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The Real Effects of Judicial Enforcement

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Non-Technical Summary

Well-functioning legal institutions are a necessary condition for contracts to be enforced, and thus for economic transactions to occur. Therefore, it is not surprising that both scholars and practitioners have examined in depth cross-country and cross-regional variation in the quality of legal institutions and courts' performances as they are potential determinant of differences in economic development.

While most economists will agree that an effective enforcement of contracts will have beneficial effects on real outcomes, providing empirical evidence of such a claim has proved a hard task. The main reason is that researchers face the risk of “omitted variables’’: These unobserved factors may simultaneously affect both economic development and the quality of law enforcement.

In this paper, I exploit a reorganization of the judicial districts geography occurred in Italy in 2013, which suppressed 26 districts. This reform is an appropriate exogenous shock that I use to quantify the real effects of changes in the quality of law enforcement.

I estimate that a 10 percent reduction in the duration of civil trials increases firm-level employment by 2.9 to 3.6 percentage points. Consistent with theory, effects are stronger in firms characterized by low tangibility and high uncertainty, as well as those operating in areas with low social capital. Moreover, my evidence suggests that a primary driver of such results is given by financing constraints. Intuitively, investors are less willing to lend to firms if they are not protected by the judicial system; in turn, lack of funds dampens firm’s growth. As a result, firms that are more depending on external financing appear to respond more to changes in trial duration. These results are also stronger in areas with poor financial development, such as those with few bank branches, suggesting that an effective law enforcement may be a powerful substitute for local banks.

These results demonstrate that law enforcement is a primary driver of economic development. For example, a back-of-the-envelope calculation suggests that differences in the quality of law enforcement are responsible for about a fourth of the gap in the employment rate between the North and the South of Italy. More generally, legislators should recognize that law enforcement per se may be as important as the quality of the legislation inprotecting investors and fostering economic growth.

The Real Effects of Judicial Enforcement*

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Abstract

This paper shows that the quality of judicial enforcement has substantial real effects. I exploit a reorganization of the judicial districts in Italy as an exogenous shock to court productivity and, using an instrumental variable approach, estimate an elasticity of employment to average trial length between -0.36 and -0.29. These results are very different from OLS estimates which do not control for endogeneity. Firms with low asset tangibility, high uncertainty, and headquartered in areas with low social capital appear to respond more to changes in the legal environment. The effects are also more pronounced in highly levered and more financially dependent firms, suggesting that weaker law enforcement tightens financing constraints, and appear to affect mainly firms in less financially developed areas. I obtain similar results for revenues and total compensation. There is, instead, a positive relationship between average wages and trial length, possibly because workers demand compensation for bearing risk due to financing constraints. These results offer a more complete picture of the interplay between legal institutions and real economic outcomes.

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1 Introduction

Well-functioning legal institutions are crucial for contracts to be enforced, and thus for economic transactions to occur (Greif (2005)). While most economists will agree that an effective enforcement of contracts by courts will have beneficial economic effects, and influence firms' behavior as well as financial markets (Pistor (2013)), it is not obvious how to assess the magnitude of its economic impact. When courts are ineffective or too costly to access, informal enforcement can often be a valid substitute in many contexts, by imposing a loss of reputation to the part that breaches a contract (MacLeod (2007)), potentially making formal legal institutions less relevant. Hence, the question of whether stronger legal institutions have real effects needs to be addressed empirically.

Not surprisingly, scholars and practitioners have examined in depth cross-country and cross-regional variation in the quality of legal institutions and courts' performances, as they are potential determinant of differences in economic development (La Porta, de Silanes, Shleifer, and Vishny (1997)). Relating legal institutions to real outcomes across countries provides interesting facts that beget within-country settings to isolate causal channels. However, restricting the focus on a single country has disadvantages, too. For instance, different regions of a single country may have been subject to the same institutional framework for decades or centuries, often not providing sufficient variation in the quality of law enforcement across areas, or lacking exogenous variation that can be exploited for identification.

To address these concerns, this paper will exploit a natural experiment as a source of shocks to the productivity of courts in a large developed economy. Specifically, I exploit a reorganization of the judiciary involving 49 court districts in Italy. In 2013, to promote judges' specializations and cost savings, 26 courts were suppressed and their districts were absorbed by 23 other districts; such "mergers" generate an heterogeneous shock to the quality of judicial enforcement, that I can use to estimate how faster courts affect employment and a host of other real outcomes.

The following example will clarify the natural experiment exploited in the paper. Let the courts A and B be equal in size, and have different productivities, with 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. We could guess that, after this reorganization, the trial length of the "new" court B would be (100+200)/2=150 days. This means that firms originally under court A are now subject to a more inefficient court; while firms originally under court B can expect a trial duration 50 days shorter.[1] Of course, the actual average trial length of the new court B needs not be exactly 150; however, such predicted value can be used as an instrument for the realized post-reform average duration of proceedings.

Still, the courts A and B were originally very different. As long as factors determining such difference vary over time and are also correlated with the outcome of interest, the risk of unobserved omitted variables cannot be dismissed. This problem can be addressed by focusing on adjacent cities located along the court district borders (Ponticelli and Alencar (2016)), allowing me to isolate the effects of the sharp change in trial length caused by the court suppression, to the extent that omitted variables are likely to evolve in a similar fashion across neighboring cities.

I hypothesize that the uncertainty and the costs caused by slow courts will negatively affect the enforcement of contracts and property rights, hence discouraging firms' growth and investment in human capital. Excessively long trials will induce firms to delay investment in human capital until any residual uncertainty regarding the investment opportunity is resolved. In addition, imperfect or costly enforcement of property rights will reduce the expected profitability of investing and hiring workforce. Using the predicted change in post-reform trial length as an instrument for the realized trial length, I am able to estimate the causal effect of quality of law enforcement on employment. I find an elasticity of employment to trial length between -0.36 and -0.29, depending on the specification, which is economically large and precisely estimated. I obtain qualitatively similar results if I use revenues or the

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

total wage bill as dependent variables. In contrast, naïve OLS regressions that do not take into account the endogeneity of the judiciary produce elasticities that are small and positive, even after controlling for local shocks.

The results are robust to a battery of additional tests. Compared to existing research that also employed spatial discontinuity designs, my setting exploits local exogenous variation in trial duration *over time*. Therefore, I can also validate a causal interpretation of the results by performing simple event-studies. Indeed, I show in this way that the rise in employment and the reduction of average trial duration in cities that end up in more efficient courts thanks to the reform do not predate the reform itself, supporting its exogeneity. Moreover, my sample size is large enough to allow for non-parametric controlling not only for local shocks, but also for local industry-level shocks. The baseline tests do not capture a small-firm effect either; if anything, results are slightly stronger for larger firms.

I also conduct a placebo test by simulating a large number of reforms in districts unaffected by the reform. Suppose that the reform I study had no economic significance; rather, an unobserved shock happened to affect employment differentially in less efficient court districts. If this were the case, I should find evidence of higher employment growth in districts that were ex-ante less efficient, even if they were not affected by the reform. However, this is not the case.

I then move to examine the economic channels underlying the results. An extensive literature in Law and Economics provides guidance for a number of tests. Firms may need more effective enforcement to protect the proceedings of an investment in intangible assets; think for example of the potential complexity of intellectual property lawsuits. Moreover, such firms may find it harder to pledge collateral to outside investors (Claessens and Laeven (2003)). Consistent with this mechanism, I find that such firms are the most sensitive to changes in the legal environment. I also examine how uncertainty, as proxied by the volatility in earnings, determines firms' response to changes in the expected length of trials. In line with the idea that firms with more uncertain cash flows are disproportionally affected by transaction costs, as hypothesized by Williamson (1979), these firms appear to be driving the relationship between changes in judicial enforcement and employment growth. Moreover, well-functioning judiciaries are likely to be needed especially in areas with poor social capital, where informal enforcement mechanisms based on reputational losses for the parties that do not comply with contracts may be less effective. Using voters' turnout as proxy for social capital (Guiso, Sapienza, and Zingales (2004b)), I find evidence consistent with this hypothesis.

I also find evidence supporting a "financial constraints" channel, as shown also in other contexts (see for example Brown, Cookson, and Heimer (2017)). Intuitively, non-functioning legal institutions are unable to protect investors from managerial moral hazard or asymmetric information, increasing the cost of financing. To the extent that hiring is, at least in part, funded through external funds, financially constrained firms will reduce employment. Indeed, I observe stronger results for firms operating in financially dependent industries (using an approach similar to Rajan and Zingales (1998)), as well as in highly levered firms (in line with Benmelech, Bergman, and Seru (2011)).

Neither theory nor intuition offers a clear guidance regarding how legal institutions and financial development interact with each other. An improvement in the courts' productivity may boost financing and thus employment only if, ex-ante, there is a sufficient presence of banks or other types of investors to fund firms. Alternatively, investors in financially developed areas may have already developed long-term relationships with firms, making the quality of the judiciary less relevant thanks to the presence of informal commitment devices. In the first case, law enforcement and financial development are complements; in the second, they are substitutes. Presumably, both channels will be at play; however, one may dominate the other. Using different proxies for financial development (credit to GDP ratio, the concentration of bank branches and a proxy for the capital adequacy of the banks operating in each province), I find that the latter hypothesis finds support in the data.

I also analyze other firm-level outcomes. I find a positive relationship between trial length and average wages. A possible explanation, that finds support in the data, is that an improvement in judicial efficiency, and so in access to financing, lowers unemployment risk, allowing firms to pay lower risk premia on wages (Agrawal and Matsa (2013) and Berk, Stanton, and Zechner (2010)). Indeed, the relationship between wages and trial length is present only in firms with high leverage or characterized by high volatility in cash flow, that is, those where workers are more exposed to layoff risk. I do not find, instead, effects on profitability; hence, if a lower trial length foster firm growth, that does not come at the cost of a lower productivity. Moreover, and consistent with previous evidence (Fabbri (2010)), there is no relationship between enforcement and capital structure.

To summarize, this paper provides causal evidence on the interplay between legal institutions and firms' behavior in a field where omitted variables and reverse causality are notoriously hard to rule out, finding large real effects of courts' enforcement. The cross-sectional heterogeneity of the results also supports a number of theories at the intersection of Law and Economics, or Law and Finance.

The paper is organized as follows. Section 2 places the paper's contribution in the related literature. Section 3 describes the institutional setting and the reform. Section 4 describes the data and the identification strategy. Section 5 shows the main results and robustness tests. Section 6 studies the economic mechanism. Section 7 concludes.

2 Related Literature

This paper contributes to at least three strands of research.

There is an ample literature on the effects of legal institutions on firms and other real outcomes. La Porta et al. (1997), La Porta, de Silanes, Shleifer, and Vishny (1998), La Porta, de Silanes, and Shleifer (1999), and La Porta, de Silanes, Shleifer, and Vishny (2000) constitute a series of seminal papers in Law and Finance. They show how the different roots of legal systems affect investors' protection across countries and how such legal provisions in turn are related to a number of measures of financial development. More recent papers that also focus on cross-country evidence are Haselmann, Pistor, and Vig (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 real effects are magnified in courts with an effective law enforcement. (See also Rodano, Serrano-Velarde, and Tarantino (2016), who study a similar reform in Italy.) However, their focus is not on the judiciary *per se*, but rather on how the judiciary may impact the effectiveness of a reform aimed at protecting outside investors. In practice, judicial enforcement may affect the real economy directly, through a number of other channels, which are instead at the core of this paper. 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 line with them, I show that the financing channel appears crucial in determining employment growth, in the less specific context of a large, developed economy, and using a simple and intuitive measure such as average trial length, in principle comparable even across countries (Palumbo, Giupponi, Nunziata, and Sanguinetti (2013)).

These papers, as well as Giacomelli and Menon (2016) and Bonetti (2016), also share with my empirical strategy the spatial discontinuity-design. However, on the methodological side, an important difference with this paper is that the variation they exploit is inherently cross-sectional in nature. Hence, these papers cannot rule out the possibility of sorting; that is, firms may choose on which side of a district border they are headquartered in based on unobserved characteristics hard to control for. My setting, however, exploits time-varying exogenous variation in trial duration. Therefore, I can include in my regression firm dummies that will absorb all the time invariant characteristics that may determine a firm's sorting, and examine how employment changes over time *within firm* by estimating event-study regressions.[2]

A novel and active series of works has the intersection of Labor and Finance at its focus, in particular the effects of shocks to financing on employment. Using data on syndicated loans matched to firms' establishments in the U.S., Chodorow-Reich (2013) estimates the impact of the financial crisis on employment. He 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 significant impacts on a firm's employment growth. Chaney, Sraer, and Thesmar (2013) and Kleiner (2014) study the impact of changes in collateral value due to real estate prices on employment, finding large effects. Unlike these works, the focus of this paper is on the quality of the judiciary; however, in line with them, I show that the financing channel appears important.

Finally, this evidence is also related to works on the real effects of financial development. Financial development is closely linked to legal institutions, either because the latter affects the former (La Porta et al. (1998), Rajan and Zingales (1998)) or because legal institutions are important determinant of the effectiveness of financial development (Chinn and Ito (2006), Pagano and Pica (2012)). I examine a connected, yet different question: how does financial development shape the effects of the effectiveness of the judiciary? In Section 6.3 I find evidence of substitutability between the two.

[2]Other papers that focus explicitly on the effects of average trial duration are Laeven and Woodruff (2007) and Fabbri (2010), who find that court productivity exhibits a positive association with firm size, using data from Mexico and Spain, respectively. Jappelli, Pagano, and Bianco (2005) show theoretically and empirically that slower courts may induce borrowers to behave strategically and reduce credit availability.

3 Institutional Setting

3.1 The Italian Court System and the 2012 Reform

The Italian civil courts system has historically been associated with two characteristics: a very high degree of inefficiency compared to other advanced economies and a substantial heterogeneity across different areas (Bianco, Giacomelli, Giorgiantonio, Palumbo, and Szego (2007), Palumbo et al. (2013)).

Its organization in its basic form goes back to the late 19th century, after the completion of the Italian 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 were (and are) instead brought to the 26 appeals courts.

The first major reform of the organization of the courts system occurred in 2012 and became effective in 2013. In November 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), that put forward a number of measures both to reassure investors and to regain credibility towards foreign partners during the sovereign debt crisis. Among them, the reduction of the total number of courts was considered long overdue.[3]

The reorganization of the courts lead 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.[4]

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). Moreover, the legislator planned to exploit economies of scale due to the increased

[3]For example, the Minister of Justice declared: "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)

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

specialization of the judges: small courts may force each judge to work on cases requiring very different kinds of expertise.

Notably, such reorganization was not made on a case-by-case basis, but was based on ex-ante, essentially mechanical criteria. All courts not located in provincial capitals were to be suppressed, under the constraint of keeping at least three courts for each appeals court.[5] Minor exceptions were made, being due to the Direzione Nazionale Antimafia (a special prosecutor specialized in the repression of mafia-like organizations) advising against suppression, which prevented the elimination of six Southern courts in areas with a strong presence of organized crime.[6] Importantly, actual courts' productivity or trends in productivity were not criteria based on which courts were suppressed.

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 environment, in terms of legal enforcement.

3.2 Predicting Average Trial Length after the Reform

The exact trial duration in a court is typically 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 (see, for example, Berk and DeMarzo (2007)). This measure, henceforth simply called "Length", is defined as:

[5] A province corresponds roughly to a US county and is named after the capital, usually its largest city.
[6] Further details are at `http://leg16.camera.it/561?appro=652` (in Italian).

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

In 2012, the year preceding the enactment of the reform, there were 165 courts. In September 2013, 26 courts were suppressed and their districts were incorporated 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, only 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 I am going to exploit 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 $\overline{\text{Length}}_{j,2012}$, defined as

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

For a firm headquartered in the pre-reform district i, therefore, the predicted change 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 productivity of the new, larger court, needs not be exactly equal to the predicted one. A large court may exploit economies of scale, resulting, for example, from the increased specialization of each judge; alternatively, it is plausible that, at least in the first months after the reform, the reorganization of the offices may have caused some slowdown. Finally, and more obviously, macro-trends due to changing economic conditions or new legislations may impact on courts that were both affected and unaffected by the reform.

However, I expect some characteristics of the suppressed courts to be "sticky" and therefore be preserved in the new, larger districts. For example, features related to the local economic activity that affect the degree of litigiosity of a geographic area, as well as individual judges' ability, will be to a large extent unchanged after the district reorganization.

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 formula 2 above is 353, which is quite close.[7]

Importantly for our purposes, there is anecdotal evidence that individuals were quite aware of the asymmetric effects of the reform. Indeed, in a petition to the Minister of Justice, the majors 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 in terms of efficiency".[8] 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 major 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"[9]

[7]Predicted lengths are generally a bit higher than realized ones, due to a sensible decline in the average trial length occurred in Italy between 2014 and 2016 (Bartolomeo (2017)) which affected all the courts. Contributing factors may have been provisions to favor out-of-court settlements and the computerization of civil proceedings. In all the empirical tests, macro-trends will be accounted for with year fixed effects.

[8]The petition is available at the url `https://www.cremaonline.it/articoli/images/24927-0-ev_bozza+x+Tibunale+fallim..pdf` (in Italian).

[9]The letter is available at the url `https://www.bassanonet.it/news/11222-sos_tribunale_cimatti_scrive_a_monti_.html` (in Italian). The statement was correct up to a characteristic approximation, given that the difference in trial durations between the two courts was just around 50 days, and not 3.5 years.

As explained in the previous section, the reform was designed in such a way that contingent characteristics of the legal 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 all the economic characteristics varying at the local level (Card and Krueger (1994)). 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 Economics literature (Ponticelli and Alencar (2016), Giacomelli and Menon (2016), Bonetti (2016)).

Intuitively, firms headquartered on the opposite side of a pre-reform district border should be unlikely to be affected by dramatically 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 non-parametrically 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.[10]

Before moving to the data, 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 Minister of Justice published data on the *actual* trial length for the year 2016 (see Bartolomeo (2017)), which can be compared to the proxy used in this paper. 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

[10]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 three district borders: 1-2 and 3-4, but also 2-3.

Notice also that, for the purposes identification, also firms headquartered in districts not subject to mergers can 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.)

to to employ a log-transformation of trial length, which will make the scale of the regressor irrelevant.[11]

4 Data and Econometric Strategy

4.1 Data Sources

My source of firm 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 are the bulk of Italian firms. Unlike in the U.S., unlisted firms have fairly strict disclosure requirements, and balance sheets of all firms need to be reported to the Italian Chamber of Commerce. I employ the procedure recommended by Kalemli-Ozcan, Sorensen, Villegas-Sanchez, Volosovych, and Yesiltas (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. 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 consistent over time.

Data on incoming and resolved cases for each Italian court is derived from the Minister 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 courts data are in Appendix A.1.

Additional control variables, at the provincial level, are obtained by combining data from

[11] Figure 2 shows raw numbers for ease of interpretation. The correlation between the logarithms of the two measures is almost identical ($R^2 = 71.54\%$).

the National Institute for Statistics (ISTAT) and the Bank of Italy Statistical Database. The National Institute for Statistics 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 the paper. After adopting the filters described in Section 4.1, I am left with an unbalanced panel of 73,190 company-years and 13,001 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 249. (However, 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 3 shows the geographic distribution of the affected courts. Darker colors correspond to higher trial lengths, and unaffected courts are left blank. Affected courts are fairly well distributed across the country, although tend to be more concentrated in the North-West 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 my sample are headquartered in the North-West. Northern courts are generally more efficient, 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 leverage (short-term liabilities plus long-term liabilities, all divided by total assets), net leverage (same as leverage, but with cash subtracted from the numerator), wages (measured as total labor costs divided by the number of employees), return-on-assets (ROA, defined as earnings before interest, debt and amortization divided by total assets), tangibility (fixed assets divided by total assets), earnings variability (standard deviation of earnings changes divided by average total assets). Standard controls for economic development are value added per capita, employment and unemployment rate, and the number of bank branches per 100,000 inhabitants. These measures are at the provincial level. Detailed definitions and sources of each variable is in Table A2.

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:

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

$\text{Length}_{k,i,b,t}$ and $\Delta_{i,j}$ are defined in equations 1 and 3, respectively, $\mathbb{1}$ is the indicator function, and $\gamma_{b,t}$ and η_k are border-year and firm fixed effects, respectively. $\epsilon_{k,i,b,t}$ is an error term. Based on the discussion of Section 3.2, I expect the estimated sample coefficient $\hat{\alpha}$ to be positive.

Estimating equation 4 produces a predicted value $\text{Log}(\widehat{\text{Length}}_{k,i,b,t})$ that can be used to obtain the causal effect of trial length on employment with this second-stage regression:

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

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 firms' employment to the trial length, which is expected to be negative.

In order to corroborate the validity of the natural experiment, it is, however, also important 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 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.

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 court level, following the recommendation of Bertrand, Duflo, and Mullainathan (2004).

5 Duration of Trials and Employment: Baseline Results

5.1 Main Results

Table 2 shows the baseline results on the elasticity of employment to trial length. (Other outcomes, such as revenues and total compensation, will be analyzed in Section 6.4.) 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 almost 754,000 observations will not be the focus of the other tests, column 1's results provide a useful benchmark. I regress Log(employees) on Log(Length), firm, and year dummies. The elasticity of employment to trial length measured in this large sample is economically small, *positive* (0.032), and significant at the 5% level.

In column 2, I adopt the spatial discontinuity-design described in Section 3.2. Thus, I include border-year dummies in the regression[12] and restrict the sample only to firms that are headquartered in municipalities near a court border, with the sample dropping by about 40%. Again, the estimated elasticity is positive, although smaller (0.011) and not precisely estimated.

This weak evidence of a positive association between employment and average trial duration from naïve OLS regressions may arise, for example, because of reverse causality. Growing local economies may burden the courts to enforce contracts or settle disputes, making them less efficient. Given this and other possible concerns, an instrumental variable approach is in order.

The regressions in columns 3 through 5 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 3, 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 (-0.14) and is significant at the 5% level.

In column 4, I implement the strategy described in Section 4.3 and estimate the first-stage regression 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 0.685% rise in actual length of proceedings, on average. The t-statistic is 6.78, suggesting that the instrument is definitely strong.

Column 5 shows the main result of the paper. I estimate the second-stage equation 5 to obtain a consistent estimate of the elasticity of employment to trial length. I estimate a highly significant value of -0.36, with a t-statistic equal to -3.63. 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

[12]The high-dimensional fixed-effects model in this paper are estimated using the Stata package *reghdfe* written by Sergio Correia.

quite slow-moving; hence, without a proper natural experiment, the degree of variation that can be exploited is limited. Thus, the evidence of Table 2 confirms the importance of finding an adequate instrument to tackle the endogeneity problem. The estimate is also substantial from an economic point of view, as I will show in Section 5.4, which interprets the magnitude of this result.

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, and do not predate it. I estimate equation 6 and plot the coefficients, together with 95% confidence intervals, in Figure 4. 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 4 is reassuring in the sense that districts that became more efficient after the reform do not appear to have been chosen based on their previous over or under-performance.

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 5 and mirror those of Figure 4, with 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 $\tau = -2, 1, 0$ and fall to -0.18, -0.26, and -0.27 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. 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 3-digits SIC code classification, the most refined available in Amadeus. The point estimate slightly decreases in magnitude, from -0.36 to -0.30, but remains precisely estimated.

In column 3, I include border-year-industry dummies, therefore controlling *locally* for industry shocks. This is a very demanding test and includes in the equation over 10,000 fixed effects, plus the over 13,000 firm dummies. However, relative to column 2, the coefficient associated with the average trial length is unaffected and, if anything, estimated more precisely.

The test in column 4 controls for variables that may correlate with the post-reform change in employment. I include, as firm variables, a proxy for size (Log(total assets)), financial risk (leverage), and profitability (return-on-assets). I also add a series of economic indicators (at the province level) for financial development (number of branches $\times$ 100,000 inhabitants), economic development (value-added per capita) and labor outcomes (employment and unemployment rates). I measure these variables at the beginning of the reform year in order to avoid the "bad controls" problem (Angrist and Pischke (2009)) and interact them with a post-reform dummy. Again, the coefficient of interest remains large in magnitude and significant. (Controls are not shown for brevity.)

The reform caused the permanent closure of 26 courts, meaning that workers and judges from the suppressed courts had to move to the facilities of the remaining 23 courts, that were often many kilometers apart. Anecdotal evidence suggests that local politicians protested intensely against the closures of the courts facilities (see Section 3.2). Thanks to the spatial discontinuity-design, the firms in the sample are at approximately at 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 court closes down. Column 5 shows that the complaints of local politicians may have not been totally unwarranted, given that the coefficient is negative and not negligible, being equal to -0.027. However, it is estimated quite imprecisely (t-statistic=-1.58). More importantly, the coefficient on the Log(Length) variable, instrumented as usual, is essentially unaffected (-0.34).

The border-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 includes also very large cities such as Genoa and Turin, with well over 500,000 inhabitants; firms headquartered therein, which could be large, listed companies that may differ (and, in principle, 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. Although the sample size drops by almost half, the point estimate of the coefficient on Log(Length) remains significant, although slightly smaller than the one estimated on the full sample (-0.29).

Finally, it is important to explore 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.

There is, however, a reason related to the institutional context for suspecting that the effect of a lower trial length will be more pronounced for large firms. In Italy, firms with over 15 employees are subject to higher firing costs, although two labor market reforms that occurred in 2012 and 2014 have substantially reduced such rigidities.[13] Column 7 includes firms with at most 15 employees, and column 8 includes firms with more than 15 employees. (Firm employment is, as before, measured at the beginning of the reform year.) The coefficients associated with trial length are both negative and significant, but

[13] Since 1970, long-term workers employed in establishments with more than 15 employees had to be reinstated if fired without justified motive. The "Fornero" reform (2012) limited the possibility of reinstatement in the case of unjustified dismissals. More importantly, the 2014 "Jobs Act" reform, limited the reinstatement provision to discriminatory dismissals and introduced severance payments of an amount increasing in the worker's tenure ("graded security"). Such payments are higher for firms with more than 15 employees. For a description of the reforms and an evaluation of their effects, see Sestito and Viviano (2018) and Boeri and Garibaldi (2018). Section 5.3 addresses the concern that the impact of the reorganization of the judicial districts may be confounded by the 2014 labor market reform.

the estimates suggest, if anything, a larger effect in larger firms (-0.335 versus -0.472). This result should not be overemphasized, as the difference between the coefficients is neither economically large nor significant. However, it is reassuring that the effect of average trial length on employment is not a small firm effect, and hence likely to be relevant from a macroeconomic perspective.

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 not subject to mergers, as long as they share a border with districts affected by the reform. 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.[14] 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.[15] 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 6.4, are used as dependent variables.

The analysis so far has exploited within firm variation. However, it is important to test of potential effects also on the extensive margin, because the positive impact of judicial enforcement on firm size could, in principle, be compensated by an increase in the number of firms exiting the market or by a drop in entry. I obtain data on firms' entry and exit for each year and municipality from Infocamere, the database of the Chamber of Commerce.[16] In Appendix-Table A4 I estimate the elasticity of entry and exit to trial length. There is some evidence of "creative disruption", with both coefficients being negative, similar in size and significant at the 5% or 10% level. The net effect is very close to zero and insignificant, suggesting that better courts do not appear to affect the stock of firms operating in the

[14] 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.

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

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

market, but rather their average size, as in Laeven and Woodruff (2007) and Giacomelli and Menon (2016).[17]

5.3 A Placebo Test

While there is substantial heterogeneity in the pre-reform average trial length, an unfortunate feature of my setting is that the reform affected all the courts at the same time. Suppose that a nationwide shock hit the economy at the same time that the court reform became effective 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, my instrument would still pick up post-reform outcomes because, by construction, Δ tends to be larger in ex-ante more inefficient courts.

Fortunately, a simple "placebo" test is available, and exploits the fact that most courts were unaffected by the reform. I proceed as follows. From the set of 165 pre-reform courts, I first exclude the 49 affected by the reform. Then, I randomly select 26 courts and simulate a merge 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 regression 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 6, 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 -0.246); more precisely, the fraction of coefficients lower than the true coefficient is only 0.06%. 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 for the courts affected by the reform, suggesting that the results I find are unlikely to be due

[17] For related evidence, see also Chemin (2009), that looks at the effects of a reform that provided Pakistani judges with more training, and explores the effects of the resulting improvement in judicial efficiency on entrepreneurship, finding large positive effects.

to any nationwide-level shock that occurred at the same time of the reform.

5.4 Economic Magnitudes

Having established that shortening average trial duration has significant real effects, it can be informative to do a back-of-the-envelope calculation to get a sense of how much it can explain of the differences in real outcomes at a more macro-level. As shown in Figure 3, Southern Italy has a much more inefficient legal system than the North, with Central Italy lying in the middle. The pattern of economic development follows a similar North-South divide.[18]

Suppose that all the Southern courts had a duration of civil proceedings equal to the median Northern court. Using the baseline estimate of -0.359 found in Table 5.1, the Southern regions would have about 814,000 additional workers employed, corresponding to a 5.97% increase in the employment rate. Given that, as of 2016, the difference in employment rate between the North and the South is 22.5% (65.9% versus 43.4%), improving the quality of the Southern legal system to levels comparable to the North would eliminate about a quarter of the gap in employment outcomes. The estimate smallest in absolute value from Table 5.1, -0.29, would still produce a 4.82% increase in the employment rate.

Of course, there are a number of reasons why these figures may be too high. For example, in general equilibrium, higher labor demand will push wages, dampening labor growth.[19] There are, however, also reasons to believe that these estimates may have a downward bias. For example, recall from Table 3 that I find slightly stronger effects of judicial productivity on employment in larger firms. Moreover, in Section 6.3, we will see that the effects appear to be much stronger in less financially developed regions, such as those of the South (Guiso, Sapienza, and Zingales (2004a)). Overall, while these numbers should be taken with a grain of salt, it is reassuring that they are neither economically marginal nor implausibly large.

[18] As it is customary, I include the islands of Sardinia and Sicily among the Southern regions.

[19] Moreover, employment statistics include also the public sector, but the results of the empirical analysis refer to the private sector only.

6 The Economic Mechanism

6.1 The effect of Tangibility, Uncertainty and Social Capital

The comprehensive information available for both firms and geographic areas allows testing for different economic channels that may be at play. In Table 4, I show how tangibility, uncertainty and social capital determine the extent to which judicial enforcement affects the real side of the economy.

Claessens and Laeven (2003) show in a cross-section of countries that enforcement of property rights is positively correlated with growth, especially in sectors that make large use of intangible assets. Intuitively, securing the returns from intangible assets is harder if property rights are not adequately enforced; hence, those sectors are those that should benefit the most from better property rights. (Similar conclusions are reached in Braun (2005).) Evidence presented by Berkowitz, Lin, and Ma (2015) suggests, however, an alternative channel that may counteract this effect. They study a property law enactment that enhanced creditors' power in China in 2007, and find that firms with more tangible assets benefitted disproportionally more, implying that firms' collateral was perceived as more valuable. Hence, which of the two channels will prevail in the context of this paper is an empirical matter. In columns 1 and 2 of Table 4, I sort firms according to their degree of asset tangibility, measured at the beginning of 2013. Following standard practice (Farre-Mensa and Ljungqvist (2016), Chaney, Sraer, and Thesmar (2012)), here and in what follows, I split firms in terciles for each of the sorting variables, and compare the coefficients on Log(Length) across the top (*High*) and bottom (*Low*) terciles subgroups. I also show the differences between the coefficients, as well as their associated standard errors. The ceofficient is almost twice as large in magnitude in the low relative to the high tangibility tercile (-0.52 versus -0.27), and the difference is statistically significant at the 5% level; thus, the first channel appears to dominate.

In his classic treatment on the Economics of Transaction Costs, Williamson (1979) argues that, among the various features that characterize transactions, "uncertainty is widely conceded to be a critical attribute". Intuitively, in presence of high uncertainty, transaction costs may be especially large, possibly to induce vertical integration between customers and suppliers. Based on this argument, I hypothesize that the firms more sensible to judicial enforcement are those characterized by high uncertainty in their profitability. I use the popular measure of earnings volatiliy introduced by Brealey, Hodges, and Capron (1976), given by the standard deviation of earnings changes scaled by the average of total assets. I use the full sample of observations between 2000 and 2016[20], and following Matsa (2010) require at least five non-missing observations for both earnings changes and total assets to compute the measure. I use EBITDA as measure of earnings, but in unreported tests I obtain similar results when using EBIT, revenues or operating revenues instead. Columns 3 and 4 show results consistent with the hypothesis; the coefficient on judicial enforcement is small and insignificant in the subsample of firms with low uncertainty (-0.21), but rises in magnitude up to -0.73 when we move to firms with high uncertainty. (The difference between the two coefficients is significant at the 1% level.)

MacLeod (2007) shows theoretically that penalties involving reputational losses and the inability of accessing the markets for the party that defaults on a contract may be effective in enforcing contracts when access to courts is too costly. However, such punishments may be concretely available only in communities with high social capital, where, as Guiso et al. (2004b) put it, "people may trust each other more because the networks in their community provide better opportunities to punish deviants". Indeed, they show that social capital appears to foster financial development primarily in areas with weak legal enforcement. In columns 5 and 6 I sort firms according to the degree of social capital of the area they are headquartered in, proxied by voters' turnout for the Chamber of Deputees in the 2013 general election.[21] The evidence is consistent with a substitutability between judicial enforcement

[20]Unfortunately, Amadeus typically generally only the most recent 8 years of data for each firm, so that in practice the volatility measure is computed over the period 2010-2016 for the vast majority of firms.

[21]This is one of the two proxies used by Guiso et al. (2004b). Data on blood donation at the provincial level, the other measure they employ, are not publicly available for the period studied in this paper.

and social capital. The coefficient on Log(Length) is essentially zero in high social capital areas, but drops to -0.54, significant at the 1% level, in firms headquartered in provinces with low turnout. The difference between the two coefficient is significant at the 10% level.

6.2 Leverage and Financial Dependence

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. Imperfect law 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)). A more effective judiciary should alleviate these problems; more so if firms have a high debt burden. Therefore, I expect the effect on employment of a change in average trial length to be stronger for more levered firms.

To test the financing channel more directly, I also differentiate firms according to their needs for external financing. 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.[22] I compute this dependence for each firm across the years 2008-2012 and then take the industry median to construct an industry proxy for financial dependence.

Results are in line with the hypotheses. Columns 1 and 2 show that the coefficient more than doubles when moving from firms with low to high leverage. The difference between the two coefficients, -0.469, is significant at the 5% level. The difference between the coefficients is similar and equal to -0.427 (significant at the 1% level) when we look at columns 3 and 4, where the sorting variable is financial dependence

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

6.3 Financial Development

Legal institutions and finance are closely connected, so it is natural to examine how they interact with each other. More specifically, a question that is often asked in the empirical literature is: are financial development and the quality of legal institutions complements or substitutes?

For example, Ponticelli and Alencar (2016), Rodano et al. (2016), and Bonetti (2016) show that reforms allowing renegotiation in bankruptcy have stronger effects when law enforcement is higher. Chinn and Ito (2006) show that the effects of financial openness on the equity market become relevant only when reaching a sufficient degree of legal development.

While this evidence supports a complementarity view, other studies suggest otherwise. For example, Pagano and Pica (2012), find that the financial development does spur growth in more financially dependent industries, but only in OECD countries, that have arguably stronger law enforcement.

This work has examined how the real effects of financial development (very broadly defined) vary depending on the quality of law enforcement. My setting allows me to address a closely-linked, yet specular question: how does an improvement of the legal institutions affect real outcomes depending on the ex-ante degree of financial development?

Table 6 shows how the real effects of law enforcement vary with some proxies for financial development, measured at the provincial level. The first one, number of branches $\times$ 100,000 inhabitants, proxies for the availability of bank financing (Bonetti (2016)). The vast majority of the firms of my sample are unlisted and not too large, so bank loans are likely to be their major source of financing. The second proxy has a conceptually similar interpretation, and is defined as the ratio of total bank loans to GDP.[23] This indicator is commonly employed as a country-level measure (Rajan and Zingales (1998)), but has been used also in work on local financial development (Jappelli et al. (2005)).

[23] More precisely, the Bank of Italy statistical database reports only the total of medium- and long-term loans, not the total loans amount.

Finally, the third proxy is aimed at capturing lenders' financial solidity, which may be especially relevant given that the sample period studied here overlaps with the sovereign debt crisis. Firms may struggle to obtain capital from under-capitalized banks, that is, banks with low Tier 1 ratios (Acharya and Steffen (2015)). Unfortunately, I do not observe actual bank-firm relationships; however, I can proxy for the capitalization of the local bank network by constructing a weighted average of the Tier 1 ratio of the banks operating in each province. I obtain the address of each bank's local branch from the Bank of Italy's Bank Register as of December 31$^{\text{st}}$ 2012, and then, for each province, construct the weighted average as:

$$\text{Average Tier 1 Ratio} \equiv \frac{\sum_i N_{i,j} \times \text{Tier } 1_i}{\sum_i N_{i,j}} \tag{7}$$

where $N_{i,j}$ indicates the number of branches belonging to bank i operating in province j. Tier 1 ratios are from Osiris.[24]

As previously done, I divide firms in terciles according to each of the sorting variables. Results are stark: in the less financially developed areas, law enforcement has no effect on employment. The baseline results appear to be driven by firms operating in regions with a lower bank presence, low credit supply and high banks' fragility, with coefficients on Log(Length) of -0.707, -0.599 and -1.018. In the provinces with higher financial development, instead, the coefficients are all very close to zero; all the differences in the coefficients across subsamples are significant at the 5% or 10% level.[25]

In sum, Table 6 supports a substitution hypothesis, although it is not easy to refine the economic channel further. Investors other than banks may be more willing to provide funds

[24] The Bank of Italy's Bank Register provides also the banks' group composition at any given point in time, so I aggregate branches to their respective groups. Osiris data and the Bank of Italy branches data are merged by name with a fuzzy matching algorithm using the Stata routine *reclink* written by Michael Blasnik. The matches are then verified manually. I am able to match 473 banks in Osiris with non-missing Tier 1 ratios out of 645 banks in the Banks' Register. The banks not matched tend to be small, so that the match rate for branches is fairly high (31,600 out of 33,170, or 95.3%). The average Tier 1 ratio for each province is computed only over the matched banks.

[25] Notice however that the results that use branches per 100,000 inhabitants as sorting variable should be interpreted with caution due to the low F-statistic in the "Low" subsample.

when law enforcement is more effective. Alternatively, a good judiciary may induce non-local banks to supply credit to firms despite not having a long-term borrower-lender relationship.

6.4 Wages and Other Outcomes

This section analyzes other outcomes of interest beyond employment. If courts' productivity spurs employment, overall real activity, and thus revenues, should follow the same pattern. In Table 7 I estimate equation 5 with Log(sales) as the dependent variable. As expected, there is a negative relationship between revenues and trial length. The coefficient is -0.144, significant at the 10% level. The elasticity of sales to trial length is equal ton about 40% the elasticity of employment; however, visual inspection of the reduced-form event study regression is particularly useful here. Figure 7 not only shows the absence of a pre-trend before the enactment of the reform. It also shows that sales do respond quite strongly to the induced change in trial length caused by to 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 adjust also their production.

In column 2 the dependent variable is the logarithm of the total wage bill at the firm level (Log(Pay)). The estimated coefficient is now highly significant and suggests an elasticity of total wage payment to trial length of about -0.21. The event study plotted in Figure 8 provides clean evidence that the change in the wage bill is due to the reform and does not anticipate it. Section A.3 in the Appendix replicates the tests on cross-sectional heterogeneity of Tables 4 5, and 6, with Log(Pay) or Log(Sales) as dependent variables, and give almost always results that are qualitatively similar to those that study the effects on employment reported in the main text.

If employment is positively affected by an improvement in courts, this expansion in size may come at the cost of lower firm productivity. In column 3 of Table 7 the dependent variable is return-on-assets (ROA); there is no evidence that this is the case. Table 7 also examines the effects of average trial duration on capital structure. Giannetti (2003) and

Fan, Titman, and Twite (2012), who look at a cross-section of firms from several developed countries, find that investors' protection at the country level is positively associated with firms' leverage ratios. When restricting the analysis to a single country, however, 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 associated. Columns 4 and 5 show coefficients of regressions where the dependent variables are leverage and net leverage, respectively. Both coefficients are positive, but not significant and fairly small. Thus, the evidence points out to a lack of a systematic relationship between quality of law enforcement and capital structure.[26] Panels A, B, and C of Figure A1 in Appendix A.3 plot coefficients for the event-studies corresponding to the last three tests, confirming that there is no evidence of any effect of the reform on profitability and capital structure.

Finally, I analyze the effect of judicial enforcement on average wage. Remember that the coefficient of column 2, where the dependent variable is Log(Pay) is large but smaller in absolute value than the baseline coefficient obtained when the dependent variable is the logarithm of employment, equal to -0.36. Hence, it is not surprising that, in column 6, Log(wage) displays a positive relationship with trial length (coefficient=0.10, t-statistic=2.11; the event study is plotted in Figure 9). A potential explanation is that wages may fall if managers, thanks to their improved access to financing, are able to reduce the risk of firing, and therefore risk premia included in wages, as predicted by implicit contract models (Baily (1974), Azariadis (1975)); other models also rely on the idea that wages and firm's risk should be inversely related to make predictions on the optimal capital structure (Titman (1984) and Berk et al. (2010)).

[26] Vig (2013) studies a securitization reform in India and shows that the strengthening of creditor rights reduces the use of debt by firms, particularly in firms with high asset tangibility, suggesting that strong creditors' rights may induce inefficient liquidation. In unreported tests, I interact the instrument with asset tangibility (measured as usual at the beginning of the year of the reform), and use this variable as a second instrument for Log(Length) and Log(Length) $\times$ tangibility. I find that the coefficient on the interaction term is positive and significant at the 10% level, both for leverage and net leverage, suggesting that the same mechanism could play a role here.

Evidence consistent with this explanation is presented in Panel B of Table 7. The "financing channel" story predicts that revenues should rise especially in riskier firms, that are arguably more vulnerable to a high cost of external funding. Among the several sorting variables used so far, two are particularly appealing as potential proxies for firm risk: leverage (Berk et al. (2010)) and earnings volatility (Brealey et al. (1976)). These firms are precisely those where access to financing should reduce unemployment risk, allowing managers to pay lower wages in equilibrium. Columns 1 and 2 show that the elasticity of wage to trial length doubles if we move from low to high leverage firms; moreover, it is very close to zero in firms with low financial dependence, and about 0.12 in firms with high dependence, although this coefficient is significant only at the 10% level. Even if the differences are not statistically significant, the results are in line with another potential channel through which judicial enforcement can affect employment, namely the effect on risk premia paid on wages.[27]

7 Conclusion

This paper has presented evidence of the real consequences of having more or less effective courts' enforcement, by showing that trial duration has a first order impact on firm employment, revenues and wages. Characteristics of the institutional setting of the reform, as well as a number of robustness tests, provide reassuring evidence on the causal nature of such results.

The economic impact of courts is magnified in presence of low asset tangibility, high uncertainty and low social capital. Moreover, such effects are more pronounced in highly levered and more financially dependent firms, and lowering trial length appears to be particularly

[27] Another possible explanation, that unfortunately I cannot test with my data, is that the the positive relation between trial length and wages is due to a "composition effect". In Italy, and in many European countries with relatively rigid labor markets, workers with temporary contracts tend to be a large fraction of new hires during an expansion of employment (Boeri and Garibaldi (2007)), and recent evidence shows that such workers tend to earn lower wages (Daruich, Di Addario, and Saggio (2018)). Hence, following a relaxation or a tightening of financing constraints, such as those due to the imperfect enforcement of contracts by courts, employment and average wages may move toward opposite directions. This explanation would need individual workers' data to be tested; however, does not contradict the "risk premium" story sketched in the main text.

effective in areas with poor financial development. The stark differences between results from simple OLS regressions and tests that explicitly control for endogeneity are stark, showing the importance of a carefully designed identification strategy when studying the effects of judicial productivity, a variable likely to be correlated with a number of institutional and economic features across regions.

This work leaves at least three questions unanswered. First, the focus of this paper has been on financing: poor law enforcement causes financial constraints, which in turn dampen labor demand. Alternatively more effective courts may reduce expenses related to labor disputes, effectively reducing labor costs and spurring employment. The paper is silent on this channel, which could be investigated in future work, using individual workers' data.

The evidence of substitutability between court productivity and financial development is quite intriguing and may foster additional work. For example, healthy banks may be more willing to lend to firms located in areas where they do not have long-standing relationships, as long as they are able to enforce contracts at low costs, making soft information less crucial. A simple implication, which could be tested with matched bank loan-firm data, is that firms should be able to increase the pool of potential lenders, as well as obtaining better financing terms.

Finally, I have argued that the long-run response of profitability, wages, and revenues to the change in trial length induced by the reform may differ from the short-run response studied here. Additional research, focusing on either this or other settings, would help to provide a more complete picture of the relationship between judicial productivity and firms' outcomes.

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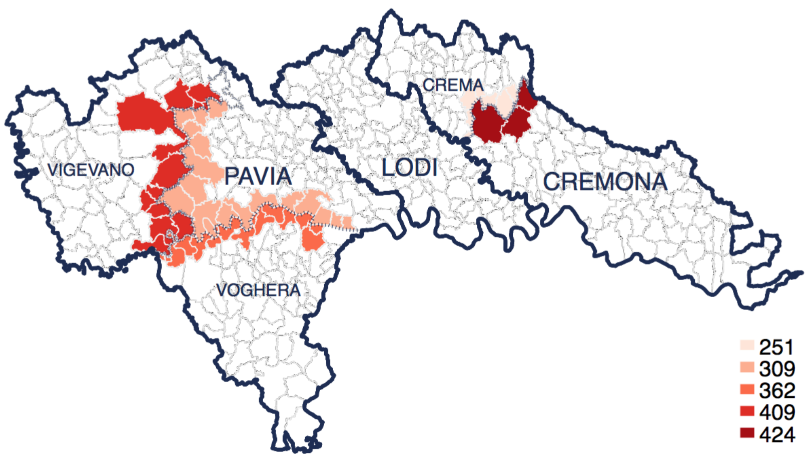
8 Figures and Tables

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

Panels A and B show choropleth maps of trial length and predicted change in trial length in six pre-reform districts from the Lombardy region. Darker versions of red correspond to higher trial lengths. 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 has the trial length of each pre-reform district. Panel B shows the predicted post-reform change in trial length.

Panel A. Trial Length



Panel B. Predicted Change in Trial Length

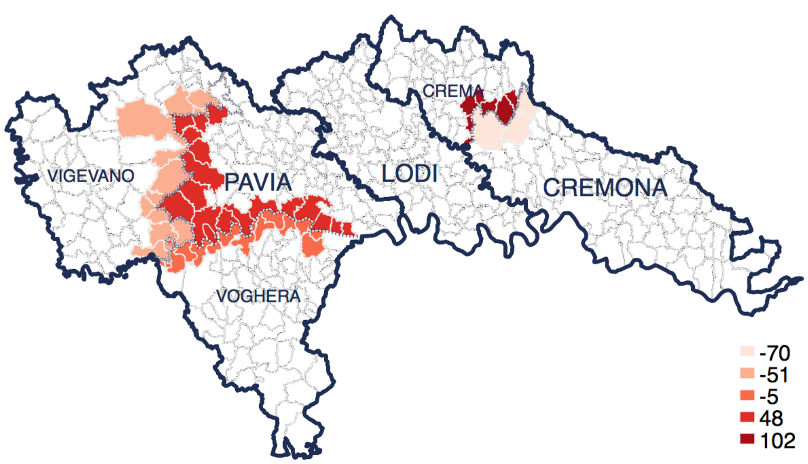


Figure 2
Actual Versus Estimated Trial Length

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 standard errors. The red line plots the estimated regression function.

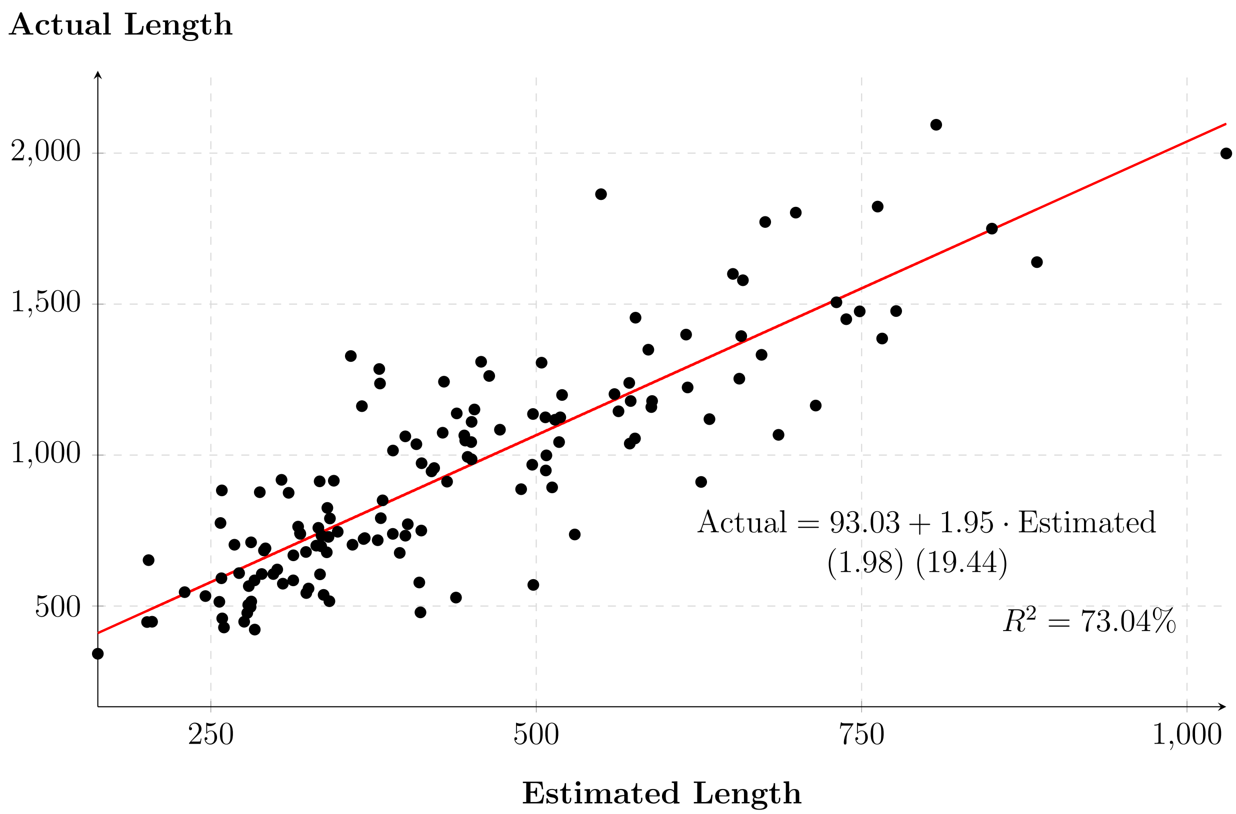


Figure 3
Trial Length across Italian Districts

Figure 3 shows a choropleth map of trial length across Italian districts as of 2012. Darker colors correspond to higher trial lengths.

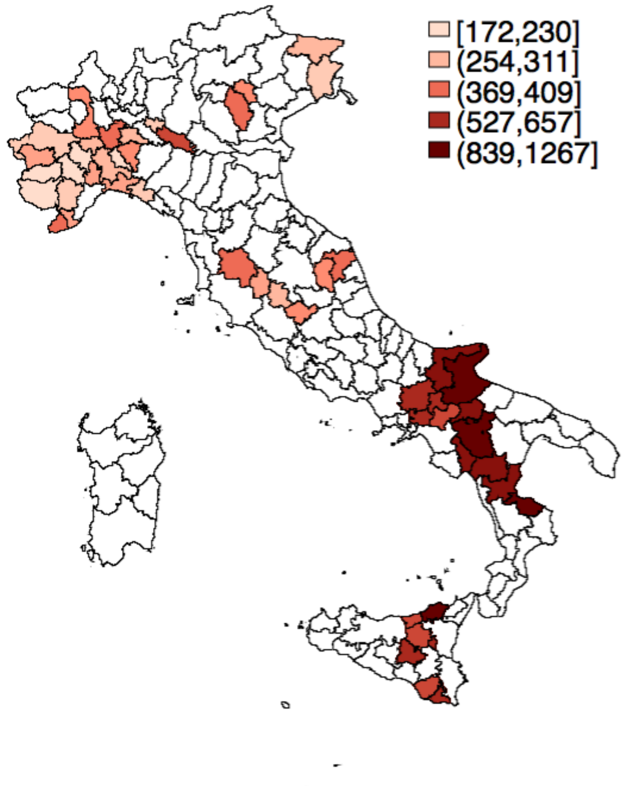


Figure 4
Event Study with Log(Length) as Dependent Variable

Figure 4 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 to 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.

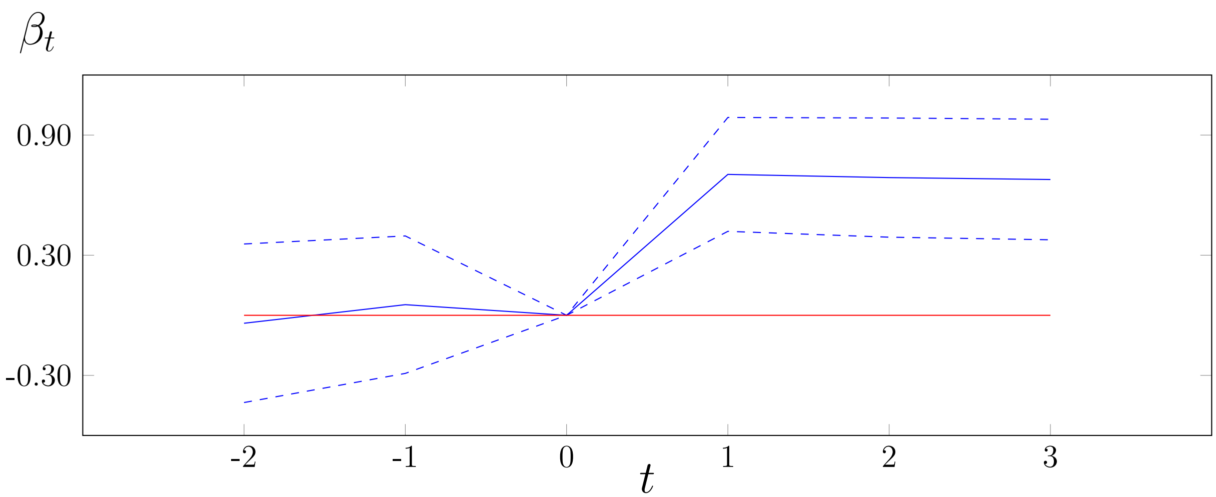


Figure 5
Event Study with Log(Employees) as Dependent Variable

Figure 5 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 to 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.

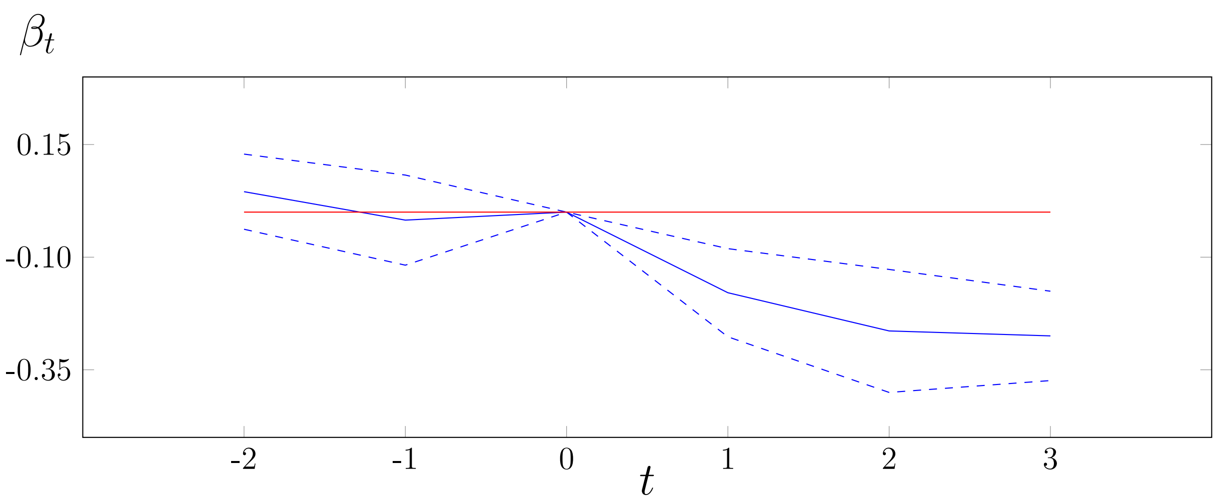


Figure 6
Placebo Test

Figure 6 plots the cumulative 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.

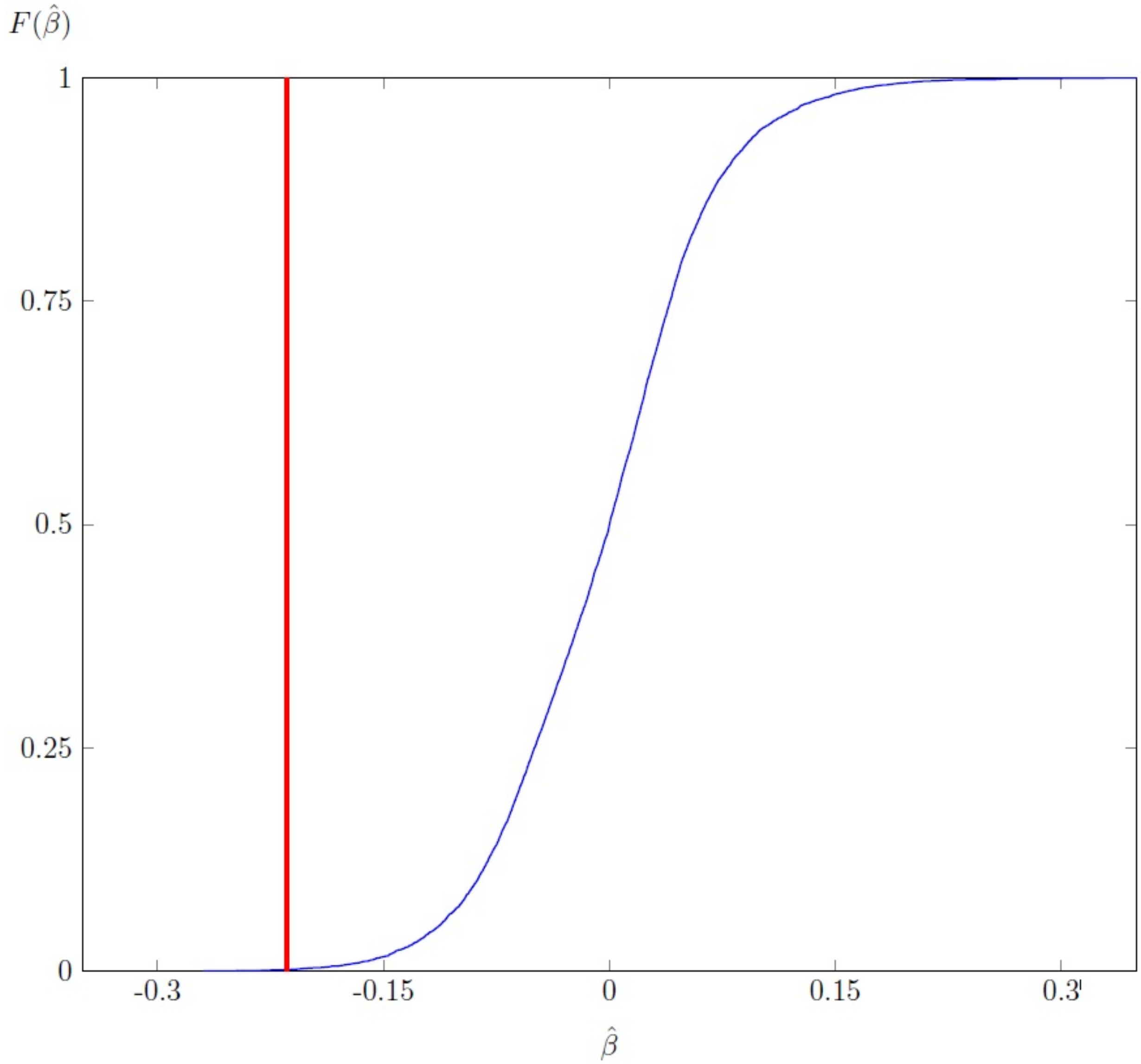


Figure 7
Event Study with Log(Sales) as Dependent Variable

Figure 7 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 to 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.

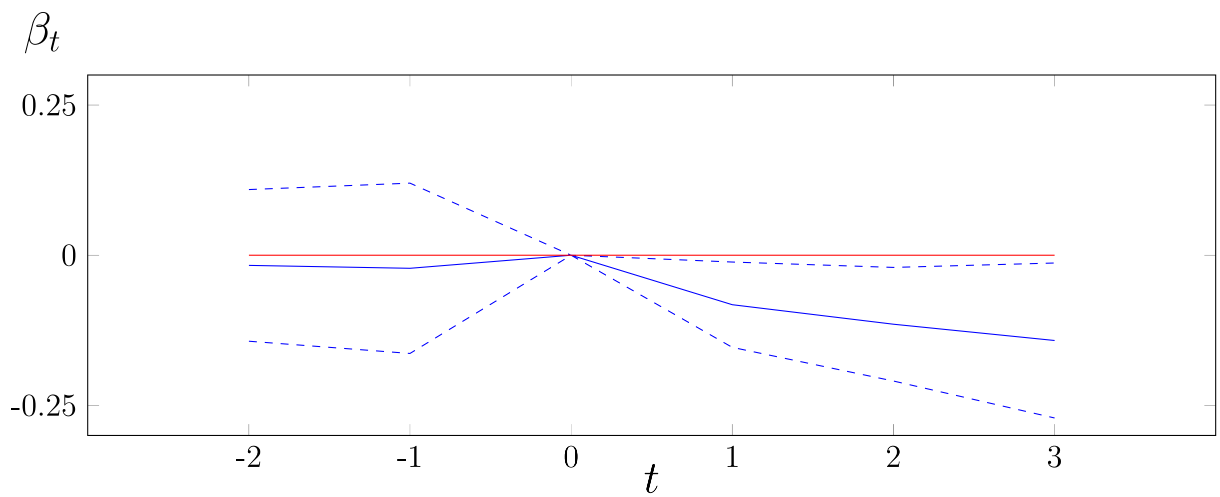


Figure 8
Event Study with Log(Total Pay) as Dependent Variable

Figure 8 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 to 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.

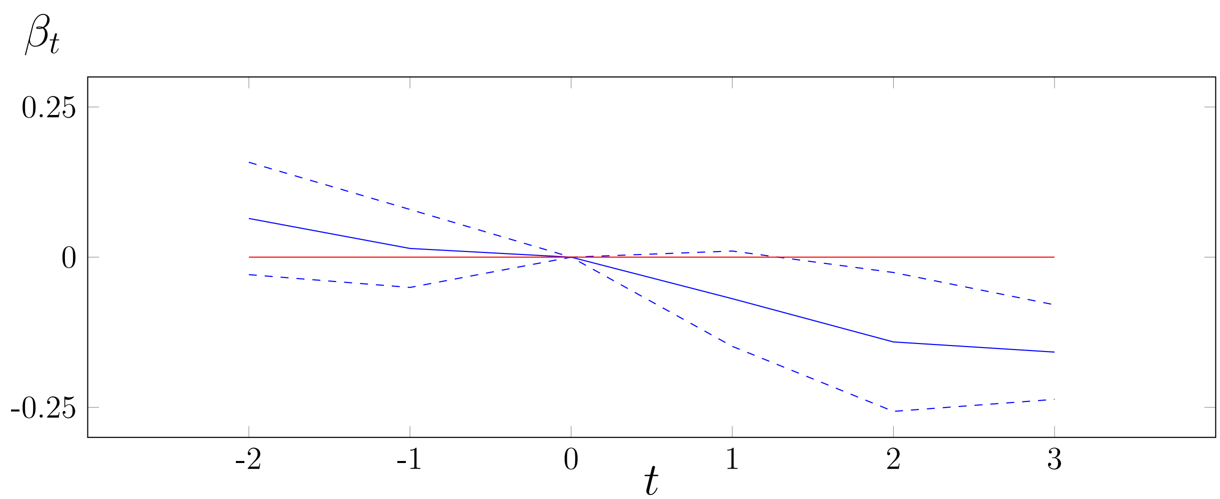


Figure 9
Event Study with Log(Wage) as Dependent Variable

Figure 7 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 to 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.

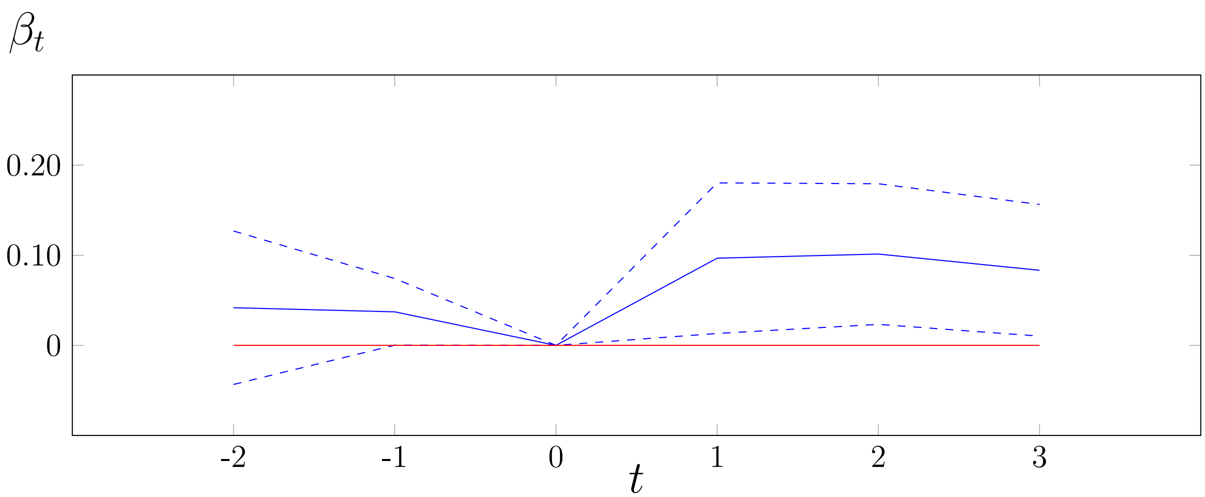


Table 1
Descriptive Statistics

Table 1 has descriptive statistics for the main variables used in the paper. Sales is revenues in million euros. Leverage is given by non current liabilities plus current liabilities, all divided by total assets. Net leverage is defined in the same way, but with cash subtracted from the numerator. Wage is the logarithm of total cost of employees divided by the number of employees (in thousand euros). Log(Length) is the logarithm of the average trial length. 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). Branches_{2012} is the number of bank branches per 100,000 inhabitants. Employment_{2012} and $\text{Unemployment}_{2012}$ are employment and unemployment rate, respectively, in percentage points. Value Added_{2012} is the value added per capita in thousand euros. All the geographic variables are measured at the province level at the end of 2012.

	N	Mean	Median	St. Dev.	Min	Max
Firm Variables						
Employees	73,190	31.64	15.00	48.21	1.00	249.00
Sales	73,186	8.31	3.29	14.04	0.58	73.53
Wage	73,059	38.35	36.60	14.74	11.92	82.29
Leverage	73,067	0.73	0.78	0.22	0.20	1.07
Net Leverage	72,800	0.64	0.71	0.28	-0.05	1.00
Return-on-Assets	72,413	0.07	0.06	0.08	-0.11	0.29
Court Variables						
Log(Length)	73,190	5.83	5.73	0.32	5.38	6.69
Δ	73,190	0.06	0.00	0.14	-0.18	0.34
$\Delta \times \mathbb{1}(t > 2013)$	73,190	0.03	0.00	0.11	-0.18	0.34
Post	73,190	0.49	0.00	0.50	0.00	1.00
Geographic Variables						
Branches_{2012}	73,190	61.11	59.44	16.00	30.58	88.95
$\text{Unemployment}_{2012}$	73,190	9.51	8.08	3.31	6.23	20.36
Employment_{2012}	73,190	71.00	71.69	5.12	55.10	78.59
Value Added_{2012}	73,190	24.84	26.76	4.99	13.77	45.53

Table 2
Baseline Results

Table 2 shows regressions testing the effect of trial length on employment. Column 1 through 3 show coefficients of 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 firm and 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, as well as columns 4 and 5, include only firms headquartered in cities located near the borders of the pre-reform court districts affected by the reform. Column 4 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 5 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	1st Stage	IV
	(1)	(2)	(3)	(4)	(5)
Log(Length)	0.032**	0.011	-0.140**		-0.359***
	(0.013)	(0.012)	(0.056)		(0.099)
$\Delta \times \mathbb{1}(t > 2013)$				0.685***	
				(0.101)	
Observations	753,730	455,054	73,190	73,190	73,190
R^2	0.925	0.925	0.933	0.993	0.933
Firm FE	X	X	X	X	X
Year FE	X				
Year-Border 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. All regressions include firm and border-year fixed effects. Column 1 shows the baseline regression. Column 2 includes year-industry dummies. Column 3 has border-year-industry dummies. Column 4 includes the following controls interacted with a post-reform dummy (not shown): number of branches per 100,000 inhabitants, unemployment rate, employment rate, value-added per capita (all measured at the province level), leverage, Log(assets) and ROA. All the control variables are measured at the end of the last year predating the reform (2012). Column 5 includes a dummy equal to 1 if a firm is located in a municipality whose court is suppressed after the reform. Column 6 excludes from the sample all the firms headquartered in the 11 provincial capitals in the sample. Column 7 and 8 include firms with at most and more than 15 employees as of the pre-reform year, 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/Suppr. Court Dummy	Excluding Large Cities	w/Number of Employees...	
							≤ 15	> 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(Length)	-0.359***	-0.303***	-0.382***	-0.308***	-0.344***	-0.290***	-0.335***	-0.472**
	(0.099)	(0.088)	(0.074)	(0.093)	(0.094)	(0.088)	(0.093)	(0.182)
Court Suppressed					-0.027			
					(0.017)			
Observations	73,190	73,033	57,678	72,747	73,190	44,855	37,943	34,775
R^2	0.933	0.936	0.947	0.934	0.933	0.929	0.837	0.884
F-Stat	45.679	48.046	35.932	48.639	66.683	63.781	44.964	43.068
Firm FE	X	X	X	X	X	X	X	X
Year-Border FE	X	X		X	X	X	X	X

Table 4
Tangibility, Uncertainty, and Social Capital

Table 4 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: tangibility (columns 1 and 2), uncertainty (columns 3 and 4), and social capital (columns 5 and 6). Tangibility is the ratio of fixed assets to total assets, measured at the end of 2012. Uncertainty is the standard deviation of changes in EBITDA divided by average total assets. Social capital is voters' turnout at the 2013 general election for the Chamber of Deputies. 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:					
	Tangibility		Uncertainty		Social Capital	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Length)	-0.515***	-0.269**	-0.214	-0.732***	-0.544***	-0.020
	(0.125)	(0.130)	(0.139)	(0.155)	(0.189)	(0.102)
Observations	24,165	24,190	23,637	23,613	26,219	21,128
R^2	0.934	0.926	0.951	0.911	0.920	0.948
F-Stat	42.177	49.979	40.328	39.701	39.786	68.083
$\beta_{Low} - \beta_{High}$	0.246**		-0.518***		0.524*	
S.E.	(0.126)		(0.189)		(0.213)	
Firm FE	X	X	X	X	X	X
Year-Border FE	X	X	X	X	X	X

Table 5
Heterogeneity in Leverage and Financial Dependence

Table 5 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. In columns 1 and 2 firms are sorted according to their leverage (total liabilities divided by total assets). In columns 3 and 4 firms are sorted according to the degree of financial dependence of their industry. 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	
	Low (1)	High (2)	Low (3)	High (4)
Log(Length)	-0.234***	-0.703***	-0.234***	-0.660***
	(0.063)	(0.200)	(0.079)	(0.166)
Observations	24,162	24,128	24,849	24,040
R^2	0.955	0.904	0.943	0.919
F-Stat	53.198	40.498	46.491	33.153
$\beta_{Low} - \beta_{High}$	-0.469**		-0.427***	
S.E.	(0.197)		(0.118)	
Firm FE	X	X	X	X
Year-Border FE	X	X	X	X

Table 6
Heterogeneity in Financial Development

Table 6 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 measures of financial development measured at the province level at the end of 2012. Branches × 100,000 inhabitants is the number of bank branches per 100,000 people. Credit to GDP is the ratio of medium and long term bank loans divided by value added. The average Tier 1 Ratio is the mean Tier 1 Ratio of all the banks operating in each province, with weights given by each bank's number of branches. 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: Branches × 100,000 Inhab.		$\frac{\text{Credit}}{\text{GDP}}$		Average Tier 1 Ratio	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Log(Length)	-0.707**	0.137	-0.599***	-0.045	-1.018*	-0.013
	(0.321)	(0.121)	(0.163)	(0.071)	(0.583)	(0.081)
Observations	28,683	21,661	35,554	24,188	27,825	20,479
R^2	0.917	0.944	0.922	0.943	0.925	0.932
F-Stat	5.292	51.862	17.471	78.255	57.147	108.237
$\beta_{Low} - \beta_{High}$	0.844**		0.554***		1.004*	
S.E.	(0.340)		(0.177)		(0.584)	
Firm FE	X	X	X	X	X	X
Year-Border FE	X	X	X	X	X	X

Table 7

Additional Outcomes and Heterogeneity in the Effects on Wages

Panels A and B of Table 7 have 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. In Panel A, the dependent variable is indicated in each column's header. Log(sales) is the logarithm of total revenues. Log(pay) is the logarithm of the total wage bill. Leverage is total liabilities divided by total assets. Net leverage is total liabilities minus cash, all divided by total assets. Log(wage) is the logarithm of the firm's average wage, equal to total labor cost divided by the number of employees. In Panel B the dependent variable is Log(wage). Firms are sorted according to leverage (columns 1 and 2) or uncertainty (columns 3 and 4). Uncertainty is the standard deviation of changes in EBITDA divided by average total assets. Firms belong to the "Low" or "High" sample if each measure is in the bottom or top sample tercile. 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.

Panel A. Additional Outcomes

	Dependent Variable:					
	Log(Sales)	Log(Pay)	ROA	Leverage	Net Leverage	Log(Wage)
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Length)	-0.144*	-0.214***	-0.003	0.006	0.006	0.099**
	(0.076)	(0.060)	(0.011)	(0.016)	(0.020)	(0.047)
Observations	72,995	73,055	72,392	73,067	72,794	73,055
R^2	0.915	0.940	0.703	0.917	0.905	0.752
F-Stat	45.561	45.723	46.195	45.697	45.828	45.723
Firm FE	X	X	X	X	X	X
Year-Border FE	X	X	X	X	X	X

Panel B. Heterogeneity in the Effects on Wages

	Sorting by:			
	Leverage		Uncertainty	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Log(Length)	0.105**	0.224**	0.027	0.123*
	(0.049)	(0.086)	(0.072)	(0.073)
Observations	24,158	24,126	23,633	23,605
R^2	0.800	0.698	0.785	0.728
F-Stat	53.315	40.501	40.329	39.784
$\beta_{Low} - \beta_{High}$	0.118		0.096	
S.E.	(0.089)		(0.081)	
Firm FE	X	X	X	X
Year-Border FE	X	X	X	X

A Appendix

This Appendix has three parts. Section A.1 explains in detail how the dataset was built, 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 3.1), 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. The website of the Chamber of Deputies provides a detailed report describing all the steps that brought 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`).

The two lists of municipalities are linked using a fuzzy matching algorithm to account for different spellings of the names.[28] 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, fusions of small municipalities have been incentivized through subsidies, and have been quite common since.) I identify such cases through web searches, and treat the affected municipalities as having been merged

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

throughout the full sample period. None of these mergers involved municipalities originally belonging to different court districts.

Amadeus reports the names of 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.

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 26 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, Genua	Genua
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
Leverage	Non current liabilities (*ncli*) plus current liabilities (*culi*), all divided by total assets (*toas*)	Amadeus
Net Leverage	Non current liabilities (*ncli*) plus current liabilities (*culi*) minus cash and cash equivalent (*cash*), all divided by total assets (*toas*)	Amadeus
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-digits SIC level). For consistency with the main analysis, only firms with total assets and sales over 1,000,000 euros in 2012 are kept.	Amadeus
Return-on-Asset	Earnings before interest, debt and amortization (*ebta*) divided by total assets (*toas*)	Amadeus
Tangibility	Fixed assets (*fias*) divided by total assets (*toas*)	Amadeus

Continued on next page

Table A2 – *Continued from previous page*

Variable	**Definition**	**Source**
Uncertainty	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
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
Branches_{2012}	Number of bank branches per 100,000 inhabitants, measured at the province level	Bank of Italy
Average Tier 1 Ratio	Given by: $\frac{\sum_i N_{i,j} \times Tier\,1_i}{\sum_i N_{i,j}}$, where $N_{i,j}$ indicates the number of branches belonging to bank i operating in province j. The weighted average is computed only across banks with non-missing Tier 1 ratio, obtained from Osiris.	Bank of Italy and Osiris
Credit to GDP	Ratio of medium and long term bank loans divided by value added, computed at the province level.	Bank of Italy and Italian Statistical Office
Employment_{2012}	Employment rate at the province level, measured at the end of 2012	Italian Statistical Office
$\text{Unemployment}_{2012}$	Unemployment rate at the province level, measured at the end of 2012	Italian Statistical Office
$\text{Value Added}_{2012}$	Value added at the province level, measured at the end of 2012	Italian Statistical Office

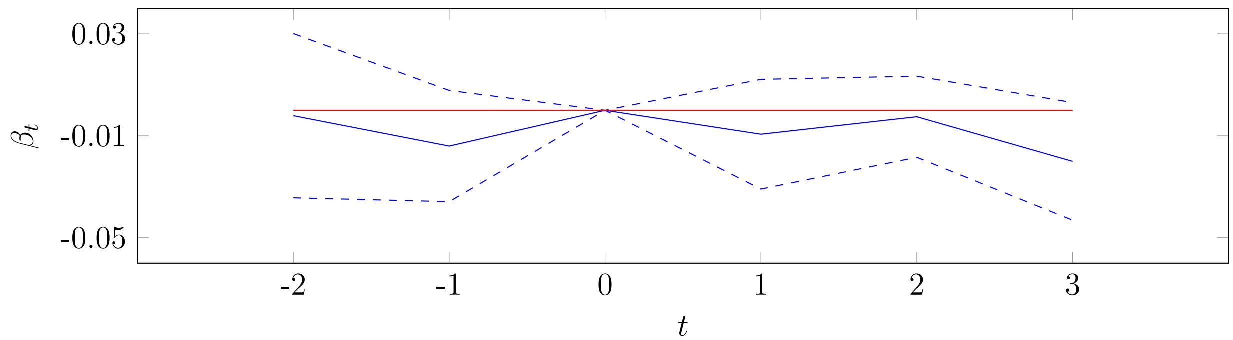
A.3 Additional Results

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.) Table A4 analyzes the effect of trial length on firms' entry and exit. The three tables that follow replicate Tables 4, 5, and 6 in the main text, but have as dependent variable either the logarithm of the total wage bill or the logarithm of revenues. More specifically, they analyze how the effect of trial length on total compensation and sales varies with respect to tangibility, uncertainty and social capital (Table A5), leverage and financial dependence (Table A6), and financial development (Table A7).

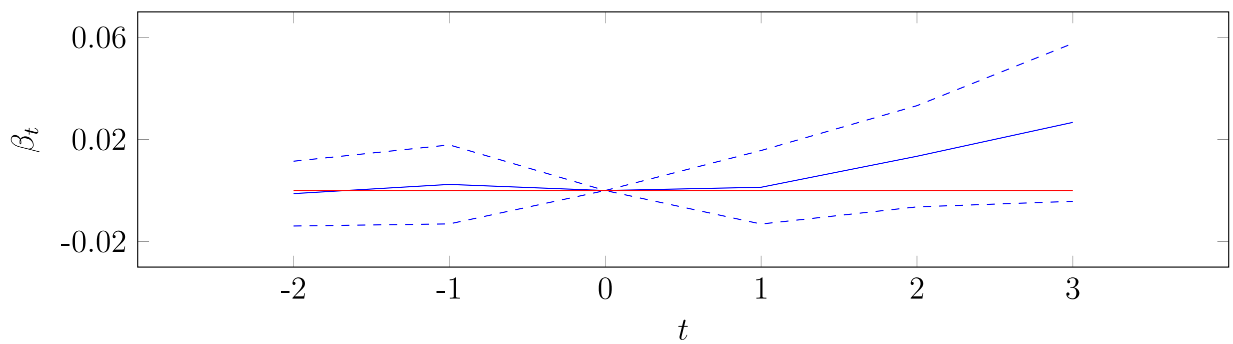
Figure A1
Additional Event Studies

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 to 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: Net Leverage

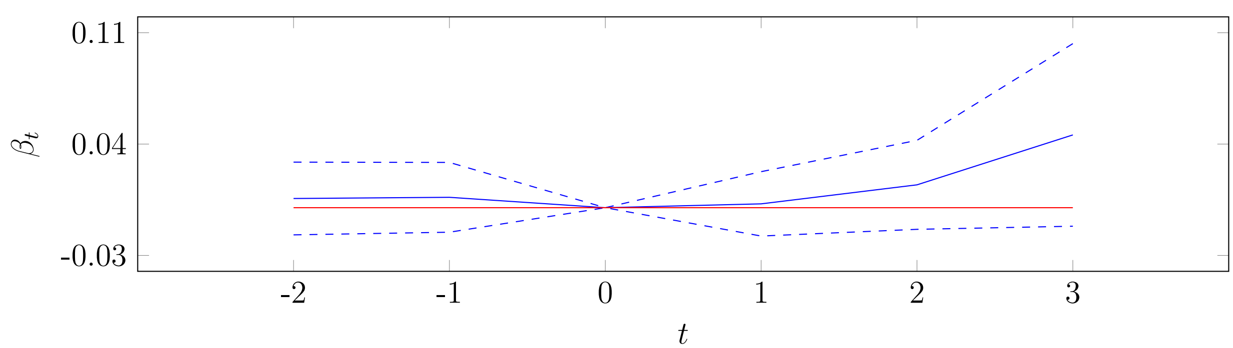


Table A3
Baseline Results with Alternative Sample Choice

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(Pay)
	OLS	1^{st} 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

Table A4
Firms' Entry and Exit

Table A4 shows regressions testing the effect of trial length on firms' entry and exit. The sample includes municipalities in districts affected by the reform and that share a border with other affected districts. All regressions include municipality fixed effects 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, 3 and 4 show results from second-stage regressions where the dependent variables are the logarithms of the number of firms created, the number of firms' closures, and the difference between the two, 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)	Log(# Entry) −Log(# Exit)
	1^{st} Stage	IV	IV	IV
	(1)	(2)	(3)	(4)
$\Delta \times \mathbb{1}(t > 2013)$	0.731***			
	(0.063)			
Log(Length)		-0.286**	-0.296*	0.036
		(0.138)	(0.152)	(0.158)
Observations	6,525	6,525	6,525	6,525
R^2	0.990	0.926	0.929	0.376
F-Stat		132.485	132.485	132.485
City FE	X	X	X	X
Year-Border FE	X	X	X	X

Table A5

Tangibility, Uncertainty, and Social Capital: Total Pay and Revenues

Panels A and B of Table A5 have regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. The dependent variable is the logarithm of the total wage bill in Panel A, and the logarithm of revenues in Panel B. All regressions include firm and border-year fixed effects. Firms are sorted according to three variables: tangibility (columns 1 and 2), uncertainty (columns 3 and 4), and social capital (columns 5 and 6). Tangibility is the ratio of fixed assets to total assets, measured at the end of 2012. Uncertainty is the standard deviation of changes in EBITDA divided by average total assets. Social capital is voters' turnout at the 2013 elections for the Chamber of Deputies. 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.

Panel A. Dependent Variable: Log(Pay)

	Sorting by: Tangibility		Uncertainty		Social Capital	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Length)	-0.332***	-0.146	-0.102	-0.553***	-0.181*	-0.062
	(0.111)	(0.109)	(0.105)	(0.118)	(0.098)	(0.060)
Observations	24,039	24,187	23,633	23,605	26,188	21,122
R^2	0.941	0.936	0.967	0.907	0.933	0.951
F-Stat	42.273	49.979	40.329	39.784	39.850	68.086
$\beta_{Low} - \beta_{High}$	0.186***		-0.451***		0.119	
S.E.	(0.161)		(0.139)		(0.114)	

Panel B. Dependent Variable: Log(Sales)

	Sorting by: Tangibility		Uncertainty		Social Capital	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Length)	-0.372**	-0.063	-0.316***	-0.130	-0.218*	0.197***
	(0.179)	(0.102)	(0.111)	(0.102)	(0.109)	(0.066)
Observations	24,073	24,143	23,615	23,497	26,119	21,096
R^2	0.894	0.928	0.951	0.871	0.904	0.934
F-Stat	41.523	49.791	40.327	39.067	39.485	68.170
$\beta_{Low} - \beta_{High}$	0.309		0.185*		0.415***	
S.E.	(0.193)		(0.094)		(0.126)	

Table A6
Heterogeneity in Leverage and Financial Dependence: Total Pay and Revenues

Panels A and B of Table A6 have regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. The dependent variable is the logarithm of the total wage bill in Panel A, and the logarithm of revenues in Panel B. All regressions include firm and border-year fixed effects. In columns 1 and 2 firms are sorted according to their leverage (total liabilities divided by total assets). In columns 3 and 4 firms are sorted according to the degree of financial dependence of their industry. 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.

Panel A. Dependent Variable: Log(Pay)

	Sorting by:			
	Leverage		Financial Dependence	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Log(Length)	-0.114	-0.402*	-0.262***	-0.336**
	(0.075)	(0.208)	(0.067)	(0.142)
Observations	24,158	24,126	24,789	23,989
R^2	0.963	0.906	0.947	0.927
F-Stat	53.315	40.501	46.490	33.223
$\beta_{Low} - \beta_{High}$	-0.287*		-0.074	
S.E.	(0.161)		(0.174)	

Panel B. Dependent Variable: Log(Sales)

	Sorting by:			
	Leverage		Financial Dependence	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Log(Length)	0.144	-0.462**	-0.098	-0.410***
	(0.132)	(0.212)	(0.066)	(0.134)
Observations	24,139	24,012	24,801	23,929
R^2	0.940	0.874	0.919	0.902
F-Stat	52.587	40.440	45.766	32.997
$\beta_{Low} - \beta_{High}$	-0.606***		-0.311**	
S.E.	(0.208)		(0.148)	

Table A7

Heterogeneity in Financial Development: Total Pay and Revenues

Panels A and B of Table A7 have regressions of Log(employees) on Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy. The dependent variable is the logarithm of the total wage bill in Panel A, and the logarithm of revenues in Panel B. All regressions include firm and border-year fixed effects. In columns 1 and 2 firms are sorted according to their leverage (total liabilities divided by total assets). In columns 3 and 4 firms are sorted according to the degree of financial dependence of their industry. 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.

Panel A. Dependent Variable: Log(Pay)

	Sorting by:					
	Branches × 100,000 Inhab.		$\frac{\text{Credit}}{\text{GDP}}$		Average Tier 1 Ratio	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Log(Length)	-0.442***	0.093	-0.349***	-0.022	-0.244	-0.052
	(0.129)	(0.094)	(0.072)	(0.086)	(0.340)	(0.055)
Observations	28,587	21,656	35,458	24,184	27,818	20,448
R^2	0.930	0.948	0.932	0.948	0.937	0.940
F-Stat	5.297	51.860	17.491	78.259	57.213	108.234
$\beta_{Low} - \beta_{High}$	0.535		0.327***		0.192	
S.E.	(0.158)		(0.110)		(0.342)	

Panel B. Dependent Variable: Log(Sales)

	Sorting by:					
	Branches × 100,000 Inhab.		$\frac{\text{Credit}}{\text{GDP}}$		Average Tier 1 Ratio	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Log(Length)	-0.479***	0.260***	-0.302***	0.087	-0.350	0.035
	(0.143)	(0.088)	(0.091)	(0.076)	(0.369)	(0.071)
Observations	28,593	21,624	35,444	24,149	27,751	20,422
R^2	0.898	0.934	0.904	0.929	0.916	0.909
F-Stat	5.253	51.896	17.274	78.221	55.855	108.278
$\beta_{Low} - \beta_{High}$	0.739***		0.389***		0.385	
S.E.	(0.169)		(0.117)		(0.373)	

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