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The Real Effects of Judicial Enforcement

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The Real Effects of Judicial Enforcement*

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Abstract

This paper shows that judicial enforcement has substantial effects on firms' decisions with regard to their employment policies. To establish causality, I exploit a reorganization of the court districts in Italy involving judicial district mergers as a shock to court productivity. I find that an improvement in enforcement, as measured by a reduction in average trial length, has a large, positive effect on firm employment. These effects are stronger in firms with high leverage, or that belong to industries more dependent on external finance and characterized by higher complementarity between labor and capital, consistent with a financing channel driving the results. Moreover, in presence of stronger enforcement, firms can raise more debt to dampen the impact of negative shocks and, in this way, reduce employment fluctuations.

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1 Introduction

Well-functioning legal institutions are crucial for enforcing contracts and facilitating transactions. For this reason, improving judicial enforcement, and in particular speeding up trial resolution by courts, has been a priority for many judicial systems.[1] The main argument in favor of effective courts is that they favor firm financing, by protecting outside investors from opportunistic behavior and, thus, reducing credit rationing (Jappelli et al., 2005). Indeed, much of the evidence shows that the presence of strong enforcement is associated with greater use of external financing and faster firm growth (Bae and Goyal, 2009; Brown et al., 2017).

A welfare evaluation of the consequences of judicial enforcement should, however, assess also the effects on firm employment, which are ex ante unclear. Theoretically, an improvement in enforcement may increase firms' access to capital, which can, in turn, induce firms to shift to more capital-intensive technologies, reducing employment as a result (Kim et al., 2019). On the other hand, empirical research has pointed towards a positive correlation between the quality of judicial enforcement and firm employment (see for example Laeven and Woodruff, 2007, and Giacomelli and Menon, 2016).

A related question is how changes in enforcement affect employment responses to negative shocks in product demand, and so firms' ability to provide workers with "insurance" from labor income risk. On the one hand, strong enforcement may facilitate financing during periods of low demand, allowing firms to stabilize employment. On the other hand, it makes easier for lenders to force unprofitable borrowing firms into bankruptcy or downsizing, resulting in layoffs. Hence, this relationship also is theoretically ambiguous.

This paper fills these gaps by focusing on a natural experiment as a shock to the average duration of court proceedings in a large developed economy. This is an important measure of judicial enforcement: Slow courts reduce lenders' incentives to seize borrowers' assets, reducing credit availability ex ante (Visaria, 2009). Specifically, I take advantage of a reorganization of the judiciary involving 49 court districts in Italy. In 2013, 26 courts were suppressed, and their districts were absorbed by 23 other districts; such "mergers" generate a heterogeneous shock to the quality of

[1] See Chemin (2018). Policy institutions have also endorsed such efforts; for example, the World Bank *Doing Business Index*, originally based on the work by Djankov et al. (2003), has among its main determinants the time and cost for resolving a commercial dispute.

judicial enforcement that I exploit for causal identification. Using an instrumental variable approach, I find that an improvement in judicial enforcement, corresponding to a reduction in the average length of proceedings, has a positive and large effect on employment. The financing channel appears important: Results are much stronger for firms highly dependent on external finance. Moreover, firms in districts with stronger enforcement dampen the impact of negative demand shocks on employment by borrowing against shortfalls in earnings, in line with models of implicit contracts (Baily, 1974; Azariadis, 1975; Holmstrom, 1983).

Consider the following example to illustrate the strategy used in the paper. Let the districts of courts A and B be equal in size and have average durations of proceedings equal to 100 and 200 days, respectively. Court A is then suppressed, all its judges move to court B, and the firms originally under the district of court A are now under the jurisdiction of court B. The expected trial length of the "new" court B is (100+200)/2=150 days. Thus, firms originally under court A are now subject to a slower court, while firms originally under court B can expect a trial duration 50 days shorter.[2] Such predicted value can be used as an instrument for the realized post-reform average duration of proceedings. Length of proceedings is unlikely to be related to other court characteristics, such as judges' "accuracy" in decision-making (Coviello et al., 2015). However, it is strongly correlated with another standard measure of enforcement, court backlog per judge. Following recent empirical work (Ponticelli and Alencar, 2016; Brown et al., 2017), I will restrict the analysis to firms in cities located along the court district borders. This will isolate the effects of the sharp change in trial length caused by the merger of the court districts, under the assumption that potential omitted variables evolve in a similar fashion across neighboring cities.

Using this instrumental variable approach, I find an elasticity of employment to average length of proceedings in the neighborhood of -0.3, which is economically large and precisely estimated. These estimates suggest real effects of judicial enforcement that are quite large when compared with what found in previous work. In contrast, naïve OLS regressions that do not take into account the endogeneity of judicial enforcement produce elasticities that are inconsistent, in sign and magnitude, across different specifications.[3] A key novelty of this setting is that it exploits local

[2]In the paper, the words "trial" and "proceeding" will be used interchangeably.

[3]Judicial enforcement tends to be stronger in more economically developed areas, and hence is

exogenous variation in trial duration *within firm*, allowing me to run event study tests that support a causal interpretation of the evidence in a reduced form difference-in-difference design. The results are robust to a battery of additional tests. They are unaffected when excluding small firms, or when controlling for a number of firm- and regional-level control variables. Moreover, they are robust to the inclusion of industry-year and even industry-year-border fixed effects, to account for the fact that different industries might be prone to lawsuits differing in their complexity. As a falsification test, I simulate court mergers in districts unaffected by the reform, and show that there is no effect of these "placebo reforms" on employment. Moreover, judicial enforcement appears to foster entrepreneurship, as measured by firm creation.

Changes in enforcement can affect multiple stakeholders, and in particular both workers and external providers of capital, so that this evidence is consistent with two economic mechanisms that have received considerable attention in the literature, involving either the former of the latter.

First, judicial enforcement may affect firing costs. Layoffs may be followed by lawsuits brought by dismissed employees. As argued by Bamieh (2016), a higher trial length is equivalent to an increase in firing costs, because longer trials are related to later resolution of uncertainty (Bloom, 2009), higher lawyers' fees, and a higher refund to workers in case or restatement. A reduction in firing costs can, in turn, result in higher employment in equilibrium (see for example Besley and Burgess, 2004; Autor et al., 2006).

Second, judicial enforcement may alleviate financing constraints when firms are hit by negative shocks.[4] By borrowing against temporary shortfalls in cash flow, firms can avoid layoffs and stabilize employment.[5] If workers are risk averse, job stability

correlated with firm employment (that is, firm employment tends to be higher in districts with low average trial length). However, in OLS regressions this association disappears when exploiting only variation over time, for example by controlling for firm fixed effects. IV results are, instead, robust to the inclusion of firm fixed effects. See also Djankov et al. (2003) for related cross-country evidence.

[4] Average trial length captures only an aspect of a concept, enforcement, which is of course broader, but has been shown to be important in practice. For example, Schiantarelli et al. (2016) show that the length of proceedings is a strong determinant of borrowers' decision to default against banks with high levels of past losses in Italy. Moreover, it is certainly relevant to policymakers and practitioners (and often emphasized). See Sections 3.1 and 3.5 for institutional details and anecdotal evidence.

[5] This argument does not imply that unconstrained firms will not fire workers after a negative demand shock. Indeed, it might be optimal to do so if changes in demand reflect variation in profit opportunities. However, financial constraints may result in a contraction of the workforce larger than what would be optimal, which is in lines with Chodorow-Reich (2013) and Benmelech et al.

will be priced in lower wages (Berk et al., 2010; Agrawal and Matsa, 2013), making hiring cheaper and leading to higher employment.

Importantly, in both scenarios greater judicial enforcement will boost a firm's employment level, but its sensitivity to demand shocks will be different. In the first case, firms will be more willing to reduce employment following negative shocks, because faster courts reduce firing costs. In the second framework, stronger enforcement allows firms to obtain financing during periods of low demand, sheltering them from shocks and stabilizing employment.

The evidence shows that the second channel dominates empirically. I find that employment in firms that, thanks to the reform, benefit from better judicial enforcement, becomes less sensitive to demand shocks, as proxied by average industry sales (Sraer and Thesmar, 2007; Ellul et al., 2018). This drop in sensitivity is driven exclusively by firms hit by negative shocks, consistent with this insurance mechanism. Although the data have aggregate firm employment figures, and so do not allow distinguishing between firing and hiring decisions, these results provide suggesting evidence that a significant part of the total employment effect might be due to reduction in layoffs in presence of stronger enforcement.

I test this channel more directly by showing that firms are more likely to raise their debt levels following negative shocks if they operate in districts with better enforcement. Again, this is in line with the idea that the ability to obtain funding when facing liquidity shortfalls is enhanced by strong enforcement.

Several pieces of evidence are consistent with an improvement in judicial enforcement allowing firms to stabilize employment. First, there is a negative relationship between strength of judicial enforcement and average wages. Although this relationship is only marginally significant in the full sample, it becomes large and precisely estimated in firms characterized by more volatile earnings or in industries with larger fluctuations in sales. Because these firms are more subject to demand shocks, these are precisely the instances where this implicit insurance is more valuable, and hence where it should be reflected in lower wages. Second, consistent with the financing channel, I find that employment rises primarily in firms that should benefit more from access to credit: those that are highly levered and that belong to financially dependent industries. Third, the effect is also larger in areas where layoffs can cause

(2011), who show that, following negative shocks, employment falls more in financially constrained firms.

arguably more distress for workers, and hence where this implicit insurance should be more beneficial, such as in provinces with a high unemployment rate.

Firing costs, on the other hand, do not appear to play a major role. The results are very similar when restricting the attention to firms with at most 15 employees, which are granted by Italian labor laws great flexibility, being able to fire "at will" without the involvement of courts.

If judicial enforcement impact employment primarily through its effects on financing constraints, firm technology should play an important role. Specifically, the effects of enforcement on employment should be magnified in presence of strong complementarity between labor and capital. Conversely, they should be smaller if an alleviation in financing constraints induces firms to substitute labor with capital. Using estimates of capital-labor elasticity of substitution recently obtained by Oberfield and Raval (2014) for the manufacturing sector, I find that the effects of enforcement on employment are much stronger in industries in which capital and labor are complement. Hence, the benefits of enforcement might be more muted or even larger in other economies, depending on their sectoral mix.

I find results similar to those on employment when looking at the total wage bill or revenues. There is no effect on debt level or leverage, consistent with cross-country evidence (Rajan and Zingales, 1995; Booth et al., 2001). Hence, enforcement appears to affect primarily the sensitivity of debt to shocks, rather than its average level.

Overall, this paper finds that judicial enforcement has beneficial real effects. A first contribution lies in the use of a natural experiment that can rule out confounding factors and exploits within firm variation to examine the real effects of judicial enforcement. In this context such strategy is necessary because differences in enforcement across countries or regions are not random, but related to institutional and historical features that make causal inferences problematic. Moreover, the paper employs an intuitive and widely used measure of court quality, such as average trial length. Second, I show that the presence of stronger enforcement not only increases firm employment, but also reduces the sensitivity of employment to demand shocks, by allowing firms to obtain financing during downturns. The evidence on this implicit insurance is crucial to rationalize the presence of a negative relationship between strength of judicial enforcement and average workers' compensation. While it is well-established that firms have an important insurance role for workers,[6] I show

[6]See Pagano (2019) and Guiso and Pistaferri (2020) for two recent literature reviews.

that there is substantial heterogeneity in their ability to provide such protection, depending on judicial enforcement and, hence, on the ability to obtain financing.

The paper is organized as follows. Section 2 summarizes the related literature. Section 3 describes the suitability of trial length as a measure of judicial enforcement, the institutional setting, and the reform. Section 4 describes the data and the identification strategy. Section 5 shows the main results on employment and robustness tests. Section 6 isolates the economic mechanism by presenting results on the sensitivity of employment to shocks, debt, wages, and cross-sectional heterogeneity tests. Section 7 concludes.

2 Related Literature

There is an ample literature on the effects of legal institutions, especially on financial development and the cost of external financing. The seminal work of La Porta et al. (1997) documents that the different roots of legal systems are related to a number of measures of financial development. Other papers that also focus on cross-country evidence are Haselmann et al. (2009), Lerner and Schoar (2005), Bae and Goyal (2009), and Qian and Strahan (2007).

More recently, some studies have used single-country data to control better for potential omitted variables. For example, Ponticelli and Alencar (2016) exploit the passage of a bankruptcy reform in Brazil that increased creditors' rights and show that its effects are magnified in courts with an effective law enforcement.[7] Rather than focusing on this interaction effect, in this paper I study whether judicial enforcement can affect the real economy *directly*. Another closely related paper is Brown et al. (2017), that uses data from Native American reservations and compares those assigned to state courts to those under the jurisdiction of tribal courts. They find that predictability in enforcement of contracts, which is higher in state courts, results in stronger credit markets and higher per capita income. In this paper I focus primarily on the effects on employment level and variability, using a simple and intuitive measure such as average trial length, in principle comparable even across countries, in the context of a developed economy.

On the methodological side, my empirical strategy follows these papers in adopting a spatial-discontinuity design. However, I exploit not only cross-sectional heterogene-

[7]See also Rodano et al., 2016, who study a similar reform in Italy.

ity, but also time-varying exogenous variation in trial duration. This strategy helps addressing the possibility of sorting, namely that firms may choose on which side of a district border they are headquartered in based on unobserved characteristics. In this setting I control for all the time invariant characteristics that may determine a firm's sorting through firm fixed effects, and examine how employment changes over time within firm.

A more recent and active series of works has the intersection of Labor and Finance as its focus, in particular the effects of shocks to financing on employment. Pagano and Pica (2012) show that a reduction in financial constraints increases employment and inter-industry job reallocation. Chodorow-Reich (2013) finds that firms that were borrowing from banks that cut lending during the Great Recession reduced employment sharply. Benmelech et al. (2011) show that cash flow shocks have a significant impact on firms' employment growth. Chaney et al. (2013) and Kleiner (2014) study the impact of changes in collateral value due to real estate prices on employment, also finding large effects. Unlike these works, the focus of this paper is on the quality of the judiciary, and I also provide evidence on wages and employment sensitivity to demand shocks; however, consistent with them, I show that the financing channel appears important.

This paper is also related to empirical work showing that firms can offer an implicit insurance against unemployment risk to risk averse and undiversified employees, which will be priced in lower wages. This consideration may also impact capital structure decisions, leading to more conservative financing choices (Berk et al., 2010; Agrawal and Matsa, 2013). Empirical research has typically exploited heterogeneity in the ability to commit to offer such contracts, due to differences in ownership structure or managerial preferences. Ellul et al. (2018) and Sraer and Thesmar (2007) show that employment in family firms is significantly less sensitive to industry shocks. In the same spirit, Mueller and Philippon (2011) argue that family ownership can ameliorate labor relations. Kim et al. (2018) exploit codetermination regulations in Germany and find that if workers have representation on corporate boards they are more protected from layoffs against industry shocks. Workers' preferences can also be reflected in financing terms, especially when workers have influence on corporate affairs. Chen et al. (2011a) and Chen et al. (2011b) show that firms in unionized industries have lower bond yields and higher cost of equity. This paper focuses on another aspect of these theories, namely the assumption that firms are able to borrow vis-à-vis adverse shocks. In practice, firms can be financially constrained, and their ability to offer insurance may be impaired. This channel has generally received less attention,[8] but will be the focus of Section 6, that will show that stronger enforcement improves firms' ability to borrow, and stabilizes employment following negative shocks.

3 Institutional Setting

3.1 The Role of Trial Length

The maintained assumption throughout the paper is that trial length represents a suitable proxy for quality of judicial enforcement. There are several economic channels that support this link. First, and more intuitively, a higher court speed increases the value of collateral that the lender can seize in case of default, as highlighted by Visaria (2009). This affects borrowers' incentives in the bad states of the world, as they have more to lose by not exerting effort or engaging in value-destroying projects, increasing pledgeable income and reducing financing constraints as a result. Similar considerations are valid not only for bank credit but for trade credit as well. Evidence of a link between court speed and credit supply and costs in India has been provided by Visaria (2009) and Chemin (2012).

Lengthy judicial proceedings can also impact financing through banks' balance sheet.[9] A high stock of non-performing loans might impair banks' ability to raise funds. Hence, lenders might be unwilling to provide funds to firms located in districts with slow courts, anticipating that in case of default they might be unable to resort to fast resolution procedures. The connection between length of judicial proceedings and non-performing loans has been highlighted, among others, by Aiyar et al. (2015).

Beyond academia, the importance of court speed has been underscored also by policy institutions. With respect to the Italian case, the high duration of proceedings has been described by the IMF as a major factor slowing investment and growth (Esposito et al., 2014). As a results, excessive delays in court decisions are a frequent

[8] One exception is Ellul et al. (2018), who show that family firms are unable to avoid layoffs due to negative demand shocks during banking crises.

[9] To quote from the chairman of the Bank of Italy: "Further legislative provisions, in addition to those already passed, could further cut credit recovery times ... [such as] new measures on how the courts are organized.", and "A reduction of two years in credit recovery times could substantially decrease, eventually even by half, bad debts as a share of the total." (Ignazio Visco, Annual Congress of the Financial Markets Operators, 2016)

topic in news reports, not only in Italy,[10] and in policymakers' discussions, as will be documented in Section 3.5, which will present some anecdotal evidence.

3.2 The Italian Court System

The Italian civil courts system has historically been associated with a high degree of heterogeneity across different districts (Bianco et al., 2007). Its organization, in its basic form, goes back to the late 19th century, after the completion of the country unification. Some courts were suppressed in 1941, and minor revisions (with the reallocation of some municipalities across courts) were conducted until 1999. Before 2013, civil trials were conducted in 165 courts, associated with districts varying in terms of size. Appeals are instead brought to the 26 appeals courts.

The reasons for the inefficiency of the court system, and in particular of its heterogeneity across districts, have been analyzed by Bianco and Palumbo (2007), who argue for "supply side" causes. The organization of judicial districts is, for the most part, outdated. Changes in economic activity and in the population, which can in turn affect the demand for civil justice, are not reflected in the size of the districts or of the court personnel, so that several courts can be understaffed. Punishments or rewards for judges are extremely rare, so that they lack real incentives beyond their personal motivation. Court presidents are also responsible for the heterogeneity in trial length, as they can differ in their managerial skills.

The focus of this paper will be on trial length, but other aspects of court enforcement might be important as well. A concern is that heterogeneity in duration of proceedings is reflecting a speed-quality trade-off, that is, slower judges or courts could simply be more accurate in their decision-making. However, Coviello et al. (2015), using data at the individual judge-level from the court of Milan, find that judges' average speed in decision-making is uncorrelated with a common measure of decision accuracy, namely the percentage of appealed cases. Similarly, previous authors have failed to detect a significant association between average trial length and the reversal

[10] "The notoriously slow pace of Italian justice—it takes an average of more than three years to enforce a contract, compared with about a year in the U.S. according to World Bank data—is a towering problem for Italy's economy. (...) Banks struggle to resolve bad loans because bringing deadbeat debtors to court takes by far the longest in Europe. (...) Businesses stay small because they prefer to work with trusted suppliers, because a contract dispute with a new partner takes years to settle." (Source: Renzi Takes Aim at Italy's Slow Courts. *Wall Street Journal*, August 27, 2014)

rate, given by the fraction of decisions overturned in the appeals court.[11] This should also address the possibility that speed is a proxy for other judges' characteristics, for example biases, as judges that systematically exhibit inclination or prejudice for or against one of the parties in conflict (say, creditors vis-à-vis borrowers) should also be more likely to have their decisions reversed.

3.3 The 2013 Reform

A first major reform of the organization of the courts system occurred in 2012 and became effective in 2013, as a side effect of a major political turmoil. On November 16, 2011, following the resignation of the incumbent prime minister, the president of the Republic appointed a cabinet of "experts" (i.e., ministers who are not professional politicians). Shortly thereafter, on December 4, the cabinet presented a law decree with a number of measures aimed both at reassuring investors and at regaining credibility towards foreign partners during the sovereign debt crisis. Among them, the reduction of the total number of courts was considered long overdue.[12]

This reorganization led to the suppression of 26 courts. The district of each suppressed court was then merged with an adjacent district of a surviving court, with these changes becoming effective in September 2013.[13] No judges or other employees were fired; however, the reorganization was supposed to obtain savings thanks to the reutilization of the courts' facilities (often large, historical buildings).

Notably, such reorganization was not made on a case-by-case basis, but was based on ex-ante, mechanical criteria. All courts not located in provincial capitals had to be suppressed, under the constraint of keeping at least three courts for each appeals court.[14] Minor exceptions were made, due to the Direzione Nazionale Antimafia (a special prosecutor specialized in the repression of mafia-related organizations) advis-

[11] See Marco Leonardi and Maria Raffaella Rancan: La giustizia rapida è anche di qualità, *Lavoce.info,* May 15, 2009. Their analysis uses 2006 data; the Data Warehouse of the Ministry of Justice was not willing to share reversal rates with me for more recent years due to concerns about data reliability.

[12] The minister of justice was quoted as saying: "We finally reorganized the courts system, which was stuck at the time of Unification, when people were moving with horse-drawn carriages, not high-speed trains." (Source: Accorpamenti di tribunali e procure. Severino: ≪Una riforma epocale≫. *corriere.it,* July 6, 2012)

[13] Legislative sources, together with other details regarding the construction of the dataset are in Appendix A.1.

[14] A province corresponds roughly to a U.S. county and is named after the capital, usually its largest city.

ing against suppression, which prevented the elimination of six Southern courts in areas with a strong presence of organized crime. Importantly, actual courts' productivity or trends in productivity were not criteria based on which courts were suppressed (or exempted from the reform).

Panels A and B of Figure 1 provide an illustrative example from the Southern part of the Lombardy region. The two maps comprise six pre-reform court districts. After the reform, the courts of Vigevano and Voghera were suppressed and their districts absorbed by the district of Pavia; the districts of Crema and Cremona were also merged, with only the latter court surviving. Finally, the district of Lodi was unaffected. Therefore, firms originally headquartered in the districts of, say, Crema and Cremona, were exposed to a very different quality of the judiciary before the reform. However, by the beginning of 2014, they were subject to exactly the same quality of legal enforcement.

3.4 Trial Length Definition

The exact trial duration in a court is generally not observed. The proxy typically used in the literature is constructed with commonly available information on pending, incoming, and resolved cases (Palumbo et al., 2013), and is conceptually similar to a number of ratios adopted by business practitioners, such as the days sales of inventory ratio. This measure, henceforth simply called "Length", is defined as:

$$\text{Length}_t \equiv \frac{\text{Pending}_{t-1} + \text{Pending}_t}{\text{Incoming}_t + \text{Resolved}_t} \times 365 \tag{1}$$

It is important to validate the proxy for trial length defined in formula 1. While data on trial length are typically not available, the Italian ministry of justice published data on the *actual* trial length for the year 2016, which can be compared to the proxy used in this paper.[15] As Figure 2 shows, the correlation between the two measures is remarkable. A linear regression of actual on estimated length produces an R^2 of 73.04%. Nevertheless, magnitudes are off, with the empirical proxy underestimating actual trial duration by a factor of 2. In the empirical analysis, I am going to employ a log-transformation of trial length, which will make the scale of the regressor irrelevant.[16]

[15] See Bartolomeo (2017).

[16] Figure 2 shows raw numbers for ease of interpretation. The correlation between the logarithms

As an additional check, I compare the proxy for trial length with a standard measure of court efficiency employed in the literature, namely the backlog per judge (see for example Ponticelli and Alencar, 2016). This is computed by dividing the number of pending lawsuits by the number of civil court judges, which is available for the year 2015, to obtain a measure of court congestion.[17] Figure 3 shows that there is a strong association between the trial length proxy and the backlog per judge, with an R^2 equal to 64.98%. This result is reassuring because the backlog per judge is likely to be the outcome of imperfect staff allocation rather than of judges' quality in decision-making: districts can be either understaffed or overstaffed, causing heterogeneity in the timing of trial resolution.[18]

3.5 Predicting Post-Reform Trial Length

As explained in Section 3.3, 26 courts were suppressed in September 2013. Their districts were merged into the districts of 23 of the remaining courts. Therefore, the number of pre-reform districts affected is 26+23=49. More precisely, in three cases a post-reform district was the result of the merging of three pre-reform districts; in each of the other 20 cases, just two districts were merged. (The full list of pre- and post-reform districts is in Appendix-Table A1.)

Going back to Figure 1, suppose that, in 2012, we had to predict the trial length of the post-reform court. We could have used pre-reform data from the courts of Crema and Cremona on pending, incoming, and resolved trials, and simulated the trial length *as if* the two districts had always been merged. This is precisely the idea that will be exploited below.

More formally, let $x_{i,j,t}$ indicate the value of variable x at year t of a pre-reform district i that became part of district j after the reform. We can simulate the trial length of the court of a post-reform district by computing the variable $\overline{\text{Length}}_{j,2012}$,

of the two measures is almost identical ($R^2 = 71.54\%$).

[17] These data are not regularly published. I was able to extract them from a 2016 report submitted by the High Council of the Italian Judiciary to the Ministry of Justice in 2016.

[18] This mismatch between demand for civil justice and judges' supply could be due to heterogeneity in economic activity. For example, areas with a large density of firms may be characterized by a large number of business-related lawsuits. This issue is addressed by the spatial discontinuity design described in Section 3.6.

defined as

$$\overline{\text{Length}_{j,2012}} \equiv \frac{\sum_{i\in j}\left(\text{Pending}_{i,j,2011} + \text{Pending}_{i,j,2012}\right)}{\sum_{i\in j}\left(\text{Incoming}_{i,j,2012} + \text{Resolved}_{i,j,2012}\right)} \times 365 \tag{2}$$

For a firm headquartered in the pre-reform district i, therefore, the predicted change in Log(Length) $\Delta_{i,j}$ is going to be:

$$\Delta_{i,j} \equiv \text{Log}(\overline{\text{Length}_{j,2012}}) - \text{Log}(\text{Length}_{i,j,2012}) \tag{3}$$

Of course, the actual trial length of the new, larger court need not be exactly equal to the predicted one. A large court may achieve economies of scale resulting, for example, from the increased specialization of each judge;[19] alternatively, it is plausible that, at least in the first months after the reform, the reorganization of the offices may have caused some slowdown. However, I expect some characteristics of the suppressed courts to be "sticky", and thus be preserved in the new, larger districts. For example, individual judges' ability will be to a large extent unchanged after the district reorganization. As Palumbo et al. (2013) and Rodano et al. (2016) point out, judges have few incentives to be effective; hence, the quality of a court depends to a large extent on their personal motivation and on the organizational skills of the court presidents, that generate randomness in the court productivity even in areas largely homogeneous in terms of other economic outcomes.

As shown in Figure 1, the pre-reform trial lengths in the districts of Crema and Cremona were 251 and 424, respectively. As expected, the average length across the three years following the reform for the merged district lies between these two numbers, and is equal to 314. The predicted value from equation 2 above is 353, which is quite close.

There is anecdotal evidence of widespread awareness of the asymmetric effects of the reform. In a petition to the minister of justice, the mayors of the district of Crema complained that their territory was "gravely ad unjustly penalized by the closure of the Court of Crema in 2013, which was among the best ones of the country

[19] In general the assignment of judges to a case is random, but larger courts can have specialized groups, for example for labor lawsuits (in this cases the assignment would be random within each group). This may be a concern to the extent that firms in the suppressed courts, that are smaller, may disproportionately benefit from moving to a larger court, and so from having more specialized and competent judges. However, Section 5.2 shows that the effect of trial length is unrelated to whether a firm was originally under the jurisdiction of an eventually suppressed court.

in terms of efficiency." In other instances, the reference to the relative speed of the merging courts was even more direct. In the Veneto region, the court of Bassano del Grappa was also suppressed, and its district included in that of Vicenza. The mayor of the city wrote in a public letter to the prime minister in 2012 that in the court of Bassano del Grappa "one can obtain a decision on average in 2.5 years, while in the court of Vicenza it typically takes 6 years," and that "the suppression of the court would gravely penalize a community and an economic area of enormous dimensions." (Links with online sources are in the Appendix.)

3.6 Exploiting Discontinuity across District Borders

The reform was designed in such a way that contingent characteristics of the institutional or economic environment played no role in determining which courts were affected. However, confounding factors that may have differentially affected firms operating in ex-ante more efficient districts are harder to rule out. To account for this possibility, I am going to employ a spatial discontinuity-design aimed at controlling for the characteristics exhibiting variation at the local level. In practice, I will focus on firms located in municipalities near the borders of affected pre-reform districts, i.e., the colored cities in Figure 1. This approach has become quite common in the recent Law and Finance literature (see for example Ponticelli and Alencar, 2016, and Brown et al., 2017).

Intuitively, firms headquartered on the opposite side of a pre-reform district border are unlikely to be affected by different economic shocks occurring around the time of the implementation of the reform. Thus, this sample restriction, together with the inclusion of border-year dummies, should be a powerful way to control nonparametrically for omitted variables varying at the local level. In this setting, all firms are headquartered in borders of districts affected by the reform; however, borders are not necessarily shared by districts that eventually merge with each other, increasing in this way the sample size.[20]

[20] As an example, suppose that districts 1 and 2 merge, and also districts 3 and 4 merge with each other. If districts 2 and 3 are adjacent, the sample will include firms headquartered in the proximity of three district borders: 1-2 and 3-4, but also 2-3. Notice also that, for the purposes of identification, also firms headquartered in districts not subject to mergers could in principle be included, as long as they share a border with a district affected by the reform, as explained in Section 5.2. Working on this alternative sample produces very similar results. (See Appendix-Table A3.)

4 Data and Econometric Strategy

4.1 Data Sources

My source of firm level information is the Bureau van Dijk Amadeus Database, which includes accounting data on European firms. Importantly, the dataset has wide coverage of unlisted firms, which make up the bulk of Italian firms. Unlike in the US, unlisted firms have fairly strict disclosure requirements, and balance sheets of all firms need to be reported to the Chamber of Commerce. I employ the procedure recommended by Kalemli-Ozcan et al. (2015) to clean data from duplicates, missing variables, and obvious data entry errors. Following Bonetti (2016), I retain only firms with at least €1 million in both total assets and sales.[21] However, to avoid any forward-looking bias, I measure the variables at the end of the last year prior to the enactment of the reform, 2012, and, if they satisfy the size requirements, keep them in the years leading up to and following 2012. I also drop firms with missing employment, the outcome variable of interest. The dataset also indicates the municipality where each firm is headquartered. Information on firms' employment is sparse until the year 2010, so I will study the 2011-2016 window to keep the sample size stable over time.

Data on incoming and resolved cases for each Italian court is derived from the ministry of justice's website. I obtain the list of municipalities belonging to each court district before and after the reform from several legislative sources. More details on the construction of the court data are in Appendix A.1.[22]

Additional control variables, at the provincial level, are obtained by combining data from the National Institute for Statistics (ISTAT) and the Bank of Italy Sta-

[21] Kalemli-Ozcan et al. (2015) show that, compared to the Eurostat "Structural Business Statistics" and "Business Demography" databases, the coverage of Amadeus is less accurate for very small firms, so imposing a size threshold is a way to reduce selection problems. Section 5.2 shows that results look similar when imposing an even more demanding requirement, namely that firms have at least 15 employees.

[22] I follow previous authors, such as Ponticelli and Alencar (2016) and Jappelli et al. (2005), and do not attempt to distinguish between different types of civil trials. Although the data published by the minister of justice do do present some level of disaggregation, exploiting this information is problematic for several reasons. First, the classification of trials is not consistent over time. Second, it tends to be quite coarse (for example, it does not allow distinguishing between labor proceedings involving the private or the public sector). Third, detailed data are often missing for some districts, for example for property execution proceedings. Finally, disaggregated measures of trial length are very correlated with each other and with the comprehensive measure used throughout the paper. See also Section 6 for some descriptive evidence at the country level.

tistical Database. ISTAT also provides a list of bordering municipalities as of 2011, which I use for my spatial discontinuity design analysis.

4.2 Descriptive Statistics

Table 1 has descriptive statistics for the main variables used in this paper. After adopting the filters described in the previous section, I am left with an unbalanced panel of 72,982 company-years and 13,024 firms. The median firm has 15 employees. Firm size ranges from one-employee establishments to large listed firms such as Fiat-Chrysler Automobiles, with over 30,000 workers employed in the country. However, all continuous variables are winsorized at the 2.5% level (in each tail of the distribution) to account for the presence of outliers, so that the number of employees is capped at 248. (Results are essentially identical if the dependent variable is not winsorized.)

The trial length goes from 150 to 1,674 days, with a mean of 369 and a median of 307 days. Figure 4 shows the geographic distribution of the affected courts. Darker colors correspond to higher trial lengths, and so worse enforcement, and unaffected courts are left blank. Treated courts are fairly well distributed across the country, although tend to be more concentrated in the Northwest and in the South. In practice, however, the northern part of the country is the most economically developed, so that about 64% of the firms in the sample are headquartered in the Northwest. Northern courts are generally faster, but there is significant variation also within regions and, more importantly, between adjacent courts.

Because both trial length and firm employment are right-skewed, I use the logarithm of both variables in all the regressions. This transformation is convenient also because it allows for the interpretation of the regression coefficients as elasticities of employment to the average duration of proceedings. The table also shows summary statistics for Δ, the expected change in the logarithm of trial length, which has a standard deviation of 0.14, in the same order of magnitude of the standard deviation of Log(Length) (0.32).

Additional firm level variables are total debt (short-term liabilities plus long-term liabilities), leverage (total debt divided by total assets), wages (total labor costs divided by the number of employees), and return-on-assets (ROA, defined as earnings before interest, debt, and amortization divided by total assets).

4.3 Econometric Strategy

Let k index each firm, b index each border and, as before, i, j, and t index pre-reform districts, post-reform districts, and years, respectively. Then, the trial length of the district where firm k is headquartered can be predicted using the following first-stage regression:

$$\text{Log}(\text{Length}_{k,i,b,t}) = \alpha \times \Delta_{i,j} \times \mathbb{1}(t > 2013) + \gamma_{b,t} + \eta_k + \epsilon_{k,i,b,t} \tag{4}$$

Length and Δ are defined in equations 1 and 3, respectively, $\mathbb{1}$ is the indicator function, and γ and η are border-year and firm fixed effects, respectively. $\epsilon_{k,i,b,t}$ is an error term. Based on the discussion of Section 3.5, I expect the estimated sample coefficient $\hat{\alpha}$ to be positive.

Estimating equation 4 produces a predicted value $\text{Log}(\widehat{\text{Length}})$ that can be used to obtain the causal effect of trial length on an outcome y with this second-stage regression:

$$\text{Log}(y_{k,i,b,t}) = \beta \times \text{Log}(\widehat{\text{Length}}_{k,i,b,t}) + \gamma_{b,t} + \eta_k + \varepsilon_{k,i,b,t} \tag{5}$$

The estimate of the coefficient $\hat{\beta}$ is the main object of interest in the analysis. Under the assumption that the instrument $\Delta \times \mathbb{1}(t > 2013)$ is valid, it will give a consistent estimate of the elasticity of firm employment, average wage, and other outcomes to the trial length, which is expected to be negative. The econometric model does not include control variables to avoid endogeneity issues. After the main results, Section 5.2 will show some variations over the baseline results, which include controlling for time-varying industry effects or firm- and regional-level control variables.

In order to corroborate the validity of the natural experiment, it is also useful to show that the association between Δ and trial length becomes apparent only after the reform is enacted. Thus, I also run the following event study regression:

$$\text{Log}(\text{Length}_{k,i,b,t}) = \Delta_{i,j} \times \sum_{\tau=-2}^{3} \mathbb{1}(t = \tau) \times \beta_\tau + \gamma_{b,t} + \eta_k + \epsilon_{k,i,b,t} \tag{6}$$

$\tau = 0$ corresponds to the year 2013, and the coefficient β_0 is normalized to zero for convenience, so that each of the other β_τs can be interpreted as the difference between β_τ and β_0. I will also estimate event study versions of the reduced form regressions; that is, equation 6 with the outcomes of interest as dependent variables, in practice employing a difference-in-difference design with a continuous treatment variable.

Finally, notice that the instrument varies at the pre-reform court level. Accordingly, in all the tests that follow standard errors will be clustered at the pre-reform district level, following the recommendation of Bertrand et al. (2004).[23]

5 Trials Duration and Employment: Baseline Results

5.1 Main Results

Table 2 shows the baseline results on the elasticity of employment to trial length. (Other outcomes, and their sensitivity to demand shocks, will be analyzed in Section 6.) First, I include in the analysis all the firms satisfying the filters of Section 4.1, without requiring them to be headquartered near a court border. Although this sample of over 755,000 observations will not be the focus of the paper, column 1's results provide a useful benchmark. A regression of Log(employees) on Log(Length) and year dummies delivers a negative and statistically significant elasticity of firm employment to trial length. The point estimate, equal to -0.20, is also economically large.

The efficiency of the judiciary is likely to be correlated with a number of characteristics, such as local economic development, quality of the public administration, or social capital, which could, in turn, affect firm decisions. As a way to address these concerns, column 2 includes border-year dummies in the regression (as described in Section 3.6) and restricts the sample only to firms that are headquartered in municipalities near a court border, in the spirit of Ponticelli and Alencar (2016). The sample drops by about 40% and the coefficient on Log(Length) becomes now insignificant and, if anything, positive. Column 3 includes firm fixed effects to account for time invariant firm characteristics: again, the coefficient remains insignificant and very small in magnitude.[24]

[23] Clustering standard errors at the post-reform district level produces very similar results.

[24] Giacomelli and Menon (2016), focusing on a different sample period and using data at the municipality, rather than at the firm, level find a negative association between average firm employment and trial length across contiguous district borders in Italy. However, consistent with the results in column 3, this relationship disappears once controlling for municipality-fixed effects. As explained in the text, both insufficient over-time variation and reverse causality are likely to cause the difference

To summarize, columns 1 through 3 display inconclusive results. The negative correlation found in column 1 may be due to omitted variables. On the other hand, the insignificant results of columns 2 and 3, that adopt the spatial discontinuity design, may arise because of lack of power, due to trial length exhibiting low variation over time and across contiguous areas. Moreover, there is a risk of reverse causality: Growing local economies may burden the courts to enforce contracts or settle disputes, making them less productive, possibly generating a spurious positive correlation. Given this and other possible confounding factors, an instrumental variable approach is necessary to establish a causal relationship.

The regressions in columns 4 through 6 focus on the sample of courts affected by the reform (49 before its enactment) and include again firm and border-year dummies. I start, in column 4, from an OLS regression. In this subsample, where the reform is a major determinant of realized trial length, the sign of the coefficient switches to negative (-0.13) and is significant at the 5% level.

Column 5 implements the strategy described in Section 4.3 and estimates the first-stage equation 4, with Log(Length) as the dependent variable and $\Delta \times \mathbb{1}(t > 2013)$, the instrument, as regressor. The instrument exhibits, as expected, a positive association with Log(Length): A 1% increase in predicted trial duration translates into a 0.68% rise in actual length of proceedings. The t-statistic is 6.72, suggesting that the instrument is quite strong.

Column 6 estimates the second-stage equation 5 to obtain a consistent estimate of the elasticity of employment to trial length. I find a highly significant value of -0.32, with a t-statistic equal to -3.41. This number is much larger in absolute value than any of the coefficients estimated in the OLS regressions. This striking difference may be due to endogeneity, as suggested above. Moreover, trial length is generally quite slow-moving; hence, without a proper natural experiment, the degree of variation over time that can be exploited is limited. Thus, the evidence of Table 2 highlights the importance of finding an adequate instrument to tackle the endogeneity problem.

The simplest way to appreciate the economic magnitude of the results is to compare these estimates with those in previous studies that have conducted related empirical studies. Giacomelli and Menon (2016), using Italian data, find an elasticity of roughly 0.20, which does not fall in the 95% confidence interval of the coefficient in

in results between OLS and IV regressions, which are instead robust to the inclusion of firm fixed effects.

column 6 and therefore can be rejected. With Spanish data, Fabbri (2010) uses as dependent variable the logarithm of capital stock, rather than the number of employees, and finds an elasticity of firm size to trial length of about 8%.[25] Lilienfeld-Toal et al. (2012) find that a reform that reduced trial duration by establishing new specialized courts in India had no effects on employment.

As explained in Section 4.3, an event study analysis can help assess if changes in trial length captured by the instrument occur as a result of the reform, rather than predating it. I estimate equation 6 and plot the coefficients, together with 95% confidence intervals, in Figure 5. As expected, the coefficients are very close to zero and statistically insignificant for $\tau \leq 0$. Δ acquires explanatory power for the average trial length only after the reform year, with all the coefficients β_1, β_2, and β_3 being positive and significant. Overall, Figure 5 is reassuring: Districts that become more efficient after the reform do not appear to have been chosen based on their previous over- or underperformance.

Given that Δ correlates with trial length only following the reform, it is crucial that the same occurs for the outcome of interest. The coefficients are plotted in Figure 6 and display a negative relation between the predicted change in trial length and firm-level employment arising only for $\tau > 0$. The three pre-reform coefficients are all very close to zero for $\tau = -2, 1$, and fall to -0.17, -0.24, and -0.24 for $\tau = 1, 2, 3$, all significant at the 1% level.

5.2 Robustness Tests

Table 3 shows a number of variations over the baseline model of equation 5, replicated in column 1 as reference. For brevity, here and in all the tables that follow, I only report the second-stage regressions and the F-statistics. Column 2 controls non-parametrically for time-varying industry trends by adding time-industry dummies, where an industry is defined using the three-digit SIC code classification, the most refined available in the database. The point estimate slightly decreases in magnitude, from -0.32 to -0.27, but remains precisely estimated. Column 3 includes border-year-industry dummies, therefore controlling *locally* for industry shocks. Relative to column 1, the coefficient associated with the average trial length is unaffected and, if anything, estimated more precisely.

[25]The paper uses a log-linear specification, so the estimated elasticity is computed at the mean of the regressor.

The tests in column 4 and 5 control for variables that may correlate with the post-reform change in employment. Firm variables include proxies for size (Log(total assets)), financial risk (leverage), and profitability (return-on-assets). I also add economic indicators at the province level for labor outcomes (employment and unemployment rates). In column 4, these variables (not shown for brevity) are measured at the beginning of the reform year in order to avoid the “bad controls” problem (Angrist and Pischke, 2009) and are interacted with a post-reform dummy. In column 6 they are lagged and inserted directly into the empirical model. The firm-level variables are highly significant and have the expected sign: size and profitability are associated with an increase in employment, whereas leverage exhibits a negative association. Employment and unemployment rates are instead insignificant. In either case, the coefficient of interest remains large in magnitude and statistically significant.

Following the reform, judges and staff from the suppressed courts had to move to the facilities of the remaining 23 courts. Anecdotal evidence suggests that local politicians protested intensely against the closures of the courts facilities (see Section 3.5). Thanks to the spatial discontinuity-design, the firms in the sample are at approximately the same distance from the remaining court, which should already account for potentially asymmetric effects of the mergers. However, I can address this concern more directly by including a dummy equal to 1 if a pre-reform court closes down. In column 5, the coefficient is negative and equal to -0.03, but estimated quite imprecisely (t-statistic= -1.58). More importantly, the coefficient on the Log(Length) variable, instrumented as usual, is unaffected (-0.31).

The spatial discontinuity design is useful to the extent that firms headquartered in bordering municipalities are geographically close and subject to similar economic shocks. This assumption is likely to be satisfied for small municipalities. However, my sample also includes very large cities such as Genoa and Turin; firms headquartered therein could differ (and, possibly, be quite far) from firms located in smaller bordering municipalities. Therefore, in column 6, I consider only firms headquartered in small cities by excluding firms located in the 11 provincial capitals present in my sample. In this exercise, the sample size drops by about 40%, but the point estimate of the coefficient on Log(Length) remains large and significant (-0.27).

In columns 7 and 8 I test whether these results are driven just by small firms. Small firms may be more financially constrained; therefore a more effective judiciary may increase their ability to obtain funds. For example, Chodorow-Reich (2013) finds that the employment effects of credit market disruptions during the Great Recession were concentrated among small firms. However, Farre-Mensa and Ljungqvist (2016) argue that size is not a suitable proxy for financial constraints. Indeed, Chaney et al. (2013) find that the effect of an increase in collateral value on employment is more pronounced for large firms.

Column 7 includes firms with at most 15 employees (the sample median), and column 8 includes all the other firms. (Firm employment is, as before, measured at the beginning of the reform year.) The coefficients associated with trial length are both negative and significant (-0.32 and -0.42, respectively), and are not statistically different from each other. This result is also important regarding the economic channel driving this evidence. Firms with no more than 15 employees can fire workers "at will" and, hence, face minimal risk of lawsuits following dismissals.[26] Thus, if firing costs were driving the results, we would expect to find no effects in this group of firms.

The main working sample includes only firms headquartered in districts subject to the reform. However, the sample can be extended to firms operating in districts that did not merge, as long as they share a border with nontreated districts. I run the baseline tests of Table 2 on this larger sample and report the results in Appendix-Table A3, where Δ is set to 0 in districts not affected by the reform.[27] The inclusion of both treated and untreated districts is not ideal because it increases the heterogeneity of the sample and weakens the predictive power of the instrument, with the F-statistic that drops from 49.68 to 22.22.[28] Nevertheless, the point estimates and the significance levels are almost identical to those obtained on the restricted sample, also when revenues or total compensation, that will be analyzed in Section 6.6, are used as dependent variables.

A last point worth noting is that the analysis so far has exploited within firm variation. However, there could also be effects on the extensive margin, because the

[26] Since the enactment of 1970 *Workers' Statute*, small firms (that is, firms with at most 15 employees) have enjoyed significant flexibility in firing decisions. Employment protection was progressively reduced for larger firms (Boeri and Garibaldi, 2019) although, as shown in the next Section, the evidence on judicial enforcement is unrelated to the effects of these labor market reforms.

[27] Instead, firms headquartered across borders of two districts not affected by the reform need to be excluded because of collinearity between the instrument and the border-year dummies.

[28] In unreported tests, I find that trial length drops by about 4.8% on average in affected courts, relative to the others. This is in line with the reform's objective, and suggests that untreated courts are unlikely to constitute a proper control group.

positive impact of judicial enforcement on firm size could, in principle, be compensated by an increase in the number of firms closing, or by a drop in entry. I obtain data on firm entry and exit for each year and municipality from Infocamere, a database maintained by the Chamber of Commerce.[29] Given that many municipalities are extremely small and exhibit very little variation over time, I aggregate the number of firms created and closing at the district border level, so that a unit of observation is a district border-year. The first-stage regression in column 1 shows that also in this setting the instrument retains its statistical power (t-statistic$= 7.70$). Column 2 shows that higher trial length appears to slow down firm creation, with an elasticity of firm entry to trial length of -0.43 (t-statistic$= 2.32$), whereas the coefficient becomes small and insignificant when the dependent variable is the logarithm of firm destruction, in column 3. Hence, the increase in firm employment is not related to a reduction in the number of active firms; if anything, enforcement seems to spur not only firm growth, but also entrepreneurship.

5.3 A Placebo Test

While there is substantial heterogeneity in the pre-reform average trial length, an unfortunate feature of this setting is that the reform affected all the treated courts at the same time. Suppose that a nationwide shock hit the economy at around the time that the court reform was implemented and that, for some reason, it increased employment in areas where courts were slower. Then, even if the reform did not have any real effects, the instrument would still pick up post-reform outcomes because, by construction, Δ tends to be larger in ex-ante more inefficient courts.[30]

I propose a simple "placebo" test, exploiting the fact that most courts are unaffected by the reform. From the set of 165 pre-reform courts, I first exclude the 49 involved in the reorganization. Then, I randomly select 26 courts and simulate a merger with an adjacent court, also chosen at random. For each pre-reform court, I construct the variable Δ derived in equation 3, and estimate a reduced form regres-

[29] I would like to thank Infocamere for providing me with the data.

[30] Two labor market reforms, in 2012 and 2015, limited the possibility of reinstatement after an "unjustified" firing for firms with more than 15 employees (reinstatement was never required for small firms). Because reinstatement had to be mandated by a court, these provisions could have spurred employment in slower courts; however, the evidence presented in this section suggests otherwise, implying that the reforms are not contaminating the effect of the courts reorganization. They were, however, successful in reducing the number of judicial proceedings involving dismissals in the private sector (see Section 6 and Figure 8).

sion where Log(employees) is regressed on $\Delta \times \mathbb{1}(t > 2013)$, firm and border-year dummies. I repeat this procedure 10,000 times and plot the cumulative distribution function of the estimated coefficients.

As shown in Figure 7, results are reassuring. There is very little mass to the left of the coefficient estimated with a reduced form regression on the baseline dataset (equal to -0.22); more precisely, only 6 out of 10,000 simulations generate a coefficient lower than the one produced by the actual reform. Finally, the average coefficient estimated from these "placebo reforms" is -0.003, which is very close to zero and much smaller in absolute value than the coefficient estimated on data with treated courts, suggesting that the results presented in this paper are unlikely to be due to any nationwide shock that occurred at the same time as the reform.

6 The Economic Channel

The previous section has established the presence of a positive link between quality of legal enforcement and employment level, that is compatible with at least two theoretical frameworks.

First, empirical research (Autor et al., 2006; Besley and Burgess, 2004, among others) has documented that employment protection can result in lower equilibrium employment. Intuitively, employment protection can dampen workers' reallocation across firms, possibly leading to lower productivity and, thus, lower employment. Laid off workers can generally sue their employers, for example if they perceive dismissals as being discriminatory. Recent work by Bamieh (2016) shows that longer duration for labor lawsuits translates into higher firing costs for three reasons. First, late resolution of uncertainty is, in all respects, a cost for the firm (Bloom, 2009). Second, Italian layers are paid according to the time spent on a case. Third, reinstated workers are paid all forgone wages from the day of dismissal.[31]

Another possibility is that firms may partially offset the impact of negative demand shocks by resorting to external financing. The moral hazard problem becomes more severe following a negative shock, that may reduce pledgeable income and induce risk-shifting behavior; thus, enforcement will be especially valuable in protecting investors and, hence, reducing financing costs. Theoretical work (Baily, 1974; Azari-

[31] Importantly, Bamieh (2016) also shows that slower judges are not more likely to rule in favor of firms.

adis, 1975; Holmstrom, 1983) suggests that firms may reduce labor costs, and increase profits, by offering workers "implicit contracts," where, under some assumptions, they can smooth the impact of demand shocks on employment. A crucial assumption of this class of models is that shareholders have better access to capital markets than workers. While cuts in the workforce might be optimal following a negative shock, financial constraints might impose an even sharper reduction in the employment level. Because this protection is valued by risk averse workers, they will require lower compensation, making hiring cheaper and raising employment in equilibrium.

Importantly, while these two explanations are both compatible with higher employment levels, they make opposite predictions regarding the *sensitivity* of employment to demand shocks. In the first scenario, lower firing costs due to improved judicial enforcement should increase employment fluctuations (Blanchard and Katz, 1997; Simintzi et al., 2014; Serfling, 2016; Bai et al., 2019). In the second scenario, access to financing should allow managers to avoid large cuts in employment levels and honor implicit contracts, in turn being able to pay lower wages. Of course, while both channels may be in place, either one of the two could prevail in practice.

A first piece of descriptive evidence shows that firing costs are unlikely to play a large role in firms' hiring decisions, at least for the period studied here. Figure 8 plots the number of firing proceedings initiated in the years 2012–2016 for the entire country. Over this period, the yearly number of trials drops by 69%, from 27,592 to just 8,580, as the ability to bring an employer to court following a dismissal has been substantially reduced over time by a series of labor market liberalizations. In marked contrast, bankruptcy trials, that involve conflicts between debtholders, rather than workers, and firms, are much more numerous and fairly constant, oscillating between 52,000 and 62,000 per year.[32]

[32] Publicly available date do not distinguish between different types of labor lawsuits. The data plotted in the figure were requested and obtained by a member of the Senate, Pietro Ichino, through a request to the minister of justice (see "Licenziamenti, crolla il contenzioso tra lavoratori e aziende," *corriere.it,* February 17, 2017), and include only firing proceedings in the private sector. They are not available before 2012. Data on bankruptcy lawsuits include four types of proceedings: private foreclosure, foreclosure endorsed by the court, reorganization, and liquidation. The point made in the text would be strengthened if other proceedings were included, for example those related to property execution, that also involve conflicts between debtholders and borrowers and range between 350,000 and 550,500 every year (see Schiantarelli et al. (2016), who focus on this kind of proceedings).

6.1 Demand Shocks and Employment

I follow Sraer and Thesmar (2007) and define, for each firm k and year t, a demand shock as the logarithm of the average sales of the firms in the same industry, excluding firm k from the computation. Taking averages rather than sums avoids the risk of mismeasurement due to attrition. Then, I estimate the following model:

$$\begin{aligned} \text{Log}(y_{k,i,b,t}) = \alpha \times \text{Log}(\widehat{\text{Length}}_{k,i,b,t}) + \beta \times \text{Log}(\widehat{\text{Length}}_{k,i,b,t}) \times \mathit{Shock}_{k,t} \\ +\gamma \times \mathit{Shock}_{k,i,b,t} + \gamma_{b,t} + \eta_k + \varepsilon_{k,i,b,t} \end{aligned} \tag{7}$$

where $\text{Log}\widehat{(\text{Length})}$ is the projection of Log(Length) on the instrument, firm dummies, and border-year dummies estimated from equation 4, *Shock* is the industry demand shock, and y is an outcome variable. In this regression the coefficient of interest is β, which is associated with the interaction term given by the projection of Log(Length) and the sale shock. This procedure follows Duchin et al. (2010).[33]

In Table 5 the sale shock is demeaned and divided by its standard deviation for ease of interpretation. Column 1 shows that a one-standard deviation shock in industry sales determines an 8% increase in firm employment. However, this sensitivity is larger in firms operating in districts with weak enforcement, supporting the implicit contract channel, as shown by the positive sign of the coefficient β, equal to 0.18 and significant at the 1% level.

In a more direct test of the implicit insurance hypothesis, I distinguish between positive and negative demand shocks. Intuitively, workers will be concerned about negative shocks that may significantly increase the likelihood of layoffs, rather than positive ones. In columns 2 and 3 I split the sample according to whether the shock is above or below the sample industry median. Columns 2 and 3 show that the effect found in the baseline result of column 1 is fully driven by negative shocks. The coefficient on the shock variable is 0.10, and the interaction coefficient with $\text{Log}\widehat{(\text{Length})}$ is 0.33, significant at the 1% level. To interpret the magnitudes, consider that a 10% reduction in predicted trial length would dampen the impact of a negative shock on employment by a third $(0.03/0.10 = 0.33)$. Column 3 shows that, instead, both the stand-alone demand shock and the interaction term do not enter significantly

[33] Alternatively, one could adopt a proper IV strategy, where the instrument $\Delta \times \mathbb{1}(t > 2013)$ and its interaction with the demand shock are used as instruments for Log(Length) and its interaction. Unfortunately, this approach performs very poorly in this setting, with an F-statistic well below conventional values.

in the equation. Hence, for firms hit by positive demand shock, judicial enforcement is largely irrelevant.

6.2 Demand Shocks and Debt Financing

Having established that firms' can partially insulate workers from shocks in presence of strong judicial enforcement, this section tests whether this ability stems from their superior access to capital markets. An implication of the theory would be that firms protected by better enforcement are more able to raise financing to counterbalance the drop in cash flow due to industry downturns.

As a simple test of this theory, Table 6 replaces the dependent variable in equation 7 with the logarithm of total firm debt. The focus on debt is motivated by the evidence in Beck et al. (2008), who find that, in a cross section of country, the use of debt by small firms is especially sensitive to the enforcement of property rights. On the other hand, equity providers in small, unlisted firms, tend to be the managers themselves; hence, the moral hazard problem that can be ameliorated by judicial enforcement is less significant. Column 1 shows that a sale shock is positively related to the ability of a firm to raise debt ($\gamma = 0.05$). However, this elasticity is amplified for firms in districts with poor enforcement: the coefficient on the interaction term is 0.15, significant at the 1% level.

As before, columns 2 and 3 test whether the coefficient on the interaction term varies depending on the direction of the demand shock. The results mirror those of Table 5, also in terms of economic magnitudes. While the interaction term enters significantly and with a large and positive coefficient when the demand shock is negative (0.22, significant at the 1% level, in column 2), judicial enforcement does not appear to affect the sensitivity of debt to positive industry shocks (column 3).

6.3 The Price of Insurance: Courts and Wages

In an implicit contract the employer commits to insulate the worker from external shocks; in return, she can pay a lower wage. Better access to financing during downturns allows firms to reduce the impact of negative demand shocks, and should result in lower wages in equilibrium for firms operating in districts characterized by good judicial enforcement. A caveat is that these predictions do not necessarily apply to workers hired after the courts reorganization because of the possibility of assortative matching (Abowd et al., 1999): Firms able to offer higher job stability may be more attractive, and workers with higher reservation wages may sort into them. While this effect may be important in the long run, in the short run average wages should mainly be determined by the compensation of workers hired previous to the reform; hence, this sorting effect may mitigate, but is unlikely to fully offset, evidence of a negative relationship between average wages and judicial enforcement.

In Table 7 I regress average wage, approximated by total staff expenses divided by the total number of employees, on Log(Length) and the usual set of fixed effects in an IV regression. In column 1 the coefficient on Log(Length) has the expected positive sign, although it is imprecisely estimated (0.08, significant at the 10% level).

This wage premium should be large only in firms where private insurance provided by firms is valuable, namely in firms exposed to fluctuations in demand. Columns 2 and 3 distinguish firms according to a standard measure of earnings volatility, given by the standard deviation of earnings changes divided by average total assets (Brealey et al., 1976; Matsa, 2010). Intuitively, firms subject to frequent changes in profitability should disproportionately benefit from the ability to borrow and sustain employment during periods of low demand, reducing average wage expenses.

In column 2, where only firms in the bottom tercile of earnings variability are included, the effect of judicial enforcement on wages is small (0.01) and insignificant. On the other one hand, the coefficient rises in firms in the top tercile of earnings variability (0.14, significant at the 5% level). The difference between the coefficients across subsamples is significant at the 5% level.[34]

An alternative test uses as a proxy for volatility the standard deviation of the average industry sales, a measure that can naturally be traced to the industry demand shocks used in the previous section. Columns 4 and 5 present evidence again consistent with the hypothesis that judicial enforcement leads to lower wages only in firms more exposed to demand shocks. The coefficient rises from an insignificant -0.02 to a large and significant 0.23 in firms with high industry sales variability, with the difference between the two coefficients significant at the 1% level.

[34] Both this and Section 6.4 that follows analyze firm heterogeneity using tercile sorting, following, among others, Chaney et al. (2012) and Farre-Mensa and Ljungqvist (2016).

6.4 Leverage, Financial Dependence, and Local Unemployment

This section and the results of Table 8 explore three layers of cross-sectional heterogeneity of the effect of courts on employment. Section 6.1 has shown that lack of judicial enforcement can reduce the ability of obtaining financing following negative demand shocks. This channel could play little role in firms that are financially unconstrained; hence, the first test analyzes the heterogeneity in terms of the *cost of financing*. Benmelech et al. (2011) show that there is a stronger relationship between employment and cash flow in highly levered firms, that is, those that may struggle obtaining external funds. A similar reasoning applies here: Suboptimal judicial enforcement may render debt renegotiation in troubled firms difficult (Rodano et al., 2016); moreover, it may induce opportunistic behavior by firms' managers towards strategic default (Jappelli et al., 2005; Schiantarelli et al., 2016). A more effective judiciary should alleviate these problems, and more so if firms have a high debt burden. Columns 1 and 2, that sort firms according to their leverage (measured at the end of 2012, the last pre-reform year, to avoid endogeneity concerns), show that this is indeed the case. The coefficient is significant in both cases, but rises in magnitude from -0.23 to -0.63 when moving from firms with low leverage to firms with high leverage. The difference is large and significant at the 5% level.

Firms may differ not only in terms of their financing cost, but also in terms of their *financing needs*. In the spirit of Rajan and Zingales (1998), I construct a proxy for financial dependence, defined as the difference between investment and cash flow, all divided by investment.[35] I compute this measure for each firm across the years 2008-2012 and then take the industry median to construct an industry proxy for financial dependence. As expected, the effect of enforcement is larger in industries with higher financial dependence. The coefficients of interest are -0.18 and -0.59 in the low and high financial dependence industries, respectively, and their difference is -0.43 (significant at the 1% level).

Sections 6.1 and 6.3 have shown that a significant advantage of improving enforcement lies in the ability to insulate workers from shocks. In the spirit of Agrawal

[35] Amadeus does not report capital expenditures, so I follow Acharya et al. (2018) and compute investment as fixed assets minus lagged fixed assets plus depreciation, setting negative values to zero. Because the financial dependence measure has investment as the denominator, only firms with strictly positive levels of investment are kept.

and Matsa (2013), the last test compares firms according to measures of *unemployment cost*. To the extent that outside options are scarce, the job stability indirectly provided by the enforcement of property rights should be especially attractive to employees. As a proxy for the status of the local economy, and so of the welfare loss that an individual may suffer from a layoff, I use the unemployment rate at the province level. Columns 5 and 6 show that, while the effects of judicial enforcement is indistinguishable from zero in areas with low unemployment, the coefficient is equal to -0.54 in areas with high unemployment, with the difference, -0.58, significant at the 1% level.

6.5 The Effect of Capital-Labor Elasticity of Substitution

An important channel through which judicial enforcement affects employment is via its impact on financing constraints. This connection suggests a role for the degree of substitutability between capital and labor. Access to financing granted by better enforcement should spur employment in firms where capital and labor are complement, but this link could be muted, or even reversed, the easier it easy to replace labor with capital.

The results so far suggest that complementarity dominates in this setting, but they might not extend to economies characterized by a different industry mix. To investigate the role of the firm production function, I explore the heterogeneity of the results with respect to the elasticity of substitution between capital and labor. I use estimates recently obtained by Oberfield and Raval (2014) for the US manufacturing sector using plant-level evidence. To the extent that sectors' technologies are not too different across countries, these estimates should be a good approximation of the capital-labor elasticity of substitution across Italian industries.

Column 1 of Table 9 shows the baseline regression estimated over the sample of manufacturing firms, for which the elasticities of substitution are available. Although the sample size drops by almost two thirds, both the point estimate (-0.30) and the statistical significance are similar to those obtained in the full sample (Table 2).

Oberfield and Raval (2014) compute elasticity of substitutions at the 3-digit NAICS level for the years 1997, 2002, and 2007.[36] I take the average for each in-

[36] See Oberfield and Raval (2014) (Online-Appendix C). The paper produces three sets of estimates, which use Census based wages, Longitudinal Business Database wages, and Census based wages employing an instrumental-variable procedure. I use the former, but results are very similar with

dustry and sort firms in two groups, according to whether they belong to the bottom or top half in terms of elasticity of substitution.

Column 2 (the "Low" subsample) shows that industries where labor and capital have a high complementarity display a strong sensitivity of firm labor to enforcement (coefficient equal to -0.41), suggesting that a relaxation of financing constraints induced by a stronger enforcement translates into an increase in employment. However, in industries with high substitutability (subsample "High", column 3) the coefficient drops to an insignificant -0.19. Although the difference between the two coefficients is not significant, likely because of the small sample size, it is economically sizeable. To summarize, the impact of a change in judicial enforcement appears to depend on firm technology, and is likely to have heterogeneous welfare effects across industries and, thus, firms and workers.

6.6 Additional Outcomes

If judicial enforcement spurs employment, overall real activity, and thus revenues, should follow the same pattern. Table 10 estimates equation 5 with Log(sales) as the dependent variable. As expected, there is a negative relationship between revenues and trial length. The coefficient is -0.14, significant at the 10% level. The elasticity of sales to trial length is equal to about 40% of the elasticity of employment; however, visual inspection of the reduced form event study regression is particularly useful here. Figure 9 shows that sales do respond quite strongly to the change in trial length induced by the reform; they do so, however, with some delay. Therefore, it appears that, as law enforcement improves, firms immediately hire workers to increase capacity. It takes some time, however, for firms to also adjust their production.

In column 2 the dependent variable is the logarithm of the total wage bill at the firm level. The estimated coefficient is now highly significant and suggests an elasticity of total wage payment to trial length of -0.21. The fact that the coefficient is smaller in magnitude is a mechanical consequence of the evidence of a negative relationship between wages and judicial enforcement presented in Section 6.3. The event study plotted in Figure 10 provides clean evidence that the change in the wage bill is due to the reform and does not anticipate it.

Section 6.2 has shown evidence that the debt level is more sensitive to demand

the other estimates. Notice also that, unlike the previous sections, Table 9 does not employ tercile sorting but simply cuts this smaller sample in half to preserve statistical power.

shocks in districts with poor enforcement. The results of column 3, where the dependent variable is the logarithm of total debt, show that the coefficient on Log(Length) is negative but insignificant. Similarly, leverage also does not appear to be related to judicial enforcement (column 4). This lack of relationship is consistent with international evidence on developed (Rajan and Zingales, 1995) and developing (Booth et al., 2001) economies. In addition, Fabbri (2010) finds no association between corporate leverage and the length of trials in a sample of Spanish firms, and shows, theoretically, that although stronger enforcement reduces the cost of credit, in general equilibrium trial length and leverage need not be related. The evidence presented in this paper shows that these results hold up even after controlling for endogeneity. However, the results in Section 6.2 add the important caveat that, while the level of debt *unconditionally* is not related to judicial enforcement, the sensitivity to demand shock is.

Finally, I test for the possibility that, if employment is positively affected by an improvement in enforcement, this expansion in size may come at the cost of lower firm profitability. In column 6 of Table 10 the dependent variable is return-on-assets (ROA); there is no evidence that this is the case. Event study evidence for total debt, leverage, and ROA is left for brevity to Appendix A.3.

7 Conclusion

This paper has presented evidence on the consequences of effective judicial enforcement by showing that changes in average duration of judicial proceedings, a typical measure of enforcement, have a first order impact on labor and financing policies. Characteristics of the institutional setting of the natural experiment studied, a reorganization of the judicial districts, as well as a number of robustness tests, provide reassuring evidence on the causal nature of the results.

I find that improving enforcement boosts firm employment level and reduces the sensitivity of employment to negative demand shocks. The growth in firm size does not come at the cost of firm entry; if anything, entrepreneurship benefits as well from stronger enforcement. The provision of implicit insurance originates from the possibility of obtaining debt financing during downturns. As a result, firms headquartered in districts with better enforcement become more attractive to workers, who are willing to accept lower wages in exchange for a reduction in layoff risk in firms or industries characterized by high volatility, either in earnings or industry sales.

The economic impact of courts is magnified in firms characterized by high financing costs, in industries that have high dependence from external finance and high complementarity between labor and capital, and provinces where implicit insurance should be more relevant, such as those where the unemployment rate is high.

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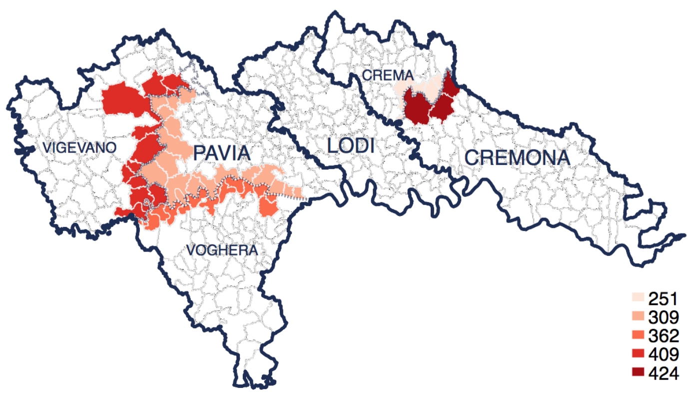
8 Figures and Tables

Figure 1
Trial Length and the Effects of the Reform: A Case Study

Panels A and B of Figure 1 show choropleth maps of trial length and predicted post-reform change in trial length, respectively, in six pre-reform districts from the Lombardy region. Darker versions of red correspond to larger trial lengths (Panel A) or changes in trial length (Panel B). Municipal areas not located near the borders of pre-reform districts are left blank. The courts of Vigevano and Voghera were suppressed, and their districts were absorbed by the district of Pavia; similarly, the district of Crema was absorbed by the district of Cremona. The district of Lodi was unaffected.

Panel A. Trial Length



Panel B. Predicted Change in Trial Length

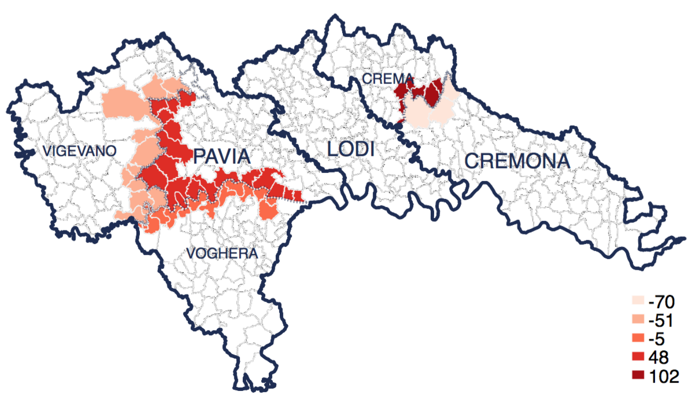


Figure 2
Actual Versus Estimated Trial Length

Figure 2 shows a scatter plot of actual versus estimated trial lengths across Italian districts in 2016. Estimated length is obtained using the formula $\text{Length}_{\text{t}} = (\text{Pending}_{\text{t-1}} + \text{Pending}_{\text{t}})/(\text{Incoming}_{\text{t}} + \text{Resolved}_{\text{t}}) \times 365$. Coefficients of a regression of actual length on estimated length are also reported, together with heteroscedasticity-consistent standard errors. The red line plots the estimated regression function.

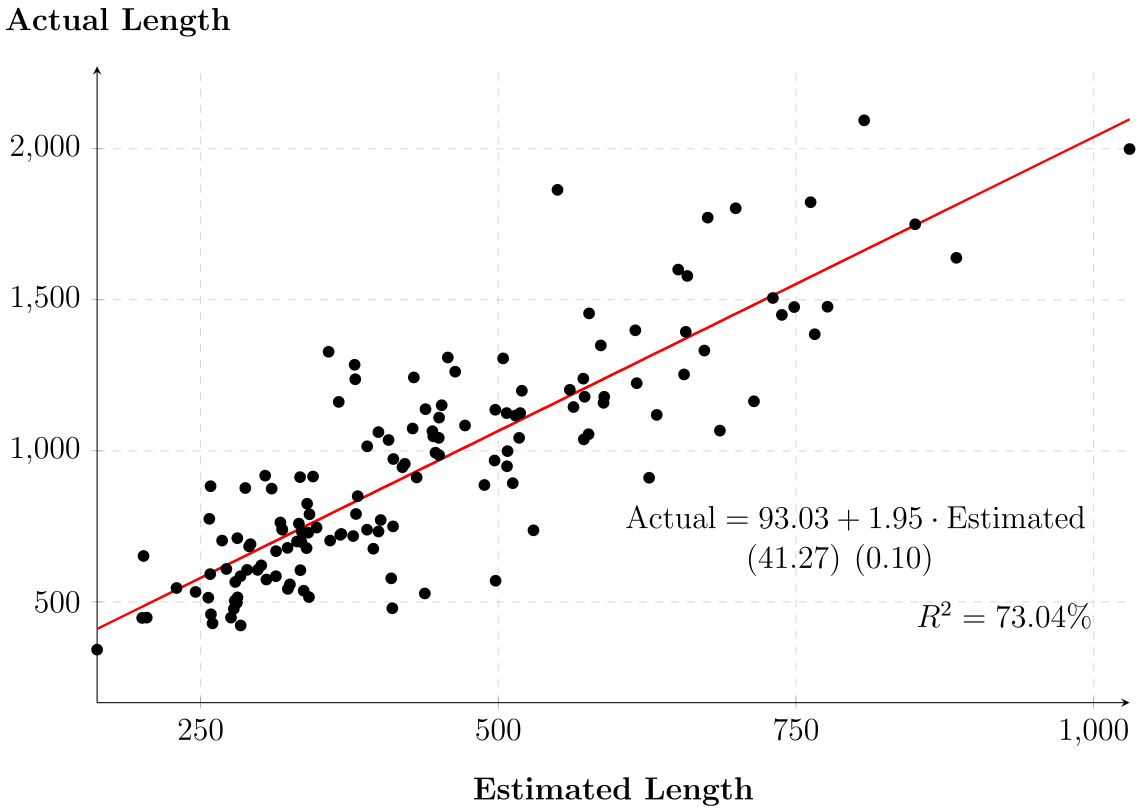


Figure 3
Trial Length Versus Backlog per Judge

Figure 3 shows a scatter plot of trial length versus backlog per judge across Italian districts in 2015. The backlog per judge is given by the number of pending cases divided by the number opf civil court judges. Coefficients of a regression of trial length on backlog per judge are also reported, together with heteroscedasticity-consistent standard errors. The red line plots the estimated regression function.

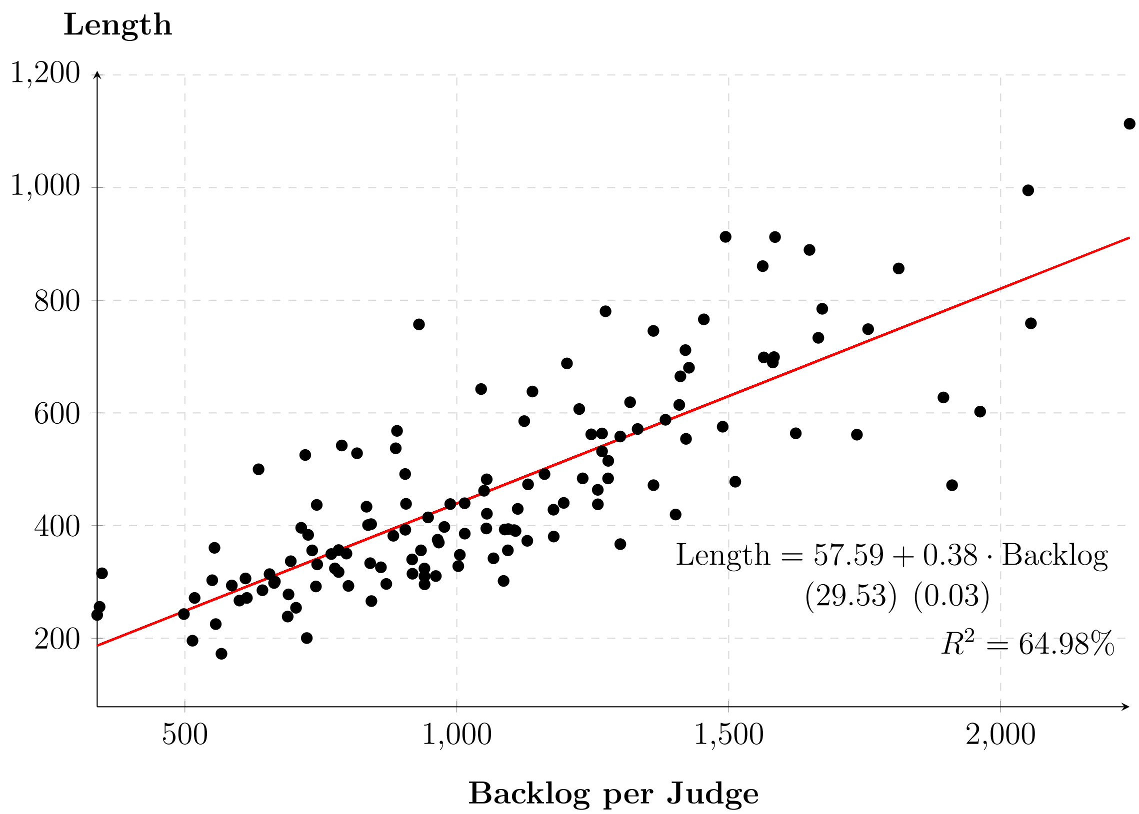


Figure 4
Trial Length across Italian Districts

Figure 4 shows a choropleth map of trial length across Italian districts as of 2012. Darker colors correspond to higher trial lengths.

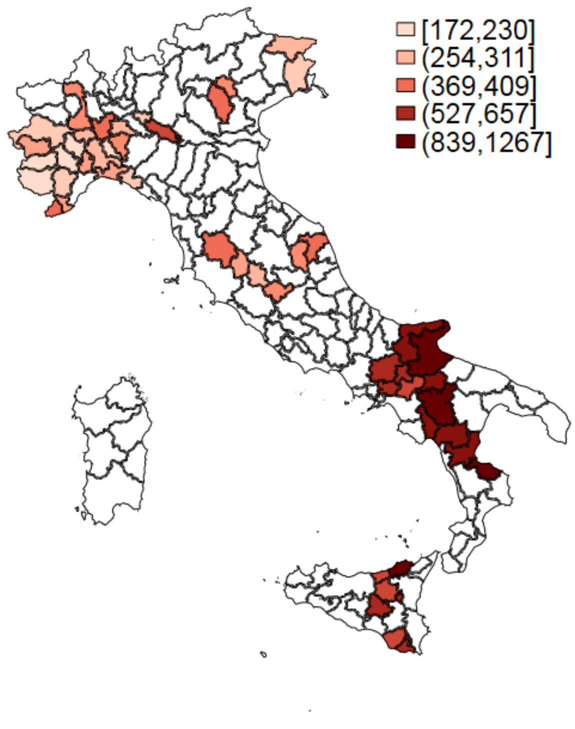


Figure 5
Event Study: Log(Length)

Figure 5 shows coefficients estimated from regressing Log(Length) on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero.

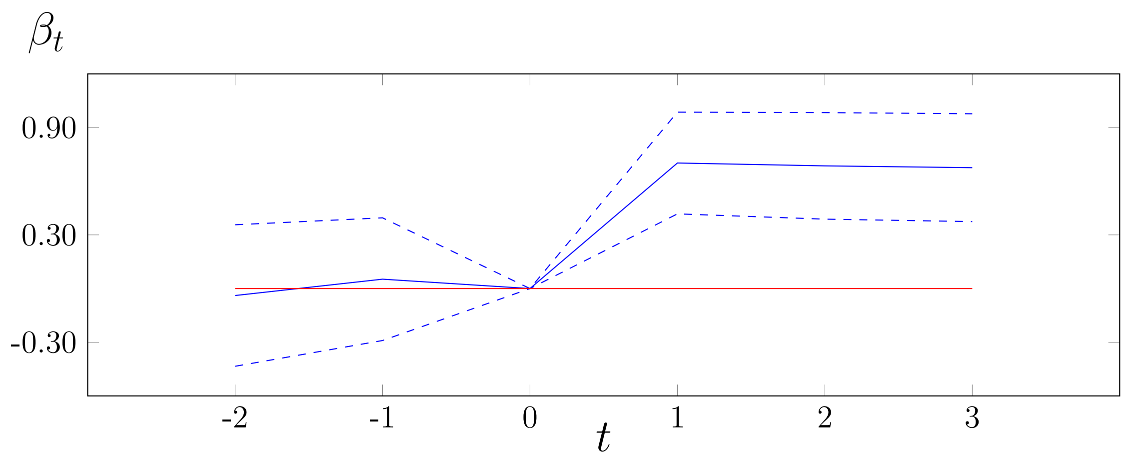


Figure 6
Event Study: Log(Employees)

Figure 6 shows coefficients estimated from regressing Log(employees) on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero.

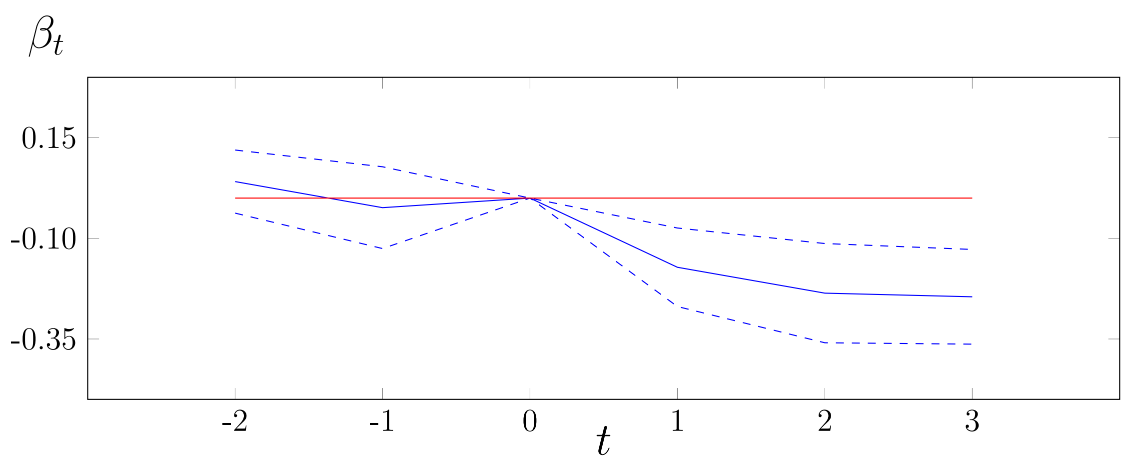


Figure 7
Placebo Test

Figure 7 plots the cumulative distribution function of coefficients obtained from simulating 10,000 placebo courts mergers, as explained in Section 5.3. The $\hat{\beta}$ coefficients plotted are estimated from a first-stage regression of Log(employees) on the instrument, border-year dummies, and firm dummies. The red vertical line indicates the first-stage coefficient from the true natural experiment.

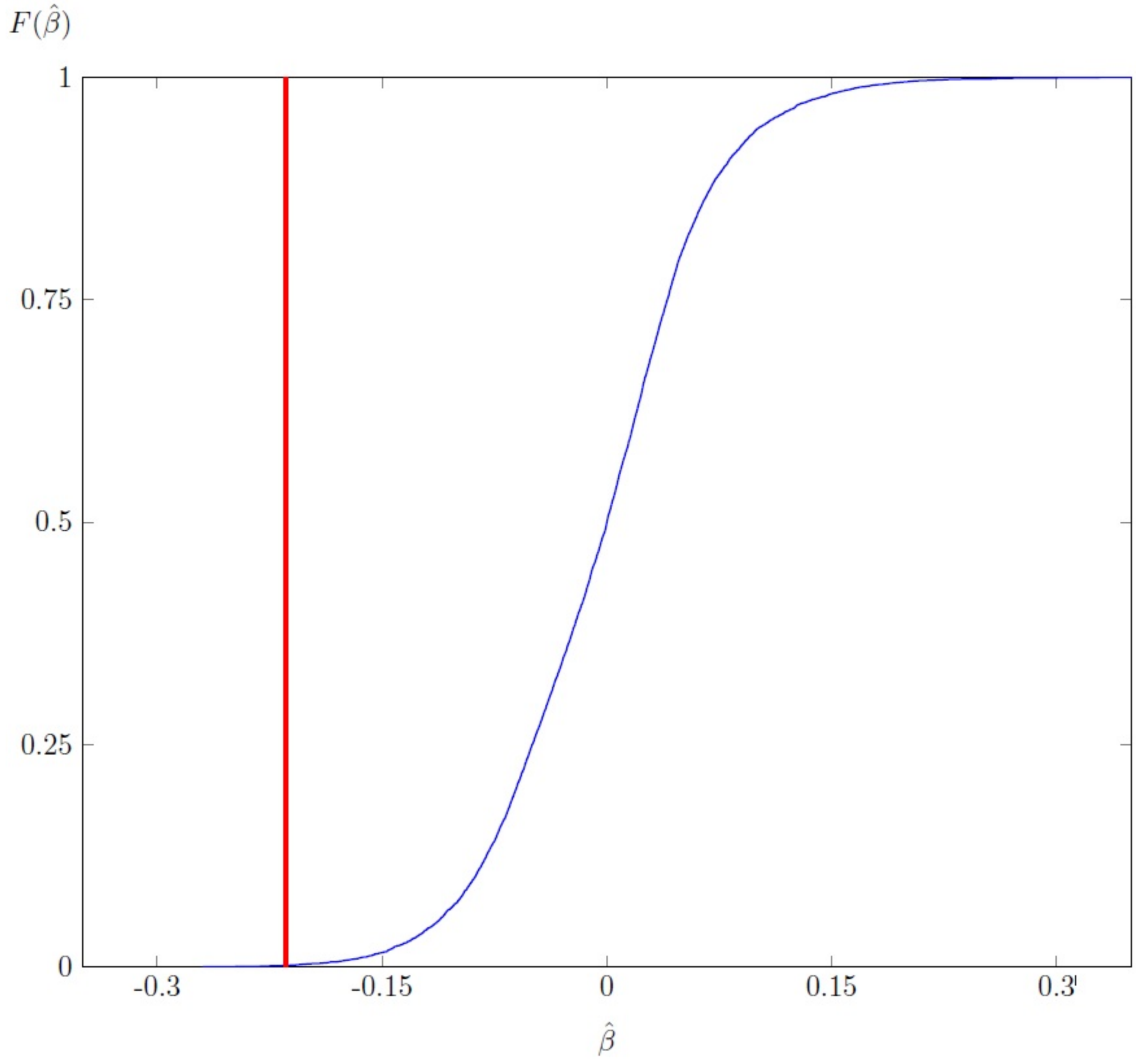


Figure 8
Bankruptcy and Firing Proceedings

The histogram plots the number (in thousand) of incoming firing and bankruptcy proceedings for the years 2012-2016, in blue and red, respectively.

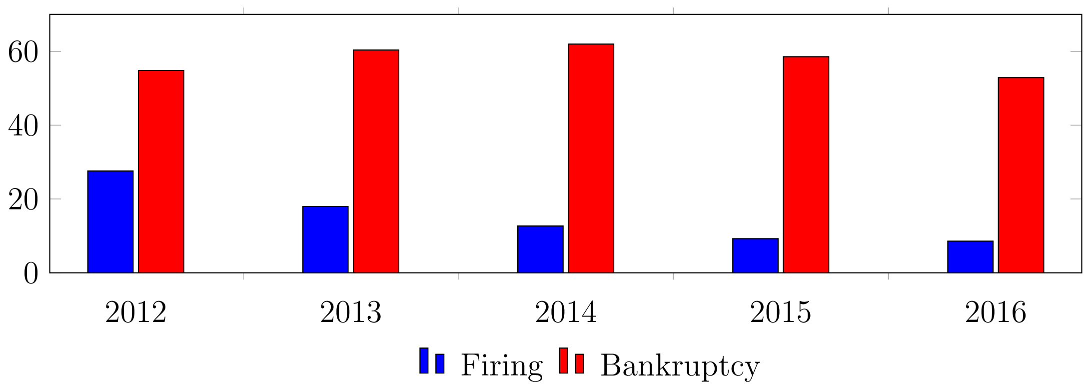


Figure 9
Event Study: Log(Sales)

Figure 9 shows coefficients estimated from regressing Log(sales) on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero.

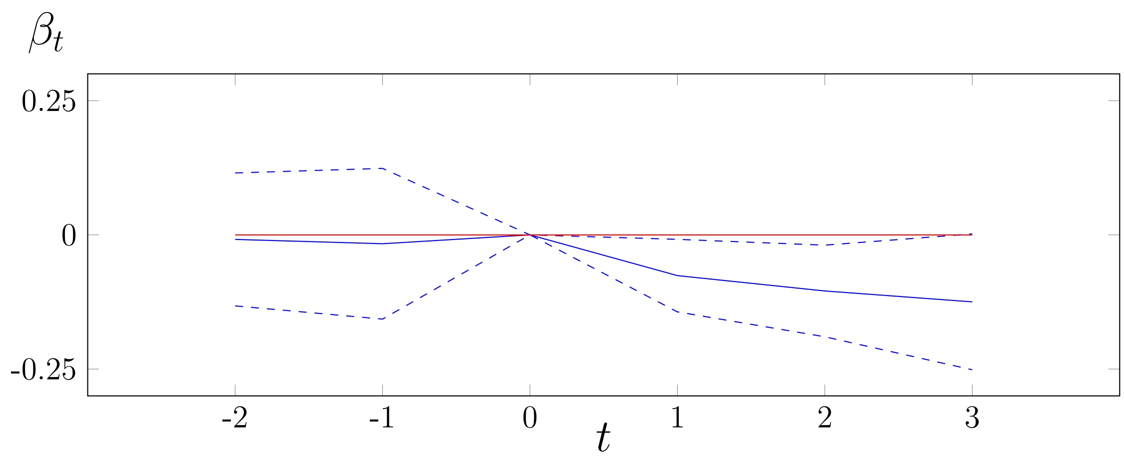


Figure 10
Event Study: Log(Wage Bill)

Figure 10 shows coefficients estimated from regressing Log(Total Pay) on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero.

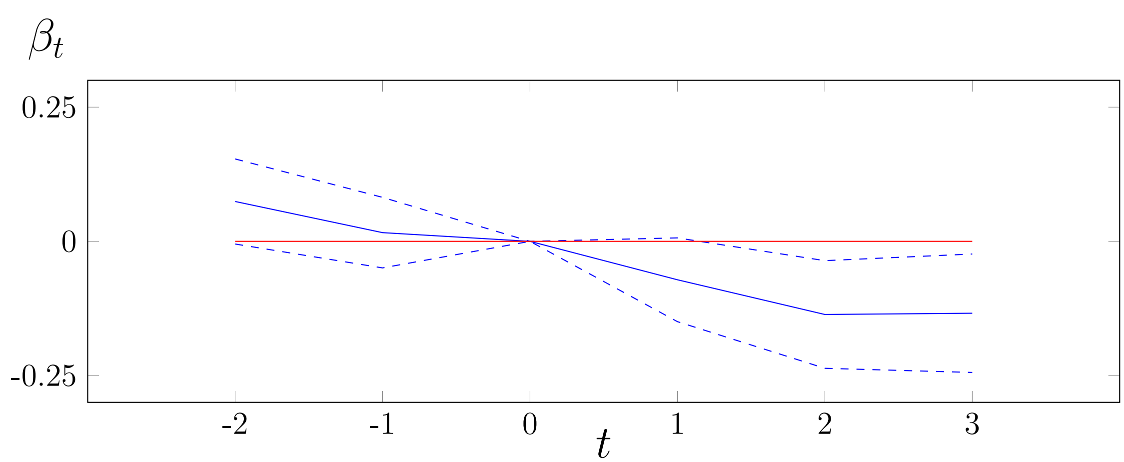


Figure 11
Event Study: Log(Average Wage)

Figure 9 shows coefficients estimated from regressing Log(Wage) on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero.

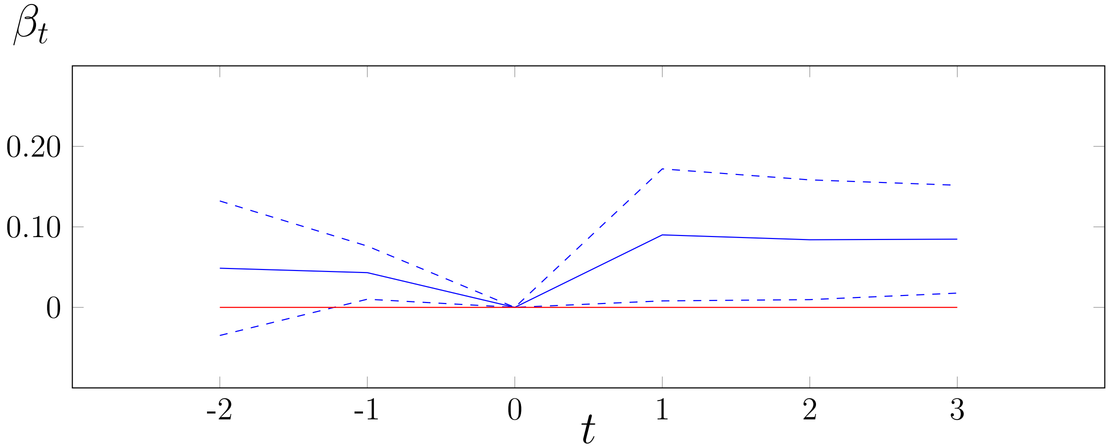


Table 1
Descriptive Statistics

Table 1 has descriptive statistics for the main variables used in the paper. Wage is the total cost of employees divided by the number of employees (in thousand euros). Log(Length) is the logarithm of the average trial length. Total debt is given by non-current liabilities plus current liabilities. Leverage is defined as total debt divided by total assets. Return-on-assets is earning before interest, debt and amortization divided by total assets. Log(Length) is the average duration of civil proceeding. Δ is the predicted post-reform change in Log(Length).

	N	Mean	Median	St. Dev.	Min	Max
Firm Variables						
Employees	72,982	31.64	15.00	48.08	1.00	248.00
Sales	72,982	8.33	3.31	14.09	0.63	73.82
Wage	72,854	38.40	36.61	14.69	12.38	82.29
Total Debt	72,857	5.95	2.35	10.15	0.50	53.30
Leverage	72,861	0.73	0.78	0.22	0.20	1.07
Return-on-Assets	72,269	0.07	0.06	0.08	-0.11	0.29
Court Variables						
Log(Length)	72,982	5.83	5.73	0.32	5.38	6.69
Δ	72,982	0.06	0.00	0.14	-0.18	0.34
$\Delta \times \mathbb{1}(t > 2013)$	72,982	0.03	0.00	0.11	-0.18	0.34

Table 2
Baseline Results

Table 2 shows regressions testing the effect of trial length on employment. Column 1 through 4 show coefficients of OLS regressions of Log(employees) on Log(Length). Column 1 includes the universe of Italian firms satisfying the filters described in Section 4.1, together with year fixed effects. Column 2 adds border-year dummies and includes only firms headquartered in cities located near the borders of pre-reform court districts. Column 3 also includes firm fixed effects. Column 4, as well as columns 5 and 6, include only firms headquartered in cities located near the borders of the pre-reform court districts affected by the reform. Column 5 shows results from a first-stage regression with Log(Length) as dependent variable and the instrument (the predicted post-reform change in Log(Length) multiplied by the post-reform dummy) as regressor. Column 6 shows results from a second-stage regression where the dependent variable is Log(employees) and the regressor is Log(Length) instrumented. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Sample:					
	Full	Bordering Cities		Bordering Cities Affected by Reform		
	OLS	OLS	OLS	OLS	1st Stage	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Length)	-0.198***	0.056	0.013	-0.128**		-0.324***
	(0.024)	(0.056)	(0.012)	(0.053)		(0.095)
$\Delta \times \mathbb{1}(t > 2013)$					0.683***	
					(0.102)	
Observations	755,028	456,059	455,630	72,982	72,982	72,982
R^2	0.003	0.016	0.924	0.934	0.993	0.934
Year FE	X					
Year-Border FE		X	X	X	X	X
Firm FE			X	X	X	X

Table 3
Robustness Tests

Table 3 has regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy, and on firm and border-year fixed effects. Column 2 and 3 include year-industry and border-year-industry dummies, respectively. Column 4 includes the following controls measured in 2012 and interacted with a post-reform dummy (not shown): leverage, Log(assets), ROA, unemployment rate, and employment rate. Column 5 includes the controls lagged. Column 6 includes a dummy equal to 1 if a firm is located in a city whose court is suppressed. Column 6 excludes firms headquartered in a provincial capital. Column 7 and 8 include firms with at most and more than 15 employees in 2012, respectively. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Baseline	w/Ind.-Year FE	w/Border-Year-Ind. FE	w/Controls × *Post* Dummy	w/Controls	w/Suppr. Court Dummy	Excluding Large Cities	w/# of Employees... ≤ 15	> 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log(Length)	-0.324***	-0.270***	-0.333***	-0.275***	-0.295***	-0.308***	-0.273***	-0.323***	-0.415**
	(0.095)	(0.081)	(0.068)	(0.091)	(0.078)	(0.094)	(0.088)	(0.102)	(0.168)
Leverage					-0.242***				
					(0.031)				
ROA					0.502***				
					(0.056)				
Log(Assets)					0.422***				
					(0.010)				
Unemployment					0.006				
					(0.005)				
Employment					-0.031				
					(0.204)				
Court Suppressed						-0.020			
						(0.017)			
Observations	72,982	72,826	57,443	72,543	71,671	72,982	44,703	37,851	34,673
R^2	0.934	0.937	0.948	0.935	0.942	0.934	0.930	0.837	0.888
F-Stat	45.155	47.285	35.656	43.381	46.602	67.696	62.695	44.112	43.018
Firm FE	X	X	X	X	X	X	X	X	X
Year-Border FE	X	X		X	X	X	X	X	X

Table 4
Firm Entry and Exit

Table 9 shows regressions testing the effect of trial length on firm entry and exit. The sample includes district borders composed by municipalities affected by the reform and that are adjacent to other affected districts. All regressions include district-border and border-year dummies. Column 1 shows results from a first-stage regression with Log(Length) as dependent variable and the instrument (the predicted post-reform change in Log(Length) multiplied by the post-reform dummy) as regressor. Columns 2 and 3 show results from second-stage regressions where the dependent variables are the logarithms of the number of firms created and the logarithm of of firms' closures, respectively, and the regressor is Log(Length) instrumented. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Dependent Variable:		
	Log(Length)	Log(# Entry)	Log(# Exit)
	OLS	IV	IV
	(1)	(2)	(3)
$\Delta \times \mathbb{1}(t > 2013)$	0.809***		
	(0.105)		
Log(Length)		-0.433**	-0.128
		(0.187)	(0.173)
Observations	666	666	666
R^2	0.981	0.991	0.993
F-Stat		59.189	59.189
District-Border FE	X	X	X
Year-Border FE	X	X	X

Table 5
Demand Shocks and Employment

Table 5 has regressions of Log(employees) on Log(Length) (Predicted), a measure of industry sales shock and the interaction between the two. Log(Length) (Predicted) is obtained by projecting Log(Length) on the predicted post-reform change in Log(Length) multiplied by the post-reform dummy, firm and border-year fixed effects. *Shock* is the logarithm of the average sales in the industry to which each firm belongs, excluding the firm itself from the computation. All regressions include firm and border-year fixed effects. Columns 2 and 3 include only observations for which the variable *Shock* is below the industry sample median or above above the industry sample median, respectively. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Shocks Sample:	All (1)	Negative (2)	Positive (3)
Log(Length)	-0.251***	-0.574***	-0.549***
(Predicted)	(0.083)	(0.148)	(0.184)
Log(Length)	0.178***	0.334***	0.008
(Predicted) × Shock	(0.049)	(0.064)	(0.095)
Shock	0.080***	0.102***	0.015*
	(0.006)	(0.015)	(0.008)
Observations	72,982	36,982	35,064
R^2	0.935	0.964	0.963
Firm FE	X	X	X
Year-Border FE	X	X	X

Table 6
Demand Shocks and Debt Level

Table 6 has regressions of Log(total debt) on Log(Length) (Predicted), a measure of industry sales shock and the interaction between the two. Log(Length) (Predicted) is obtained by projecting Log(Length) on the predicted post-reform change in Log(Length) multiplied by the post-reform dummy, firm and border-year fixed effects. *Shock* is the logarithm of the average sales in the industry to which each firm belongs, excluding the firm itself from the computation. All regressions include firm and border-year fixed effects. Columns 2 and 3 include only observations for which the variable *Shock* is below the industry sample median or above above the industry sample median, respectively. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Shocks Sample:	All	Negative	Positive
	(1)	(2)	(3)
Log(Length)	0.011	0.012	-0.330**
(Predicted)	(0.041)	(0.088)	(0.132)
Log(Length)	0.151***	0.315***	0.087*
(Predicted) × Shock	(0.054)	(0.073)	(0.048)
Shock	0.052***	0.055***	0.013**
	(0.005)	(0.009)	(0.005)
Observations	72,363	36,620	34,773
R^2	0.956	0.975	0.973
Firm FE	X	X	X
Year-Border FE	X	X	X

Table 7
Wages and Demand Volatility

Table 7 has regressions of Log(wage) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. All regressions include firm and border-year fixed effects. The dependent variable is total labor expenses divided by number of employees. Column 1 includes the full sample. In columns 2 and 3 firms are sorted according their earnings variability (standard deviation of earnings changes divided by average total assets). In columns 4 and 5 they are sorted by industry sales variability (standard deviation of the average industry sales). Firms belong to the Low or High sample if each measure is in the bottom or top sample tercile. The table also reports differences between the coefficients estimated in the Low and High subsamples. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

		Sorting by:			
		Earnings Variability		Industry Sales Variability	
	Baseline (1)	Low (2)	High (3)	Low (4)	High (5)
Log(Length)	0.081*	0.010	0.136**	-0.024	0.234***
	(0.047)	(0.072)	(0.064)	(0.050)	(0.077)
Observations	72,850	23,653	23,533	25,686	23,147
R^2	0.757	0.785	0.735	0.789	0.738
F-Stat	45.199	40.191	39.369	51.352	39.739
$\beta_{High} - \beta_{Low}$		0.126**		0.258**	
S.E.		(0.073)		(0.077)	
Firm FE	X	X	X	X	X
Year-Border FE	X	X	X	X	X

Table 8

The Effect of Financing Costs, Financial Dependence and Unemployment Costs

Table 8 has regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. All regressions include firm and border-year fixed effects. Firms are sorted according to three variables: leverage (columns 1 and 2), financial dependence (columns 3 and 4), and unemployment rate (columns 5 and 6). Leverage is the ratio of total liabilities to total assets. Financial dependence is the median financing gap for each industry. All sorting variables are measured at the end of the last pre-reform year (2012). Firms belong to the "Low" or "High" sample if each measure is in the bottom or top sample tercile. The table also reports differences between the coefficients estimated in the "Low" and "High" subsamples. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Sorting by:					
	Leverage		Financial Dependence		Unemployment	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Log(Length)	-0.227***	-0.626***	-0.183**	-0.585***	0.033	-0.543***
	(0.057)	(0.205)	(0.072)	(0.188)	(0.118)	(0.143)
Observations	24,137	24,010	24,461	24,002	26,449	34,409
R^2	0.955	0.906	0.946	0.919	0.946	0.925
F-Stat	52.491	40.341	44.353	34.440	65.435	17.286
$\beta_{High} - \beta_{Low}$	-0.399**		-0.402***		-0.576***	
S.E.	(0.196)		(0.144)		(0.185)	
Firm FE	X	X	X	X	X	X
Year-Border FE	X	X	X	X	X	X

Table 9

The Effect of Capital-Labor Elasticity of Substitution

Table 9 has regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. All regressions include firm and border-year fixed effects. Only firms in the manufacturing sector are included. Column 1 shows the baseline regression. Columns 2 and 3 include firms in industries characterized by the bottom and top sample capital-labor elasticity of substitution, respectively. Elasticities are taken from Oberfield and Raval (2014). Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

		Sorting by: Capital-Labor Elasticity of Substitution	
	Baseline	Low	High
	(1)	(2)	(3)
Log(Length)	-0.303***	-0.405**	-0.194
	(0.105)	(0.155)	(0.116)
Observations	26,888	14,332	12,463
R^2	0.949	0.951	0.948
F-Stat	44.348	39.091	51.644
$\beta_{High} - \beta_{Low}$		0.210	
S.E.		(0.167)	
District-Border FE	X	X	X
Year-Border FE	X	X	X

Table 10
Additional Outcomes

Table 10 has regressions with Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy as main regressor. All regressions include firm and border-year fixed effects. The dependent variable is indicated in each column's header. Leverage is total liabilities divided by total assets. Log(debt) is the logarithm of total firm liabilities. Leverage is total liabilities scaled by total assets. ROA is earnings before interest, taxes, depreciation, and amortization scaled by lagged total assets. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Dependent Variable:				
	Log(Sales)	Log(Wage Bill)	Log(Total Debt)	Leverage	ROA
	(1)	(2)	(3)	(4)	(5)
Log(Length)	-0.135*	-0.209***	-0.046	0.004	-0.002
	(0.070)	(0.053)	(0.034)	(0.016)	(0.011)
Observations	72,982	72,850	72,363	72,861	72,249
R^2	0.915	0.945	0.956	0.917	0.704
F-Stat	45.155	45.199	45.532	45.173	45.656
Firm FE	X	X	X	X	X
Year-Border FE	X	X	X	X	X

A Appendix

This Appendix has three parts. Section A.1 explains in more detail the construction of the dataset and lists, in Table A1, all the courts affected by the reform. Section A.2 contains definitions for all the variables of the paper. Section A.3 has results omitted from the main text for brevity.

A.1 Details on the Data Cleaning Process

As explained in the main text (Section 3.3), the pre-reform allocation of municipalities across judicial districts was established in 1941, with minor changes across time, the latest occurring in 1999. By combining several legislative sources (Royal Decree 12/1941, Law 30/1989, Legislative Decree 51/1998, Law Decree 6/1999, and Legislative Decree 491/1999), I match each municipality with one of the 165 pre-reform court districts. The reform studied in the main text suppressed 26 courts and established a new one (not studied in the paper), so that after the law became effective, the country had 165-26+1=140 courts. The distribution of municipalities across the post-reform courts is drawn from Legislative Decree 155/2012.[37] The website of the Chamber of Deputies provides a detailed report describing all the steps that led to the approval of the reform, available at http://leg16.camera.it/561?appro=652 (in Italian). I then obtain, for each municipality, the list of bordering cities from the National Institute for Statistics (ISTAT) website (available at https://www.istat.it/it/archivio/137333).

Sources for the anecdotal evidence briefly presented in Section 3.5 are local newspapers from the Crema and Bassano del Grappa districts; the quotes (in Italian) are, as of October 2019, available at https://www.cremaonline.it/articoli/images/24927-0-ev_bozza+x+Tibunale+fallim..pdf and https://www.bassanonet.it/news/11222-sos_tribunale_cimatti_scrive_a_monti_.html, respectively.

The two lists of municipalities are linked using a fuzzy matching algorithm to account

[37] Further details are at http://leg16.camera.it/561?appro=652 (in Italian).

for different spellings of the names.[38] The accuracy of each match is then verified manually. During the sample period, in a few cases groups of small municipalities merged to give rise to larger administrative units. (Starting from 2014, the fusion of small municipalities has been encouraged through subsidies, and has been quite common since.) I identify such cases through web searches, and treat the affected municipalities as having been merged throughout the full sample period. None of these mergers involved municipalities originally belonging to different court districts.

Amadeus reports the names of the municipality (called *city_nat*) and province (*region_nat*) where each firm is headquartered. To be conservative, I merge the Amadeus data and the districts dataset using not only the municipality, but also the province name. Therefore, if any of the two variables is missing, or if a firm's municipality is assigned to an incorrect province in Amadeus, the firm is excluded from my sample. As before, I use a fuzzy matching algorithm to merge the two datasets.

Table A1 shows the list of the pre- and post-reform courts affected by the reform.

[38] I use the Stata module *reclink* developed by Michael Blasnik.

Table A1
Pre- and Post-Reform Districts

Table A1 lists all the courts affected by the reform. The first column lists the 49 pre-reform courts; the second column lists the 23 courts remaining.

Pre-Reform Courts	**Post-Reform Courts**
Acqui Terme, Alessandria, Tortona	Alessandria
Alba, Asti	Asti
Ariano Irpino, Benevento	Benevento
Avellino, Sant'Angelo dei Lombardi	Avellino
Bassano Del Grappa, Vicenza	Vicenza
Camerino, Macerata	Macerata
Casale Monferrato, Vercelli	Vercelli
Castrovillari, Rossano	Castrovillari
Chiavari, Genoa	Genoa
Crema, Cremona	Cremona
Cuneo, Mondovì , Saluzzo	Cuneo
Enna, Nicosia	Enna
Foggia, Lucera	Foggia
Imperia, Sanremo	Imperia
Lagonegro, Sala Consilina	Lagonegro
Melfi, Potenza	Potenza
Mistretta, Patti	Patti
Modica, Ragusa	Ragusa
Montepulciano, Siena	Siena
Orvieto, Terni	Terni
Pavia, Vigevano, Voghera	Pavia
Pinerolo, Turin	Turin
Tolmezzo, Udine	Udine

A.2 Data Definitions

Table A2
Variables Definitions and Sources

This table has definitions and data sources of the main variables used in the regressions. Amadeus data items are in italic.

Variable	**Definition**	**Source**
Log(Employees)	Logarithm of the number of employees (*empl*)	Amadeus
Log(Wage)	Logarithm of total costs of employees (*staf*) divided by number of employees (*empl*).	Amadeus
Total Debt	Short-term debt (*culi*) plus long-term debt (*ncli*)	Amadeus
Shock	Logarithm of the average sales in the industry to which each firm belongs, excluding the firm itself from the computation	Amadeus
Earnings Variability	Standard deviation of the change in EBITDA (*ebta*) divided by the average total assets (*toas*). Numerator and denominator are computed using observations from the full sample, requiring at least 5 non-missing observations per firm	Amadeus
Industry Sales Variability	Standard deviation of the average industry sales throughout the sample period.	Amadeus
Leverage	Total debt divided by total assets (*toas*)	Amadeus

Continued on next page

Table A2 – *Continued from previous page*

Variable	Definition	Source
Financial Dependence	For each firm in Amadeus between 2008 and 2012, the financing deficit is computed as the change in fixed assets (fixed assets (*fias*) minus lagged fixed assets plus depreciation (*depr*)) minus cash flow (*cf*). The financing deficit and the change in fixed assets are summed over the five years for each firm, and the ratio between these two sums is computed. The financial dependence is the median ratio for each industry (defined at the three-digit SIC level). For consistency with the main analysis, only firms with total assets and sales over €1 million in 2012 are kept.	Amadeus
Return-on-Asset	Earnings before interest, debt and amortization (*ebta*) divided by total assets (*toas*)	Amadeus
Length	Estimated length of a civil court proceeding, defined as $(\text{Pending}_{t-1} + \text{Pending}_t)/(\text{Incoming}_t + \text{Resolved}_t)$	Italian Minister of Justice
Δ	Predicted change in Log(Length) due to the reform.	Italian Minister of Justice

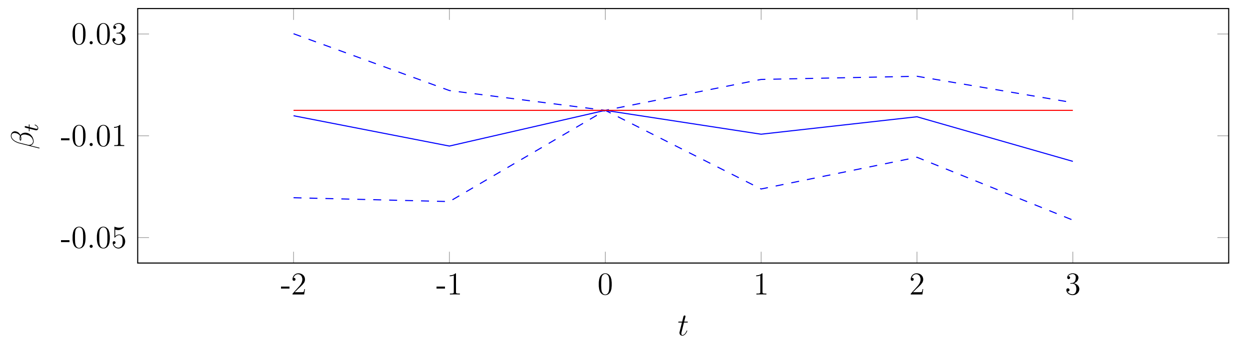
A.3 Additional Results

This section has additional results not reported in the main text. Panels A, B, and C of Figure A1 plots event studies for ROA, leverage and net leverage, respectively. Table A3 estimates the effect of trial length on employment, sales, and total compensation on a sample that includes not only firms headquartered in districts affected by the reform, but also firms located in unaffected districts that share a border with affected ones. (See Section 5.2 in the main text for details.)

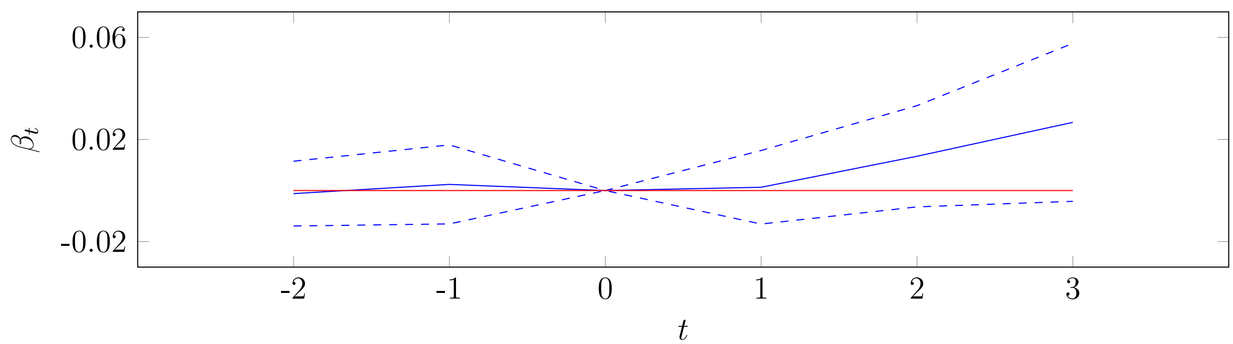
Figure A1
Additional Event Studies

Panels A, B, and C show coefficients estimated from regressing ROA, leverage and net leverage, respectively, on border-year dummies, firm dummies, and year dummies multiplied by Δ, the predicted change in Log(Length). The coefficients β_ts associated with the year dummies $\times$ Δ interactions are plotted together with the 95% confidence intervals. $t = 0$ corresponds to the reform year (2013), and β_0 is normalized to zero. ROA (return-on-assets) is earnings before interest, debt and amortization divided by total assets. Leverage is total liabilities divided by total assets. Net leverage is total liabilities minus cash, all divided by total assets. Standard errors are clustered at the pre-reform district level.

Panel A. Dependent Variable: ROA



Panel B. Dependent Variable: Leverage



Panel C. Dependent Variable: Log(Debt)

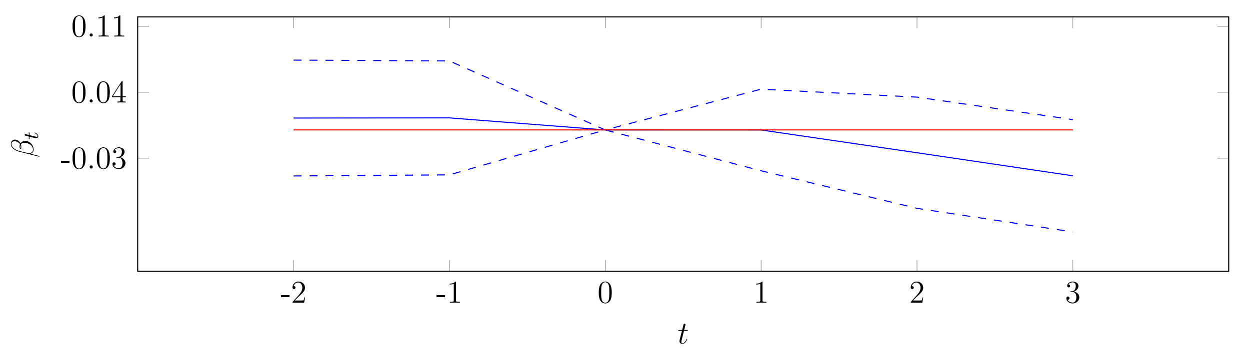


Table A3
Baseline Results with Alternative Sample Choice

Table A3 shows regressions testing the effect of trial length on employment, sales, and total compensation. The sample includes firms headquartered in districts affected by the reform, as well as districts that are not, but that share a border with affected districts. All regressions include firm fixed effects and border-year dummies. In column 1, the logarithm of employment is regressed on the logarithm of trial length. Column 2 shows results from a first-stage regression with Log(Length) as dependent variable and the instrument (the predicted post-reform change in Log(Length) multiplied by the post-reform dummy) as regressor. Columns 3, 4, and 5 show results from second-stage regressions where the dependent variables are the logarithms of employees, revenues and total compensation, respectively, and the regressor is Log(Length) instrumented. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Dependent Variable:				
	Log(Empl.)	Log(Length)	Log(Empl.)	Log(Sales)	Log(Wage Bill)
	OLS	1st Stage	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)
Log(Length)	-0.004		-0.343***	-0.135*	-0.278***
	(0.030)		(0.111)	(0.072)	(0.066)
$\Delta \times \mathbb{1}(t > 2013)$		0.605***			
		(0.128)			
Observations	104,944	104,944	104,944	104,695	102,741
R^2	0.934	0.974	0.933	0.927	0.938
F-Stat			22.217	22.179	21.853
Firm FE	X	X	X	X	X
Year-Border FE	X	X	X	X	X

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No. 181	Holger Kraft, Farina Weiss	Consumption-Portfolio Choice with Preferences for Cash
No. 180	Tobias H. Tröger	Why MREL Won't Help Much