Effect of Firing Frictions on Turnover*

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Abstract

This paper documents a causal relationship between firing costs and turnover at the firm level, by leveraging litigation-generated quasi-experimental variation in firing costs. The causal effect of firing costs on turnover is of the expected negative direction, and of sizable magnitude. This effect is attributed to the effect of litigation risk on regular operations. Exposure to greater litigation risk causes the firm to reduce the riskiness of its normal operations: in particular, employee turnover is ratcheted down to reduce the risk of additional wrongful termination lawsuits. Value added is also shown to decrease in firing costs. Finally, the analysis suggests that trial length need not *per se* affect turnover or value added.

Keywords: Turnover, value added, firing costs, trial length.

JEL Classification: J63, K31, K41.

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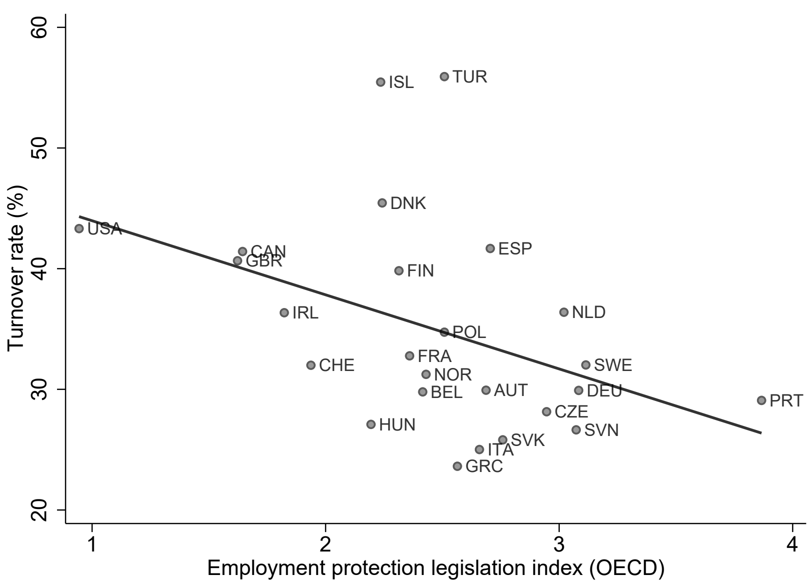
1 Introduction

When firms face larger financial frictions from firing employees, turnover is expected to decrease (see, e.g., Bentolila and Bertola (1990) and Hopenhayn and Rogerson (1993)). This causal effect underpins a large literature that studies the effect of labor market rigidities on various outcomes: unemployment, job reallocations, adjustment to macroeconomic shocks, etc. To our knowledge, however, the causal relationship between firing frictions and turnover has not yet been examined at the firm level.

At a correlational level, Figure 1 illustrates that, indeed, countries with stricter employment protection also have a lower turnover rate. Of course, Figure 1 has important limitations: first, it is a mere cross-country correlation; second, turnover is computed based on aggregate (country-sector), rather than firm-level flows; third, the causal mechanisms that generate the correlation are not known.

Figure 1: Cross-country relation between employment protection and turnover



Note: Turnover is computed as the country average of job flows (hirings plus separations) divided by total employment between 2000 and 2007; employment protection legislation index is measured using the OECD Indicators of Employment Protection between 2000 and 2007. Note that hirings are defined as the number of workers who are with one employer at time t but were not with that employer at time $t-1$. The same definition applies to separations. Source: our elaboration using data from Bassanini and Garnero (2013) (https://sites.google.com/site/bassaxsite/downloadable-datasets)

In this paper, we provide micro-economic evidence concerning the causal link between employment protection and turnover. We match firm balance sheet data (for firm performance) with restricted-use social security data (for turnover) and court docket data (for information about trial length) for firms that are sued in Rome's labor court (the largest labor court in Europe). To the best of our knowledge, we are the first to document *at the firm level* that turnover and value added decrease when the statutory financial penalties for wrongful termination increase. This finding is reasonable. However, the causal channel we shed light on is somewhat different in nature to what is generally assumed, as will be discussed below. We also document that trial length need not *per se* affect turnover or value added. The latter is an important – if counterintuitive – finding in light of the policy focus on the speed of trials as a measure of the "ease of doing business."

Our empirical strategy leverages the random allocation of judges to wrongful termination lawsuits. Due to a peculiar feature of Italian law, if a firm with more than 15 employees is found guilty of the wrongful termination of an employee, the firm must hire back the worker without prejudice *and pay back all the wages accrued during the trial.* The latter provision implies that for these firms, the financial penalty for wrongful termination is effectively increasing in the time it takes the judge to decide the case (refer to Section 2 for a formal proof of this statement). We find that when these firms are randomly assigned to a systematically slower judge, trials last longer and this causes a decrease in turnover and value added.

Our interpretation of these estimates is *not* that firms permanently reduce turnover to prevent more-costly future litigation, because being assigned to a slow judge today is a temporary shock that does not predict the cost of future litigation. Rather, our interpretation is that a labor trial creates financial risk for the firm. This risk lives "in the background" of the firm's regular operational decisions, which we conceptualize as choosing a point on the risk-return frontier. A firm that is matched with a slower judge is saddled with a worse litigation risk which, under certain assumptions, leads the firm to reduce the risk from its regular operations. We interpret the observed reduction in employee turnover as an effort to reduce the risk of another wrongful termination lawsuit. Other unobserved risk-reduction moves may also be made. The total effect, we conjecture, is to move the firm along the risk-return frontier toward less risk and, consequently, lower expected returns. The latter manifest themselves as the observed drop in value added.

Consistent with this interpretation, we also document some interesting null effects. We find that being assigned to a systematically slower judge has no effect on turnover or value added for firms with fewer or equal than 15 employees, or in non-termination cases (pension treatment, etc). This makes sense because, by statute, in these types of cases, trial duration has no impact on the financial penalty for guilty firms.[1] The null effects indicate that trial duration *per se* does not seem to impact turnover and value added, despite a longer trial requiring more legal services.

Our identification strategy has advantages and limitations. Among the advantages are: first, we are able to test the random allocation of judges to cases; second, the source of randomness is uncorrelated with any aggregate shocks that might befall the environment; and last, we can rule out reverse causation, namely, that a change in turnover might cause a change in trial length. Among the limitations are that firm size (above vs below 15 employees) is not randomly allocated, so we must be cautious in drawing inferences regarding the effect of statutory employment protection on firm performance. Moreover, we must worry about the exclusion restriction failing due to other characteristics of the judge, other than the speed of decision, that affect firm outcomes. We discuss this issue in Section 4.

The two most closely related papers are Gianfreda and Vallanti (2017) and Cahuc et al. (2021). Neither paper has data on firm-level turnover, which is a key dependent variable for us. Furthermore, 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 effect 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. In their setting, some judges are more likely to find in favor of the plaintiff. In our setting, in

[1]For example, if a firm with fewer or equal than 15 employees is found guilty of wrongful termination, the firm is merely required to pay the terminated worker a fixed multiple of her salary, irrespective of trial duration.

contrast, judges vary in the speed of disposition, and this translates into a variation in the financial penalty for wrongful termination. Conceptually, the two papers are similar, except for the fact that we can measure firm-level turnover.

Our paper contributes to the literature on the effects of employment protection legislation on turnover and productivity. Following the seminal contributions of Bentolila and Bertola (1990) and Hopenhayn and Rogerson (1993), several empirical papers including: Autor, Kerr and Kugler (2007); Kugler and Pica (2008); Adhvaryu, Chari and Sharma (2013); Petrin and Sivadasan (2013); Bassanini and Garnero (2013); Fraisse, Kramarz and Prost (2015); Sestito and Viviano (2018), find negative effects of firing costs on labor reallocation and productivity. All of these papers achieve identification based on "systemic" variation in employment protection legislation, whereas we leverage "unforeseeable" variation based on the random assignment of judges to cases. Finally, Blanchard and Tirole (2003, 2004, 2008) argue in favor of levying layoff taxes to make firms internalize the social cost of unemployment: our empirical analysis measures the firm-level (as opposed to systemic) effect of such a tax.

Closely related to our identification strategy, 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, 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); Rodano (2021)). 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.

In summary, none of the papers in the literature provides causal firm-level evidence of the effect of employment protection on turnover.

2 Conceptual framework

This section presents a theory that rationalizes why unforeseeable delays in the disposition of termination lawsuits may have an effect on defendant firms' behavior.

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 with trial duration t. With probability $(1-p)$ the firm is found innocent and is required to pay nothing. With probability p the firm is found guilty. According to Italian law, the penalty depends on firm size and trial duration.

- **Firms with fewer or equal than 15 employees, or cases where damages are unrelated to trial duration.** The law requires a guilty firm with fewer or equal than 15 employees to pay damages, in the amount of a fixed multiple of the employee's wages w.

- **Firms with more than 15 employees.** If the guilty firm has more than 15 employees the termination is deemed null, meaning that the firm has to pay back wages accrued during the trial, and must thereafter hire back the worker without prejudice. In this case, after a trial lasting t months, the guilty firm owes the worker back pay w for every period that the trial lasted and, in addition, is required to hire back the worker at a future per-period loss $R-w$.

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.

Consider a firm with more than 15 employees. The payoff stream for the guilty firm, starting the day the employee is fired, is:

$$\overbrace{-w, -w, ..., -w}^{t \text{ periods}}, (R-w), (R-w), ...$$

If the judge is slower and takes $t' > t$ periods to decide the case, the guilty firm's payoff stream is:

$$\overbrace{-w, -w, ..., -w}^{t'>t \text{ periods}}, (R-w), (R-w), ...$$

The difference between the first payoff stream (shorter trial lasting t) and the second (longer trial lasting $t' > t$) equals:

$$\overbrace{0, 0, ..., 0}^{t \text{ periods}}, \overbrace{R, R, ..., R}^{t'-t \text{ periods}}, 0, 0, ...$$

This difference is a positive monetary stream whose net present value is increasing in the difference $t' - t$. This means that the guilty firm prefers trials to be short: longer trials are more costly for the guilty firm.

Lemma 1. *Conditional on being found guilty of wrongful termination, a firm with more than 15 employees benefits financially if trials are shorter.*

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 size and trial duration. If found guilty (probability p), a firm with more than 15 employees has a worse financial outcome, in net present value, if the trial lasts longer. If the guilty firm has fewer or equal than 15 employees, it pays a fixed multiple of the worker wages, so this firm is indifferent as to the duration of the trial.[2] Therefore we have proved the following result.

[2] The firm is indifferent because in the absence of discounting and of legal interest, it has no time preference as to when a lump sum is paid. If the legal interest rate was lower than the firm's interest rate, the firm with fewer or equal then 15 would benefit from longer trials.

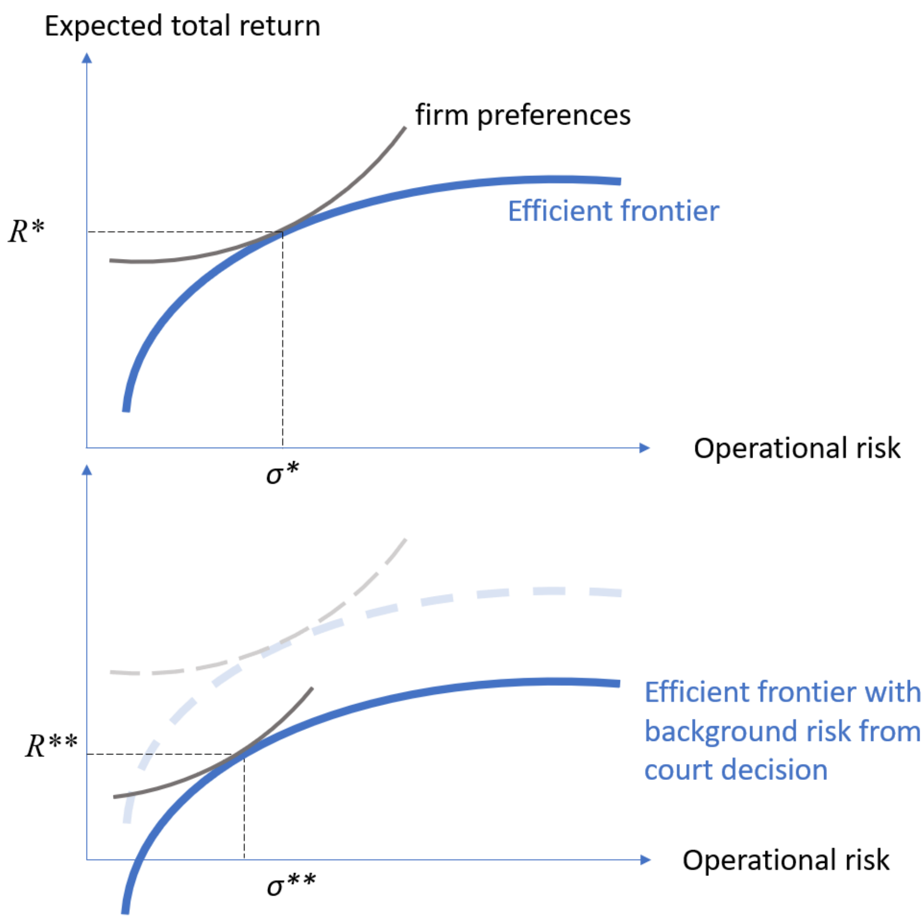
Proposition 1. ***(effect of duration on risk profile of trial outcome)*** *A firm with more than 15 employees that is assigned to a slower judge faces a random shock (trial outcome) with lower mean and higher variance, than a firm which is assigned to a faster judge. A firm with fewer or equal than 15 employees, or a firm involved in a case where penalties do not depend on trial duration, faces a random shock that is independent of the speed of the 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. 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 2. We believe this channel explains the findings in Section 5. This observation is formally stated in the following corollary.

Corollary 1. ***(effect of trial length on operational risk-taking and operational productivity)*** *Suppose the firm's risk preferences are risk vulnerable in the sense of Gollier and Pratt (1996). A firm with more than 15 employees 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. Trial length does not affect the attitudes towards the operational risk of a firm with fewer or equal than 15 employees, or of a firm involved in a case where penalties do not depend on trial duration.*

Figure 2: Effect of trial duration on operational risk taking and operational productivity.



Note: The top panel illustrates the trade-off between operational risk and expected return 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^*).

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

3 Institutional setting and the data

3.1 Institutional setting

The penalties for wrongful termination are set by statute as a function of the number of employees. If a firm with more than 15 employees[3] is found guilty of wrongful termination, the termination is deemed null, meaning that the firm must pay back wages accrued during the trial, and must hire back the worker without prejudice after the trial is over.[4] As explained in Section 2, this feature creates a mechanical dependence between trial duration and penalties for the guilty firm. If a firm with fewer than or equal 15 employees is 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.[5] For these smaller firms, therefore, 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 (i.e., compensation disputes, pension cases, etc.) are also not affected by trial duration.

Each of our cases is decided by a monocratic judge – i.e., neither other judges nor a jury are involved in the disposition – who specializes in labor law. The Italian Constitution requires that cases be randomly assigned to judges. Judges are not allowed to render themselves unavailable for assignments, unless they are sick for long periods (more than

[3]For this computation, managers are not counted as employees: see Art. 18 of Law N.300/1970. We are able to correctly "count out" managers, and so correctly assess a firm's penalty, by virtue of information contained in the social security records.

[4]However, firm and 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.

[5]See Law 604/66, which fixes the damages for wrongful dismissals. The details of the labor law during our sample period are discussed in Ichino (1996) 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.

one week).[6] We test for random assignment in Section 4.

3.2 Data

Our analysis links several data sources. Information on court cases comes from confidential data from cases filed in the Court of First Instance in Rome, Italy, between January 1, 2001 and July 1, 2014 (this is the labor court of first instance in Europe's largest tribunal.[7]) Our data contains information on case characteristics (type, number of plaintiffs and defendants), identifiers for judges, defendants, and plaintiffs, date of assignment, and date of disposition.[8] We define case duration as the time elapsed between the date of assignment and the date of disposition. To avoid right-censoring, we drop cases filed after December 31, 2011, so that the matched firm data are available for at least seven years after the trial start. After applying these restrictions, we obtain a "baseline sample" including 398,078 trials assigned to 95 judges.

The Italian Social Security Administration (INPS) 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 1999-2018.[9] We asked INPS to restrict the match to trials where a firm is a party in trial.[10] 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 discard trials where a firm has previously appeared in our sample.

[6]In a few rare cases some judges show prolonged periods of inactivity (many months). We drop from our sample judges who had less than 10 cases per year.

[7]Largest by number of cases: see https://www.repubblica.it/2007/01/sezioni/cronaca/bolzoni-tribunale/tribunale-seconda-puntata/tribunale-seconda-puntata.html

[8]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 reasons that become known after filing, or because exceptional circumstances arise.

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

[10]Not all disputes involve a firm. For example, there are many cases between individuals and public administrations or other entities. The court-level data does not reveal whether a party is a person, a firm, or a government agency. But we know the parties' names and, occasionally, their social security number. The parties' names and, when available, their social security number, were used to link the two datasets.

This restriction reduces the sample to 13,785 trials, and the same number of unique firms.[11] We call this the "firm-restricted" sample.

We use both datasets, the baseline one (398,078 trials) and the firm-restricted one (13,785 trials), to test for the random assignment of cases to judges.

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.[12] This is because this paper focuses on the economic frictions associated with termination (i.e., we do not focus on disputes over, e.g., compensation or other complaints).[13] The restriction to more than 15 employees is motivated by the fact that our hypothesized mechanism (i.e., the variation in financial penalty for wrongful termination) is only operative for firms above 15 employees. The remaining 12,638 firms will be used as placebo.

A key advantage of social security data is that it allows us to compute at the firm level. We compute monthly turnover as the sum of hires and separations normalized by the pre-trial workforce of the firm; for example, the number of employees in the 12 months before the trial starts. This information is not available from balance sheet data and is a key contribution in this paper.[14] Our second dependent variable, value added, is provided at the yearly frequency by Cerved data.[15]

4 Empirical Strategy

In this section, we describe our empirical strategy. We begin by describing how we use the random allocation of cases to judges to estimate the effect of court delays on firm-level outcomes. We then test whether or not cases are in fact randomly assigned to judges, and provide support for other identifying assumptions.

[11] Table A.1 reports the distribution of the number of trials by firms. Most firms (68 percent) experience only one trial and it is very uncommon for firms to have more than three trials (10 percent).

[12] As discussed in Section 3.1, we exclude managerial positions from this computation.

[13] There are 3,668 worker compensation disputes and 3,241 "other" types of trials. The category "other trials" groups together allowance, pension, temporary contracts, qualification, disability, etc.

[14] In Appendix A we also analyze hiring and separation rates and the stock of employment, separately.

[15] We focus on value added as a measure of performance rather than on earnings before interest, taxes, depreciation, and amortization (EBITDA), because value added is arguably less subject than EBITDA to shocks that the firm cannot control.

We are interested in the causal effects of trial duration on firm turnover and value added, where these outcomes are measured starting at the moment the case is filed. We estimate the following equation:

$$Y_i = \beta D_i + X_i'\theta + \varepsilon_i, \tag{1}$$

where β is the parameter of interest, D_i is trial duration measured in months from filing to disposition of case i. X_i is a vector of control variables including the year in which case i was filed, the age of the firm at the time of the trial, and the firm's sector.[16] Y_i is the dependent variable of interest measured one, three, and six years after firm i's court case begins (e.g., turnover in the six years after the case was filed). Following Abadie et al. (2017), we cluster standard errors at the judge level because this is the level at which our randomization takes place.

The OLS estimates of model (1) will be biased if, as seems plausible, trial duration D_i correlates with firm characteristics that affect labor turnover or firm performance. We address this concern by exploiting the random allocation of cases to judges (conditional on year fixed effects) and the fact that some judges are systematically slower than others. This procedure creates quasi-experimental random variation in trial duration, depending on which judge firms are assigned to.

Our main analysis is based on two-stage least squares (2SLS-LATE) estimation of β, with equation (1) as the second-stage equation and a first-stage equation specified as

$$D_i = \gamma Z_{-i} + X_i'\delta + \nu_i. \tag{2}$$

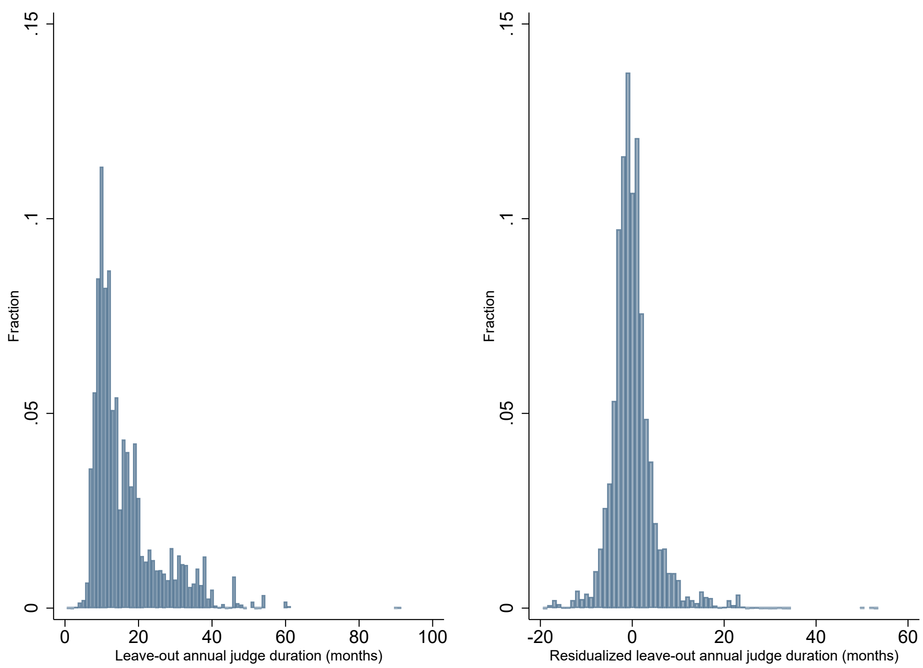
The scalar variable Z_{-i} denotes the leave-out mean duration of trials of the judge assigned to firm i's case. The mean is computed on all cases assigned to that judge during the calendar year in which case i was filed, excluding case i. The leave-out mean is computed using the "baseline sample" of 398,078 cases, using all past and future cases assigned to a given judge in a given year. In Appendix A, we show robustness to alternative measures of Z_{-i}. If the instrument is valid, the 2SLS-LATE estimates $\hat{\beta}$ represent the causal effect

[16] Years of filing must be controlled for because the randomization of cases to judges is conditional on the pool of available judges in each given year. However, the inclusion of additional controls only affects the precision of our estimates and not the point estimate because of the randomization of cases to judges, as shown in Section 4.1.

of one extra month of trial duration resulting from being randomly assigned to a slower judge.

There is considerable variation in the instrument Z_{-i}, which is convenient for estimation purposes. Figure 3, left-hand panel, plots Z_{-i}. Intuitively, variation in Z_{-i} is a measure of the variation in speed of disposition that is *due to the judge*. Indeed, since each judge is assigned around 300 cases per year, by the law of large numbers we expect each Z_{-i} to be approximately the same for all is within a given year×judge. Therefore, the dispersion depicted in the figure is a measure of year×judge variation in durations. 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 dispersion remains sizable after controlling for the year of case filing (right-hand panel). This dispersion reveals the cross-judge difference in duration, which will be our identifying variation.

Figure 3: Distribution of the instrument Z_{-i}



Note: The plotted values are raw leave-out annual judge duration and the residuals from regressions on year of filing fixed effects. Bin size 1 month.

Figure 3 also 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.

4.1 Validity of the instrument

First, we confirm empirically that cases are randomly allocated to judges. Then, we discuss other desirable properties of the instrument: relevance, exclusion restriction, and monotonicity.

Random allocation of cases to judges

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

$$Z_{-i} = \zeta K_i + \gamma T_i + \epsilon_i. \tag{3}$$

where T_i are the year-of-assignment fixed effects. If cases are randomly allocated within a year, case characteristics K_i should not predict any judge characteristic, including the judge's speed Z_{-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 1 columns (1) and (2)), then in the "firm-restricted sample" of trials, in which at least one party, either plaintiff or defendant, is a firm and is linked to the social security archives (Table 1 columns (3) and (4)). In both samples, the estimates $\hat{\zeta}$ are quantitatively small and only one is individually statistically significant at the 10% level. Tables A.2 and A.3 provide further evidence on the randomization of cases to judges and show that the zero effects reported in Table 1 are not due to measurement error because the variables K_i are highly predictive of cases duration. This provides strong evidence that cases are randomly assigned to judges, conditional on year of assignment.[17]

Instrument relevance

Table 2 reports first-stage estimates of equation (2) where we regress trial duration D_i on our instrument for judge speed, Z_{-i} and controlling for the year-of-assignment, the type of case, the number of parties and the lawyers' characteristics in the "baseline sample" (N = 398,078), and for the year-of-assignment, the type of case, the sector and

[17] Coviello, Ichino and Persico (2019) show random allocation of cases to judges in the same dataset.

Table 1: 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 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 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. * $p < 0.1$.

age of the firm in the "firm-restricted sample" (N = 13,785). In Panel A, we include year dummies but no other controls otherwise. The estimates are highly significant, suggesting that being assigned to a judge who is on average one month slower, increases the duration of the current trial by roughly 15 days. Panel B of Table 2 controls for X_i. In both specifications, the first-stage F-statistics (instrument) are well above 10, indicating that a judge's speed on trials other than i is a relevant predictor of the duration of trial i.

Incidentally, comparing Panels A and B provides a further test of random assignment: if judges are randomly assigned, predetermined variables should not significantly change the estimates, as 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.

Table 2: 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	1.017***	0.528***
	(0.013)	(0.081)
F-statistic (instrument)	6,341	42
	B. Additional Controls	
Judge duration	0.983***	0.525***
	(0.017)	(0.081)
F-statistic (instrument)	3,493	42
Dependent mean	19	17
Instrument mean	16	18

Notes: The dependent variable is trial duration. 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. *** $p < 0.01$.

Exclusion restriction

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 called *insindacabilita' delle scelte imprenditoriali*, whereby the judge is required to stay out of the entrepreneur's choices as far as possible. 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). As for the latter, we estimate that the speed at which a judge decides does not affect judge decisions, which we measure with the probability that the plaintiff wins the case.[18] Therefore, we believe that the case for the exclusion restriction is reasonably solid in our context.

Monotonicity

Imbens and Angrist (1994) show that a monotonicity assumption is required for identifying 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 for any subsample. For this test, we continue to construct the judge delay variable using the full sample of available cases but estimate the first stage on a specific subsample defined by the specific type of litigation or the size of the firms. Results are reported in column 1 of Table A.4. Panel A splits the sample by type of litigation; Panel B, by firm characteristics. For all these subsamples, 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 subsamples as we did for the first test but redefine the instrument for each subsample to be the judge's delay for cases outside of the subsample. For example, for the firing case subsample, we use a judge's delay constructed from all cases except firing cases. Column 2 of Table A.4 lists the first-stage estimates using

[18] We run model (1) using the same identification strategy as in Section 4 but with the dependent variable a dummy indicating that the plaintiff wins, and we find a small and statistically insignificant effect (β =-0.001 and standard error of 0.001). A one standard deviation increase in the duration of trials decreases the probability that the plaintiff wins by 0.11 percentage points (approximately 4%).

this reverse-sample instrument, which excludes own-type cases. The first-stage estimates (and the first-stage F-statistics obtained as the square of the t-tests) are all positive and statistically different from zero (larger than 10), suggesting that judges who are slower for one type of case are also slower for other case types.

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

5 Results

5.1 Core findings

Table 3 reports the 2SLS-LATE estimates of equation (1) in the "estimation sample" of interest: firing cases with defendant firms employing more than 15 workers. These estimates indicate that longer trials reduce firm turnover and value added. Columns 1 shows that within a one-year window 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 value added by 1,265 Euros, or approximately 1%; turnover decreases by 0.03 percentage points, or approximately 56%. Columns 2 and 3 confirm these results, both qualitatively and quantitatively, at the three- and six-year windows.

As a sanity check, in Table A.5 we report the reduced form estimates (ITTs) of equation (1) where we only replace D_i with Z_{-i}. As expected, these estimates are smaller than the 2SLS-LATE estimates (i.e., the latter estimates are obtained as the ratio between the ITTs and the first-stage estimates) and confirm that being randomly assigned to a slower judge causes a reduction in firm turnover and value added.

In sum, recall that for firms with more than 15 employees, increasing trial duration mechanically implies higher expected financial penalties for wrongful termination. This penalty increase is shown here to causes firms to reduce turnover and value added. This evidence is consistent with the theoretical predictions of Proposition 1.

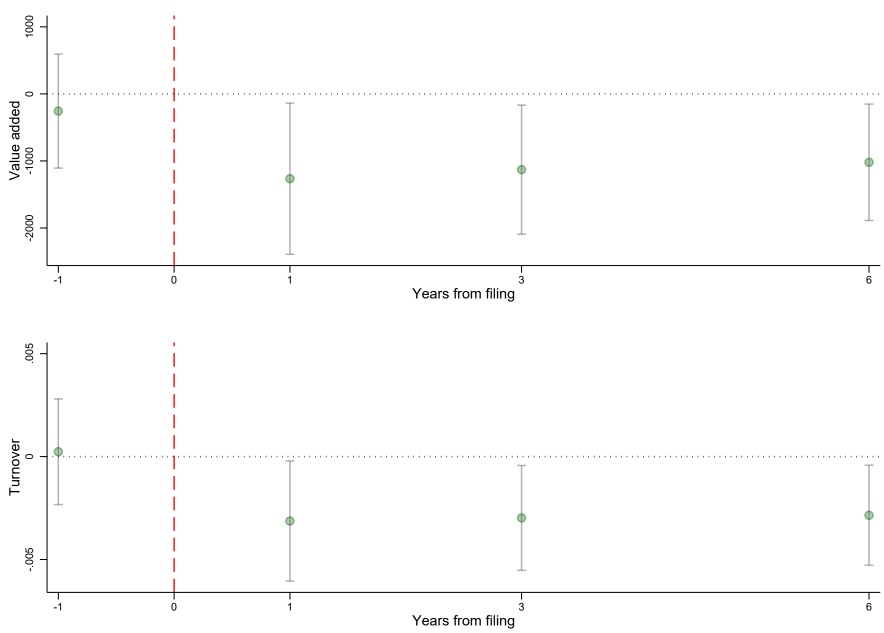
Table 3: Delayed justice reduces value added and turnover – 2SLS-LATE estimates.

	Subsample – firing trials (Employees > 15) (N = 1,147)			
	Within 1 year filing (1)	Within 3 years filing (2)	Within 6 years filing (3)	Dep.Var. mean (4)
Value added	-1,265**	-1,130**	-1,020**	1,886K
	(575)	(491)	(443)	
Turnover	-0.003**	-0.003**	-0.003**	0.056
	(0.001)	(0.001)	(0.001)	

Notes: Value added is expressed in 2014 Euros. 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. In columns 1-3, the dependent variables are computed as 1, 3, 6 years averages after filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. *Dep. Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The first stage F-statistic is 24. Standard errors are clustered at the judge level. ** $p < 0.05$.

Figure 4 provides a graphical representation of the estimates in Table 3. Reassuringly, the figure shows that there are no anticipation effects (as one would expect given that judges are randomly assigned).

Figure 4: Delayed justice reduces value added and turnover and it has no effects on pre-trial outcomes



Note: The plotted values are 2SLS-LATE estimates for 1, 3, 6 years time windows following the filing of the case. Vertical bars show 95% confidence intervals.

5.2 Placebo tests and robustness checks

We perform four placebo tests for firms where, statutorily, firing costs are not affected by trial length. In all cases, we expect to find no relation between trial duration and firm outcomes. The first test is on firing cases for firms with fewer or equal than 15 employees (Panel A of Table 4).[19] The second and third tests concern compensation cases (Panel B), and "other cases" (Panel C). The final placebo test considers the cases where the plaintiffs, rather than the workers, are the firms (see Panel D of Table 4). We run model (1) using the same identification strategy as in Section 4. Reassuringly, in all panels of Table 4 we find no effects.

Taken together, the placebo tests are strong evidence that the estimated effects are due to the increase in financial exposure associated with longer trials, and not to trial length *per se*.

Next, we check whether hiring or separation rates explain the results on turnover. Table 5 reports the same 2SLS-LATE with hirings and separations as outcomes.[20] We find that both hirings and separations are reduced. These results are coherent with our theory that the firm reduces the riskiness of its normal operations.

[19]For these firms, as discussed in Section 3, statutorily, firing costs for these firms are not affected by trial length; therefore, we expect to find no relation between trial duration and firm outcomes.

[20]Table 5 also shows that employment is not affected by trial duration.

Table 4: Placebo tests

	Within 1 year filing (1)	Within 3 years filing (2)	Within 6 years filing (3)	Dependent mean (4)
	A. Subsample – firing trials (Employees <= 15) (N = 2,429), (F-statistic instrument = 28)			
Value added	-127	-131	-231	704K
	(112)	(113)	(200)	
Turnover	-0.005	-0.004	-0.005	0.104
	(0.004)	(0.003)	(0.004)	
	B. Subsample – compensation trials (N = 3,668), (F-statistic instrument = 36)			
Value added	296	389	368	1,456K
	(596)	(648)	(613)	
Turnover	-0.001	-0.001	-0.001	0.141
	(0.001)	(0.001)	(0.001)	
	C. Subsample – other types of trials (N = 3,241, (F-statistic instrument = 33))			
Value added	-936	-986	-969	1,134K
	(1976)	(1986)	(1979)	
Turnover	-0.002	-0.002	-0.002	0.243
	(0.002)	(0.002)	(0.002)	
	D. Subsample – plaintiff firms (all types of trials) (N = 3,300), (F-statistic instrument = 34)			
Value added	-342	-312	-326	1,143K
	(520)	(490)	(510)	
Turnover	0.016	0.014	0.015	0.180
	(0.017)	(0.016)	(0.016)	

Notes: Annual value added is expressed in 2014 Euros. Monthly turnover is the sum of monthly 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. In columns 1-3, the dependent variables are computed as 1, 3, 6 years averages after filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. *Dep. Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. Standard errors are clustered at the judge level.

Table 5: Delayed justice reduces hiring and separations

	Subsample – firing trials (Employees > 15) (N = 1,147)			
	Within 1 year filing (1)	Within 3 years filing (2)	Within 6 years filing (3)	Dep.Var. mean (4)
	A. 2SLS-LATE estimates			
Hires	-0.001**	-0.001**	-0.001**	0.020
	(0.0004)	(0.0004)	(0.0003)	
Separations	-0.002**	-0.002**	-0.002**	0.036
	(0.0009)	(0.0008)	(0.0008)	
Labor	1.26	1.33	1.39	468
	(1.15)	(1.35)	(1.41)	
	B. Reduced-form estimates			
Hires	-0.0005**	-0.0005**	-0.0005**	0.020
	(0.00022)	(0.00022)	(0.00020)	
Separations	-0.001**	-0.001**	-0.001**	0.036
	(0.0004)	(0.0004)	(0.0004)	
Labor	0.63	0.67	0.69	468
	(0.575)	(0.675)	(0.705)	

Notes: Hires and separations are measured at the monthly level and normalized by the pre-trial workforce of the firm, e.g., the number of employees in the 12 months before the trial starts. Labor is the number of employees. In columns 1-3, the dependent variables are computed as 1, 3, 6 years averages after filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. The first stage F-statistic is 24. Standard errors are clustered at the judge level. ** $p < 0.05$.

5.3 Interpretation

The results in Section 5.1 indicate that a combination of longer trials and larger penalties decrease firm-level turnover and value added. The results in Section 5.2 suggest that longer trials are not responsible for the effect, because the effects are not present in firms with less than or equal 15 employees and other placebo samples. Finally, the fact that the variation in penalty (generated by a slower judge) in *this trial* is not predictive of greater penalties (slower judges) in *future trials*, indicates that the firm's behavior cannot be anticipatory of future trial costs, but it must be a response to increased costs *in the present trial*. Taken together, the evidence supports our interpretation that the firm becomes more cautious when facing a larger litigation-related financial risk.

6 Conclusions

This paper contributes to the large literature that implicitly assumes a causal relationship between firing costs and turnover. For the first time to our knowledge, we are able to prove a causal relationship between firing costs and turnover at the firm level, by leveraging litigation-generated quasi-experimental variation in firing costs.

We find a causal effect of firing costs on turnover of the expected (negative) direction, and of sizable magnitude. The causal channel, however, is novel and subtle. The effect we measure does not come from firms reducing turnover to prevent more-costly future labor litigation. Rather, our effect comes from the background risk created by the present litigation. Once sued, a firm faces the risk of being found guilty and penalized monetarily. The larger the penalty (firing costs), the larger the present litigation risk for the firm. Exposure to a larger litigation risk causes the firm to reduce the riskiness of its normal operations: in particular, employee turnover is ratcheted down to reduce the risk of additional wrongful termination lawsuits. In addition, other moves along the risk-return frontier may be effected, with a view to reducing operational risk, even at the cost of reducing expected returns from operations. Thus the mechanism is consistent with the drop in value added that we also document.

Our analysis does not exclude the presence of the additional, more conventional, channel whereby firms reduce turnover to prevent more costly future litigation. This effect may or may not be there. Our identification strategy simply does not explore this channel.

Finally, our analysis suggests that trial length need not *per se* affect turnover or value added. This is an important, if counterintuitive, finding in light of the policy focus on the speed of trials as a measure of the "ease of doing business." This finding may be consistent with anecdotal evidence that, in our specific setting, labor lawyers charge "by the case" and not "by the hearing." If that is in fact the case, then lawyers may be absorbing some of the cost of "doing business." This research avenue is intriguing, but it is beyond the scope of this paper.

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Appendix A

Table A.1: Number of trials per firm

Number of firms involved in	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: 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: Shown is the population of cases filed in the labor court of Rome in 2000–12. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: 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	0.000013	.0000106	.000017	1,295K	6,176K
Labor	.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: Shown is the sample of firms going the labor court of Rome in 2000–12. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Tests for the monotonicity assumption

	Baseline Instrument	Reverse-sample Instrument
	First stage	First stage
	(1)	(2)
A. Type of litigation		
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
B. Types of firms		
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: The dependent variable is trial duration. Standard errors are clustered at the judge level. *** $p < 0.01$.

Table A.5: Slow judge reduces value added and turnover – reduced-form estimates.

	Subsample – firing trials (Employees > 15) (N = 1,147)			
	Within 1 year filing (1)	Within 3 years filing (2)	Within 6 years filing (3)	Dep.Var. mean (4)
Value added	-632**	-565**	-510**	1,886K
	(287)	(245)	(221)	
Turnover	-0.002**	-0.002**	-0.002**	0.056
	(0.001)	(0.001)	(0.001)	

Notes: Annual value added is expressed in 2014 Euros. Monthly 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. In columns 1-3, the dependent variables are computed as 1, 3, 6 years averages after filing. The mean and standard deviation of trial duration is 15 and 14 months, respectively. *Dep.Var.* means are the sample means of the dependent variable calculated in the 12 months before filing. Standard errors are clustered at the judge level. ** $p < 0.05$.