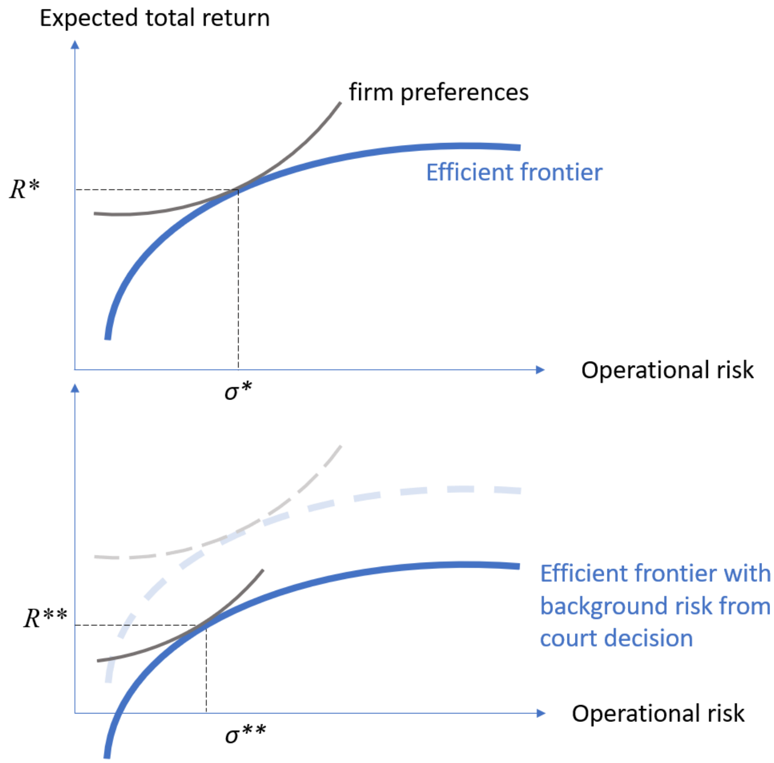
Proposition 1 implies that, in firms with more than 15 employees, longer trials result in a financial risk profile that is first- and second-order stochastically dominated by shorter trials. In the language of Gollier and Pratt (1996), a firm that is randomly assigned to a slower judge is subjected to an increase in unfair background risk. This language means that in a context where the firm faces a decision problem unrelated to the litigation, the trial outcome creates a larger actuarially unfair financial risk that is "in the background" of the firm's decision problem, but that affects the firm's choice in the decision problem.

What does the presence of litigation risk imply for the firm's operational choices? We conceptualize the firm's operational choices as a decision problem amounting to choosing a point on a risk-return frontier. Under a condition on the curvature of the payoff function that Gollier and Pratt (1996) call "risk vulnerability", adding unfair background risk reduces the firm's appetite for other sources of risk, including risk from ordinary operations in our context. This implies that a firm that is exposed to a longer trial will want to reduce risk in its operational choices and, in exchange, accept lower expected returns. One of the ways that the firm can reduce risk is to reduce worker turnover, because terminating another worker creates legal risk. More generally, we expect the firm to become more cautious along many other unobserved dimensions. This reduced appetite for risk induced by greater background risk causes the firm to accept lower expected productivity, as shown in Figure 4. These observations are formally stated in the following proposition.

Proposition 3. ***(effect of trial length within risk aversion theory)*** *Suppose the firm's risk preferences are risk vulnerable in the sense of Gollier and Pratt (1996). A firm that is exposed to a longer trial will want to reduce the riskiness of its operations (including the legal risk coming from worker turnover) and will accept lower expected productivity from operations.*

The reader might object to the assumption that the firm's preference exhibit risk aversion, especially given limited liability in case of bankruptcy. In the Italian context, however, more than 50% of all firms are controlled by individual persons, as opposed to corporations or holding companies, and this percentage exceeds 75% for firms with fewer

Figure 4: Effect of trial duration on operational risk taking and operational productivity.



Note: The top panel illustrates the trade-off between operational risk σ and expected return R for a firm that is not exposed to risk from a trial. The firm's optimal choice is (σ^*, R^*). The firm in the bottom panel is exposed to trial risk; therefore, it faces a less favorable efficient frontier than the firm in the top panel and, given the preferences depicted in the figure, chooses less risk and less expected return than (σ^{**}, R^*).

than 50 employees, which is the sample where most of our effect comes from (see Section 8.3).[30] It is plausible that firms inherit their controlling shareholder's risk attitudes, especially for relatively small risks which are unlikely to bring about bankruptcy, such as the ones we leverage.

9.4 Option value theory vs risk aversion theory

During trial, the left-hand panels of Figure 1, and Figure 2 show that firms slow down hiring and separations, and they do this more sharply when the trial lasts longer. A theory based on risk aversion predicts these patterns (Proposition 3). Option value theory predicts most of these patterns, except that separations are not expected to slow down during trial: see Proposition 2.

[30] See for example Table 15 and Table 16 in Giacomelli and Trento (2005).

After the trial, risk aversion theory predicts that whatever monetary costs the firm has incurred due to the trial are now sunk. Therefore, behavior going forward should return to normal irrespective of whether the firm won or lost the trial. Option value theory, instead, predicts a rich set of patterns depending on whether the firm won or lost the trial (Proposition 2), that don't seem to find a counterpart in the post-trial phase of Figure A.5.

In addition, risk aversion theory predicts that greater exposure to background risk should magnify the effects of being exposed to trial risk, consistent with the findings in Section 8.2. Option value theory does not yield such a prediction.

Overall, then, we believe that option value theory fits the available evidence less well than a theory based on the firm's aversion to risk.

10 Conclusions

For the first time to our knowledge, this paper measures the causal effects of exogenous idiosyncratic changes to business uncertainty expectations on firm behavior. We show that litigation-generated quasi-experimental variation in firm-level business risk causes variation in workforce management. Specifically, we leverage the fact that a firm that is being sued faces the risk of being found guilty and penalized monetarily. The larger the expected penalty, the larger the present litigation risk for the firm (business risk). We find that exposure to a larger business risk causes employee turnover, hiring, and firing to decrease sizeably.

The effect we measure comes from the risk created by the present litigation, and not from precautionary measures directed at preventing more-likely future litigation. We compare two theories based on their ability to accommodate the above facts: option value theory, whereby firms minimize adjustment costs by putting turnover on hold until the uncertainty is resolved; and a theory whereby firms seek to minimize the additional risk created by turnover, while the uncertainty is resolved. We conclude that the latter theory seems to be more consistent with our findings.

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Online Appendix A

Table A.1: Number of trials per firm

Firms	Number of trials	Cumulative share(%)
9,396	1	68
2,281	2	85
821	3	91
384	4	94
209	5	96
134	6	97
560	>6	100

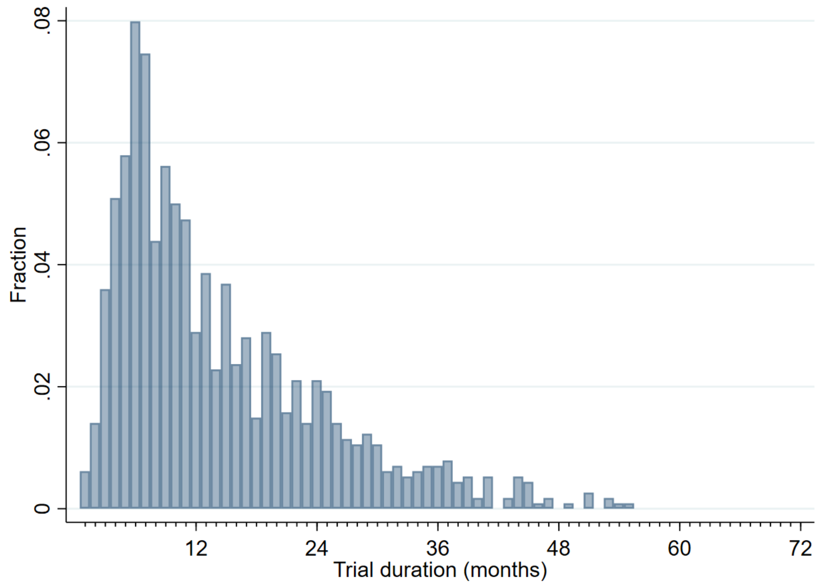
Notes: The table reports the distribution of the number of trials for each firm. For example, the first row says that there are 9,396 firms that have only one trial in the years considered in our sample (2000-2012). Total of 13,785 firms (27,839 trials).

Table A.2: Summary statistics for the estimation sample

	Estimations sample N=1,147	
Variable	*Mean*	*St dev*
Turnover	0.056	0.078
Hires	0.020	0.037
Separations	0.036	0.047
Employment	468	2,996
Value added	1,886	5,492
EBITDA	428	1,789
Sales	8,392	25,648
Investments	179	1,419
Trial duration, D_i	15	14
Judge duration, $\tilde{D}_{-i}$	15	9

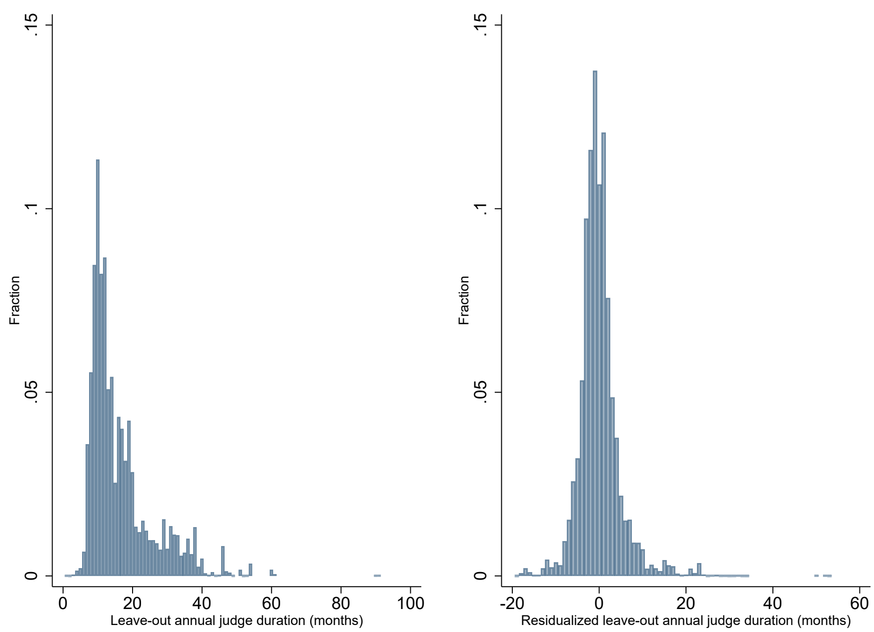
Notes: Turnover, hires, separations, and employment are the averages in the twelve months before the case was filed. Turnover, hires, and separations are normalized by employment. Value added, EBITDA, sales, and investments are measured in the year before the case was filed; these variables are expressed in (thousands) 2014 Euros. "Trial duration" (D_i) is the average duration (measured in months) experienced by the 1,147 firms in the estimation sample. "Judge duration" ($\tilde{D}_{-i}$) is the average duration, computed using the baseline sample of 398,078, of all the cases assigned to the judge of case i during the calendar year in which case i is filed, excluding case i (this variable is the instrument used in our econometric framework; see Section 5).

Figure A.1: Duration of Trials D_i



Notes: The figure plots the distribution of the duration of trials D_i in the estimation sample.

Figure A.2: Distribution of the instrument $\tilde{D}_{-i}$



Notes: The figure plots the distribution of the instrument (leave-out mean duration) $\tilde{D}_{-i}$, which for each case i is computed as the mean duration of cases assigned to that judge during the calendar year in which case i was filed, excluding case i. The left panel plots the raw data, while the right panel plots the distribution of the residuals obtained from the regression of $\tilde{D}_{-i}$ on year of filing fixed effects. A bin is a month.

Table A.3: Tests for random assignment

	Baseline sample (N = 398,078)		Firm-restricted sample (N = 13,785)	
	Coefficient Estimate (1)	Standard Error (2)	Coefficient Estimate (3)	Standard Error (4)
Type of litigation:				
Firing	.147	.156	-.185	.239
Compensation	.501*	.202	.0475	.252
Allowance	-.066	.140	.	.
Pension	.006	.165	.	.
Temp. Contracts	.137	.166	.	.
Qualification	.224	.182	.	.
Other type I	.160	.155	.	.
Other type II	.167	.163	.	.
Other type III	.041	.138	.	.
Number of parties	-.025	.017	.	.
Plaintiff lawyer born in Rome	-.061	.047	.	.
Defendant lawyer born in Rome	-.024	.109	.	.
Age plaintiff lawyer	-.004	.002	.	.
Age defendant lawyer	-.002	.004	.	.
Plaintiff lawyer female	.013	.067	.	.
Defendant lawyer female	-.081	.089	.	.
Sector of the firm:				
Manufacturing	.	.	-.320*	.187
Services	.	.	-.154	.154
Hires	.	.	-.0001	.0002
Separations	.	.	-.0003	.0003
Weekly wages	.	.	-.00001	.00001
Age of the firm	.	.	.002	.003
Value added	.	.	.00001	.00002
Labor	.	.	.00001	.00003
Capital	.	.	.00003	.0002

Notes: The table reports the results of the randomization test in the "baseline sample" and in the "firm-restricted sample". The outcome variable is the judge's speed. Firm level variables, such as the sector of the firm or its size are not present on the "baseline sample" because they are measured only using INPS archives. All cases different from firing and compensation are aggregated in the "firm-restricted sample" for statistical convenience because there are very few observations in each category. Other variables, such as the age of the lawyers are not present in the "firm-restricted sample" because the variables were deleted for data anonymization purposes. All estimations include year of filing fixed effects. The omitted category for type of case is "disability" in the "baseline sample" and "other types of cases" in the "firm-restricted sample". Mean judge duration is 16 months and standard deviation is 9 months in the "baseline sample", and 18 and 9 months in the "firm-restricted sample". Standard errors are clustered at the judge level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4: Random assignment: "baseline sample" (N = 398,078)

	Dependent Variable				Explanatory Variable	
	Trial duration		Judge duration			
	Coefficient Estimate (1)	Standard Error (2)	Coefficient Estimate (3)	Standard Error (4)	Mean (5)	Standard Deviation (6)
Type of litigation:						
Firing	4.203***	.682	.147	.156	.0766	.266
Compensation	11.828***	.925	.501*	.202	.2	.4
Allowance	.710	.610	-.066	.140	.193	.395
Pension	.714	.632	.006	.165	.045	.207
Temp. Contracts	3.986***	.703	.137	.166	.045	.207
Qualification	6.746***	.684	.224	.182	.0205	.142
Other type I	7.332***	.616	.160	.155	.185	.388
Other type II	8.203***	.825	.167	.163	.115	.319
Other type III	2.822***	.682	.041	.138	.0871	.282
Number of parties	-.763***	.133	-.025	.017	2.59	1.06
Plaintiff lawyer born in Rome	.106	.310	-.061	.047	.604	.489
Defendant lawyer born in Rome	-1.135***	.308	-.024	.109	.434	.496
Age plaintiff lawyer	-.048**	.015	-.004	.002	46.6	11.8
Age defendant lawyer	-.045***	.012	-.002	.004	44.6	12.2
Plaintiff lawyer female	.222	.376	.013	.067	.29	.454
Defendant lawyer female	-.651*	.327	-.081	.089	.352	.478
F-statistic for joint test	46.17		3.75			
p-value	.000		.000			

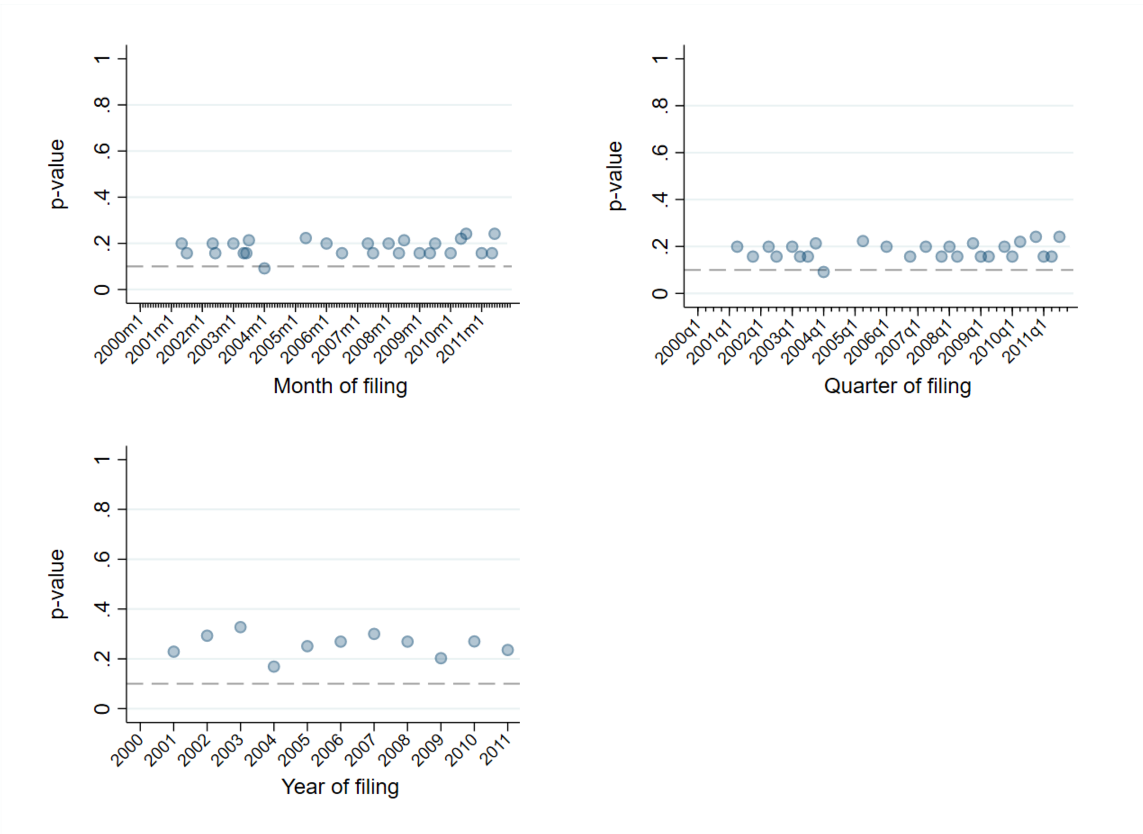
Notes: The tale reports estimated coefficients (standard errors) of the randomization tests. The sample includes all cases filed in the labor court of Rome in 2000–11. All estimations include year fixed effects. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for type of case is "disability". Mean trial duration 18.69, judge duration is 16.39 months. Standard deviation trial duration 24.92, judge duration is 9.23 months. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Random assignment: "firm-restricted sample" (N = 13,785)

	Dependent Variable				Explanatory Variable	
	Trial duration		Judge duration			
	Coefficient Estimate (1)	Standard Error (2)	Coefficient Estimate (3)	Standard Error (4)	Mean (5)	Standard Deviation (6)
Type of litigation:						
Firing	-1.366***	.470	-.185	.239	.264	.4408
Compensation	2.004***	.630	.0475	.252	.2817	.4498
Sector of the firm:						
Manufacturing	.0248	.588	-.320*	.187	.1211	.3262
Services	.0957	.463	-.154	.154	.769	.4215
Hires	-.00248	.00202	-.000153	.000254	8.8	151
Separations	.00294	.00217	-.000270	.000298	7.6	144
Weekly wages	-.000299**	.000138	-.00000499	.0000367	483	583
Age of the firm	-.00542	.0127	.00179	.00342	13	12
Value added	.0000101	.000013	.0000106	.000017	1,295K	6,176K
Employment	.0000311	.000113	.0000129	.0000311	213	1,821
Capital	.0000231	.000182	.0000261	.000186	701K	3,551K
F-statistic for joint test	8.984		1.394			
p-value	.000		.202			

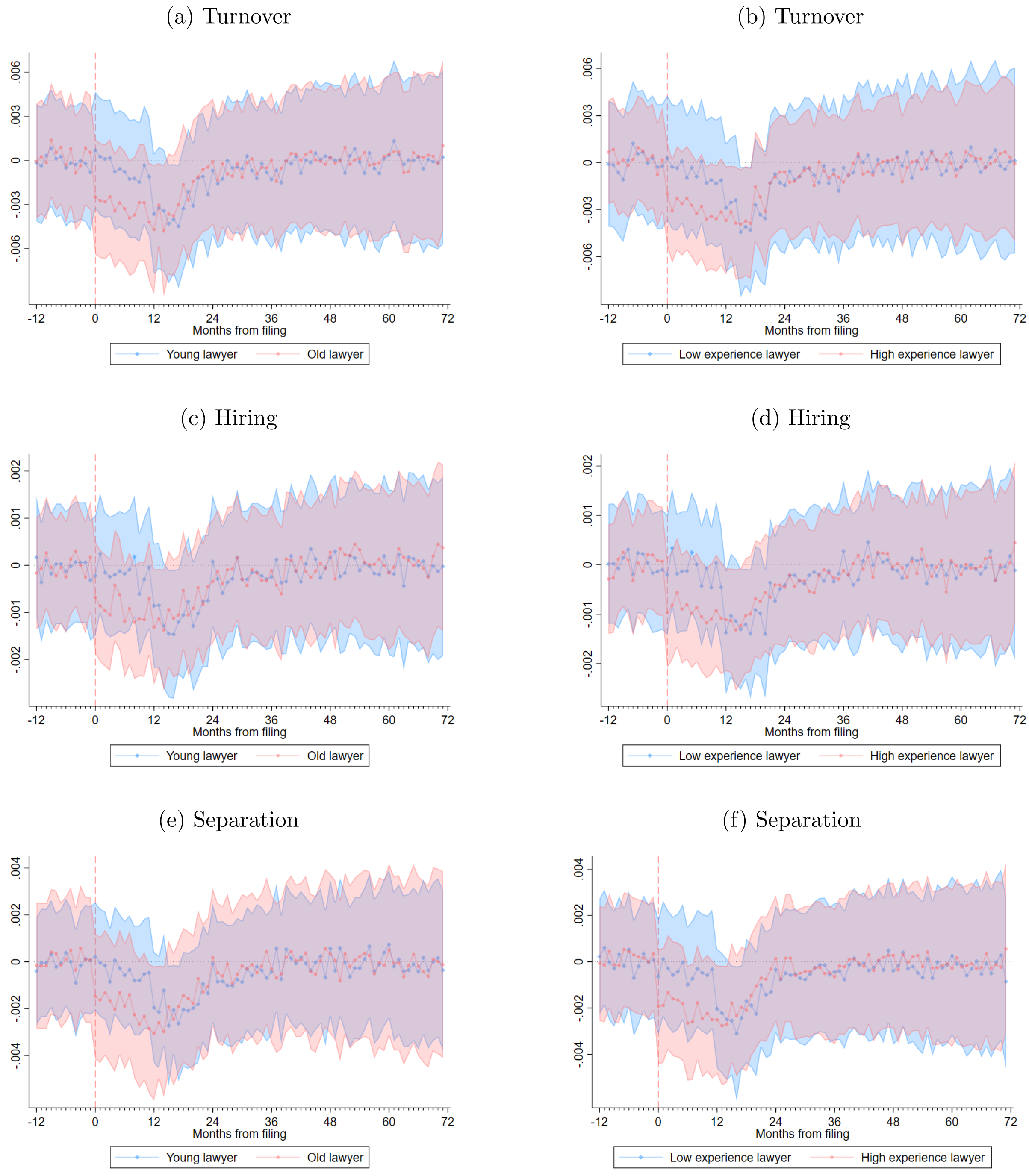
Notes: The tale reports estimated coefficients (standard errors) of the randomization tests. The sample includes the firms going the labor court of Rome in 2000–11. All estimations include year fixed effects. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for type of case is "other cases", grouping all cases different from "firing" and "compensation". The omitted category for sector of the firm is "constructions". Mean trial duration 17.34, judge duration is 17.74 months. Standard deviation trial duration 17.9, judge duration is 8.91 months. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Figure A.3: Random assignment for firms involved in multiple litigation procedures



Note: The figure reports the *p-values* of the Chi-square tests of independence between the identity of judges assigned to cases in the first trial and the identity of judges assigned in the second trial in the sample of firm that have at least two trials filed to the court the same month (top-left panel), same quarter (top-right panel), and same year (bottom-left panel). Dashed lines represent 10% significance levels.

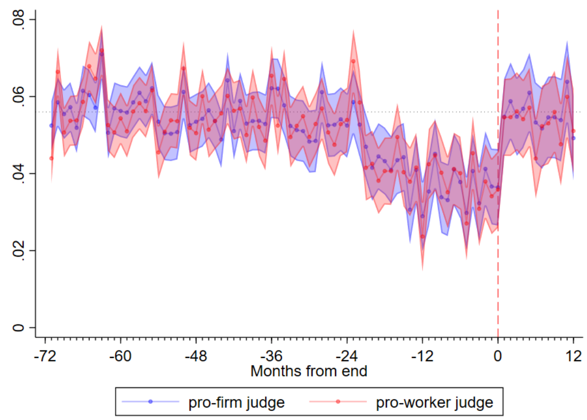
Figure A.4: Dynamic effects on workforce management for different level of experience of the firm's lawyer



Note: The figure plots the 2SLS-LATE estimated coefficients obtained from month-by-month regressions of equation 1 [-12,72] months before and after the case was filed. The estimation sample is split by the median value of experience of the defendant attorney (the one representing the firm). Experience is measured by attorney age (panels a, c, and e), or by the number of cases that a given attorney is seen to handle in our baseline sample (panels b, d, and f). *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separated normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. Shades indicate 95% confidence intervals.

Figure A.5: Pro-firm vs pro-worker judges

(a) Turnover from end



(b) Hiring from end

(c) Separation from end

Note: The figure plots the monthly means of our workforce outcomes for firms assigned to "pro-worker" (red) and "pro-firm" (blue) judges, -72, to +12 months from the end of the trial. To code a case/firm i as being assigned to a pro-worker or pro-firm judge we first compute (in the baseline sample) the leave-out mean of judge decisions in favor of the worker during the calendar year in which case i is filed, excluding case i. The case is coded as assigned to a pro-worker judge if it exceeds the median leave-out mean across all judges, where the median is computed among all cases assigned in the same year as case i. Blue and red shades indicate 95% confidence intervals computed using the monthly standard deviations.

Table A.6: Losing a litigation does not affect turnover, hiring, and separations – 2SLS-LATE estimates.

	sub-sample – firing trials (Employees > 15) (N = 975)			
Months from disposition:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
Turnover	0.003 (0.004)	0.003 (0.004)	0.003 (0.004)	0.056
Hires	0.002 (0.003)	0.002 (0.003)	0.002 (0.003)	0.020
Separations	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.036
Employment	-2.65 (3.56)	-2.46 (3.09)	-2.87 (3.37)	468

Notes: The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the case being decided in favor of a worker discussed in equation (1), and instrumented with the case's leave-out mean of judges decisions pro-worker. In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from disposition of the case. *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. The mean and standard deviation of the treatment (average ruling in favor of the worker) is 0.22 and 0.42, respectively. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 26. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Being assigned to a pro-worker judge does not affect turnover, hiring, and separations – reduced form estimates.

	sub-sample – firing trials (Employees > 15) ($N = 975$)			
Months from disposition:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
Turnover	0.0023	0.0023	0.0023	0.056
	(0.0032)	(0.0032)	(0.0032)	
Hires	0.0015	0.0015	0.0015	0.020
	(0.0024)	(0.0024)	(0.0024)	
Separations	0.0009	0.0009	0.0009	0.036
	(0.0017)	(0.0017)	(0.0017)	
Employment	-2.22	-1.99	-2.31	468
	(2.85)	(2.47)	(2.69)	

Notes: The table reports the reduced-form estimates coefficients (and standard errors) of being assigned to a pro-worker judge, which is measured with the case's leave-out mean of judges decisions pro-worker. In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from disposition of the case. *Turnover* is the sum of hires and separations normalized by the pre-trial workforce of the firm, e.g., the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the pre-trial number of workers of the firm, e.g., the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. The mean and standard deviation of the treatment (average judge ruling in favor of the worker) is 0.30 and 0.11, respectively. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Tests for the monotonicity assumption: characteristics of the litigation

	Baseline Instrument	Reverse-sample Instrument
	First stage	First stage
	(1)	(2)
1. Other type I		
Judge duration	1.021***	0.532***
SE	(0.0958)	(0.0880)
Observations	73,667	73,587
Judge duration mean	18	18
Trial duration mean	14	16
2. Other type II		
Judge duration	1.309***	0.720***
SE	(0.115)	(0.142)
Observations	45,777	45,729
Judge duration mean	20	20
Trial duration mean	16	18
3. Other type III		
Judge duration	0.820***	0.503***
SE	(0.139)	(0.102)
Observations	34,662	34,653
Judge duration mean	19	22
Trial duration mean	19	19
4. Compensation		
Judge duration	1.417***	0.757***
SE	(0.0813)	(0.129)
Observations	79,642	79,453
Judge duration mean	18	18
Trial duration mean	26	26
5. Allowance		
Judge duration	0.403***	0.256***
SE	(0.0439)	(0.0413)
Observations	76,760	76,732
Judge duration mean	15	19
Trial duration mean	13	13
6. Firing		
Judge duration	0.546***	0.433***
SE	(0.0784)	(0.0673)
Observations	30,492	30,482
Judge duration mean	17	19
Trial duration mean	17	17
7. Pension		
Judge duration	0.429***	0.354***
SE	(0.0467)	(0.0470)
Observations	17,920	17,918
Judge duration mean	16	19
Trial duration mean	13	13
8. Temp. Contracts		
Judge duration	0.947***	0.804***
SE	(0.108)	(0.0986)
Observations	17,910	17,909
Judge duration mean	13	16
Trial duration mean	14	14
9. Disability		
Judge duration	0.709***	0.456***
SE	(0.136)	(0.104)
Observations	13,103	13,103
Judge duration mean	24	27
Trial duration mean	20	20
10. Qualification		
Judge duration	0.848***	0.772***
SE	(0.0522)	(0.0560)
Observations	8,145	8,144
Judge duration mean	13	15
Trial duration mean	16	16

Notes: Column (1) of the table reports OLS estimates of trial duration (D_i) on $\tilde{D}_{-i}$, where $\tilde{D}_{-i}$ is constructed in the "baseline sample" of 398,078 cases, but the first-stages are estimated on different sub-samples defined by the specific type of litigation, which is reported in each row title of the table. For example, column (1) in panel 6 reports first-stage results only in the sub-sample of firing trials. In column (2), for each sub-sample of type of litigation, $\tilde{D}_{-i}$ is computed outside of the sub-sample. For example, for the firing cases sub-sample, we compute $\tilde{D}_{-i}$ on all cases except firing cases but estimate the first-stage regression for firing cases. Each estimate running equation 5 and controlling for the number of parties and the lawyers' characteristics. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Tests for the monotonicity assumption: characteristics of the firm

	Baseline Instrument	Reverse-sample Instrument
	First stage	First stage
	(1)	(2)
1. Firm size above median		
Judge duration	0.515***	0.511***
SE	(0.0819)	(0.112)
Observations	6,888	3,874
Judge duration mean	18	18
Trial duration mean	17	17
2. Firm size below median		
Judge duration	0.591***	0.480***
SE	(0.0713)	(0.0800)
Observations	6,897	3,765
Judge duration mean	18	16
Trial duration mean	18	18
3. Firm wages above median		
Judge duration	0.529***	0.462***
SE	(0.0769)	(0.0706)
Observations	6,892	3,838
Judge duration mean	18	17
Trial duration mean	17	17
4. Firm wages below median		
Judge duration	0.576***	0.564***
SE	(0.0676)	(0.0656)
Observations	6,893	3,721
Judge duration mean	18	17
Trial duration mean	17	17
5. Firm turnover above median		
Judge duration	0.572***	0.460***
SE	(0.0812)	(0.0797)
Observations	7,066	3,698
Judge duration mean	18	16
Trial duration mean	18	18
6. Firm turnover below median		
Judge duration	0.535***	0.403***
SE	(0.0634)	(0.0850)
Observations	6,719	3,718
Judge duration mean	18	17
Trial duration mean	17	17

Notes: Column (1) of the table reports OLS estimates of trial duration (D_i) on $\tilde{D}_{-i}$, where $\tilde{D}_{-i}$ is constructed in the "baseline sample" of 398,078 cases, but the first-stages are estimated on different sub-samples defined by the firm's characteristic reported in each row title of the table. For example, column (1) in panel 1 reports first-stage results only in the sub-sample of firms employing a number of employees above the median (large firms). In column (2), for each sub-sample defined by the firm's characteristic, $\tilde{D}_{-i}$ is computed outside of the sub-sample. For example, for the large firm sub-sample, we compute $\tilde{D}_{-i}$ on all cases except those of large firms but estimate the first-stage regression in the sub-sample of large firms. Each estimate running equation 5 and controlling for the type of case, the sector and age of the firm. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: After the end of the trial – 2SLS-LATE estimates.

	sub-sample – firing trials (Employees > 15) ($N = 975$)			
Months from disposition:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
Turnover	-0.0004 (0.0015)	-0.0004 (0.0016)	-0.0004 (0.0018)	0.056
Hires	-0.0001 (0.0005)	-0.0001 (0.0005)	-0.0001 (0.0006)	0.020
Separations	-0.0002 (0.0008)	-0.0002 (0.0009)	-0.0002 (0.0009)	0.036
Employment	1.68 (1.52)	1.78 (1.45)	1.56 (1.61)	468

Notes: The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials (D_i) on workforce outcomes (Y_{it}) discussed in equation (1), and instrumented with the variable judge duration ($\tilde{D}_{-i}$) after the end of the trial. In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from disposition of the case. *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration (D_i) is 15 and 14 months, respectively. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 22. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Heterogenous effects by ex-ante uncertainty – 2SLS-LATE estimates.

Months from filing:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
	Panel A: firms in high uncertainty sectors ($N = 574$)			
Value added	-1,807* (1,045)	-1,799 (1,098)	-669 (1,119)	1,697K
Turnover	-0.004* (0.0022)	-0.004* (0.0022)	-0.0007 (0.0022)	0.065
Hires	-0.0014* (0.0008)	-0.0014* (0.0008)	-0.0003 (0.0008)	0.023
Separations	-0.0026* (0.0014)	-0.0026* (0.0014)	-0.0004 (0.0014)	0.042
Employment	1.13 (2.31)	1.18 (2.19)	1.35 (2.45)	374
	Panel B: firms in low uncertainty sectors ($N = 573$)			
Value added	-885 (1,021)	-909 (1,109)	-798 (1,043)	2,075K
Turnover	-0.002 (0.0023)	-0.002 (0.0023)	-0.002 (0.0023)	0.047
Hires	-0.0004 (0.0009)	-0.0004 (0.0009)	-0.0004 (0.0009)	0.017
Separations	-0.0016 (0.0016)	-0.0016 (0.0016)	-0.0016 (0.0016)	0.030
Employment	1.26 (2.31)	1.33 (2.09)	1.39 (2.15)	562

Notes: The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials (D_i) on workforce outcomes (Y_{it}) discussed in equation (1), and instrumented with the variable judge duration ($\tilde{D}_{-i}$). In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from filing the case. Panel A (resp., Panel B) considers firms operating in high (resp., low) uncertainty sectors. Uncertainty is measured at the sectoral level using the INVID data. Firms are split using the median difference in managers' expectations over the minimum and the maximum one-year ahead growth rates of sales. *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 16 in the high uncertainty sub-sample and 18 in the low uncertainty sub-sample. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Heterogenous effects by firm size – 2SLS-LATE estimates.

Months from filing:	1-12	13-24	25-36	Dep.Var. mean
	(1)	(2)	(3)	(4)
	Panel A: small firms employing > 15 and ≤ 50 employees ($N = 574$)			
Value added	-2,375*	-2,167*	-1,245	444K
	(1,397)	(1,275)	(1,132)	
Turnover	-0.006*	-0.006*	-0.0006	0.059
	(0.0035)	(0.0035)	(0.0035)	
Hires	-0.002*	-0.002*	-0.0001	0.022
	(0.0011)	(0.0011)	(0.0011)	
Separations	-0.004*	-0.004*	-0.0005	0.037
	(0.0022)	(0.0022)	(0.0022)	
Employment	1.45	1.26	1.60	30
	(2.12)	(2.09)	(2.29)	
	Panel B: large firms employing > 50 employees ($N = 573$)			
Value added	-876	785	-456	3,328K
	(1,307)	(1,331)	(1,303)	
Turnover	-0.0015	-0.0015	-0.0015	0.053
	(0.0035)	(0.0035)	(0.0035)	
Hires	-0.0005	-0.0005	-0.0005	0.018
	(0.0012)	(0.0012)	(0.0012)	
Separations	-0.001	-0.001	-0.001	0.035
	(0.0023)	(0.0023)	(0.0023)	
Employment	1.25	1.19	1.34	906
	(2.21)	(2.19)	(2.17)	

The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials (D_i) on workforce outcomes (Y_{it}) discussed in equation (1), and instrumented with the variable judge duration ($\tilde{D}_{-i}$). In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from filing the case. Panel A (resp., Panel B) considers the sub-sample of small (resp., large) firms. Small and large firms are split using the median pre-trial level of employment. *Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. All models control for the year in which case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 17 in the sub-sample of small firms, and 18 in the sub-sample of large firms employing more than 50 employees. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.13: Heterogenous effects by Fornero reform – 2SLS-LATE estimates.

Months from filing:	1-12 (1)	13-24 (2)	25-36 (3)	Dep.Var. mean (4)
	Panel A: cases ending before Fornero reform (N = 1,044)			
Value added	-1,409** (633)	-1,287** (609)	-489 (619)	1,917K
Turnover	-0.003** (0.001)	-0.003** (0.001)	-0.0005 (0.001)	0.054
Hires	-0.001** (0.0004)	-0.001** (0.0004)	-0.0002 (0.0004)	0.021
Separations	-0.002** (0.0009)	-0.002** (0.0009)	-0.0003 (0.0008)	0.037
Employment	2.57 (2.54)	2.98 (2.96)	2.47 (2.34)	457
	Panel B: cases ending after Fornero reform (N = 103)			
Value added	-1,695 (7,337)	-1,199 (7,097)	-678 (7,896)	1,571K
Turnover	-0.004 (0.013)	-0.006 (0.014)	-0.0007 (0.012)	0.076
Hires	-0.0009 (0.0049)	-0.0008 (0.0048)	-0.0001 (0.0045)	0.010
Separations	-0.003 (0.014)	-0.002 (0.011)	-0.0005 (0.017)	0.026
Employment	1.78 (26.19)	2.08 (30.93)	1.14 (25.84)	580

The table reports the 2SLS-LATE estimated coefficients (and standard errors) of the effects of the duration of trials (D_i) on workforce outcomes (Y_{it}) discussed in equation (1), and instrumented with the variable judge duration ($\tilde{D}_{-i}$). In columns 1-3, the dependent variables are computed as monthly average 1-12, 13-24, or 25-36 months from filing the case. Panel A (resp., Panel B) considers the sub-sample of small (resp., large) firms. Small and large firms are split using the median pre-trial level of employment.*Turnover* is the sum of hires and separations normalized by the number of employees in the 12 months before the trial starts. *Hires* and *Separations* are the monthly number of workers hired and separeted normalized by the number of employees in the 12 months before the trial starts. *Employment* is the monthly number of employees. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. All models control for the year in which the case was filed, the age of the firm at the time of the trial, and the firm's sector. The first stage F-statistic is 24 in the sub-sample of cases ending before the Fornero reform, and 9 in the sub-sample of cases ending after the Fornero reform. Standard errors are clustered at the judge level. *** p<0.01, ** p<0.05, * p<0.1.