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The Real Effects of Judicial Enforcement: Evidence from Italy

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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", that is, unobserved factors that 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% reduction in the duration of civil trials increases firm-level employment by 2.4%-2.9%. 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 from 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 in protecting investors and fostering economic growth.

The Real Effects of Judicial Enforcement: Evidence from Italy*

Vincenzo Pezone†

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Abstract

I analyze the real effects of the quality of the judicial enforcement by showing that an increase in the average duration of civil proceedings reduces firms' employment. I exploit a reorganization of court 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 -0.24 and -0.29. These results are very different from OLS estimates which do not control for endogeneity, and suggest that stronger law enforcement eases financing constraints. The effects are more pronounced in highly levered and more financially dependent firms, and appear to affect mainly firms in less financially developed areas. Revenues respond more slowly than employment to the reform, and wages fall as the judiciary improves. There is no evidence of effects on capital structure and profitability. 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 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. Comparing legal systems from different countries (La Porta, de Silanes, Shleifer, and Vishny (1997)) is suggestive of how different legislations may have shaped economic outcomes, but too many potential confounding factors discourage causal interpretations. Restricting the focus on a single country solves some of these issues, but has its own disadvantages, too. First, 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. Second, even if sufficient variation exists, it may be hard to make causal claims without a natural experiment. Third, works focusing on a single economy often lack external validity because study legal environments that are often very country-specific.

This paper will 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. I will argue that this setting helps to address all three concerns. As for the lack of variation, although Italy has been subject to the same civil code for over 150 years, it displays striking heterogeneity in the quality of the judiciary across regions, and also within regions across time (Bianco, Giacomelli, Giorgiantonio, Palumbo, and Szego (2007)). Regarding the second concern, i.e. the lack of proper natural experiments in most of the literature, this paper will analyze the effects of a plausibly exogenous shock. Finally, the crucial variable in my tests is the average duration of court proceedings, a measure that is comparable even across countries (Palumbo, Giupponi, Nunziata, and Sanguinetti (2013)), potentially making these estimates valid externally.

The following example helps to clarify my identification strategy. The courts A and B are 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 would B 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 lasting 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 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), Giacomelli and Menon (2016), and Bonetti (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 then hypothesize that imperfect contract enforcement due to slow courts will exacerbate firms' financing constraints, in turn dampening employment growth. 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 estimate an elasticity of employment to trial length between -0.244 and -0.292, depending on the specification, which is economically large and precisely estimated. In contrast, naïve OLS regressions that do not take into account the endogeneity of the judiciary produce elasticities that are small

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

and positive, even after controlling for local shocks. Back-of-the-envelope calculations show that the difference in the degree of law enforcement may be responsible for a large fraction of the difference in employment outcomes between the most and the least developed areas of the country, North and South, respectively (Felice (2015)).

Importantly, I also show in an event-study framework 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.

The results are robust to a battery of robustness tests. My sample size is large enough to allow for non-parametric controlling not only for local shocks, but also for local industry-level shocks. Moreover, the baseline tests do not capture a small firm effect; if anything, results are 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. I these were the case, I should then find evidence of higher employment growth in districts that are 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 channel underlying the results. Confirming that the financing channel is at play, 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)).

A less obvious prediction has to do with the interaction between financial development and quality of the legal institutions. If the two are complements, we should observe stronger effects of trial length on employment in more financially developed areas. Alternatively, effective courts may be a substitute for the presence of well-functioning credit markets. Using different proxies for financial development, I find that the latter hypothesis finds support in the data.

I also analyze other firm-level outcomes. I find evidence of a negative impact of trial duration on sales; however, this estimate is quite imprecise. Further investigation shows that revenues do respond to the change in trial length induced by the reform, but with some delay. This is consistent with firms building capacities as courts become more efficient, although they are unable to quickly expand their output. As a result, output per worker falls, and so do wages, in line with a simple bargaining story. I do not find, instead, effects on profitability and capital structure.

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. It also examines a number of different outcomes, such as wages or revenues together with employment, and sketches a simple unifying explanation of their comovement. Finally, it also raises a number of questions that may be fruitful to address in future research, related to the refinement of the economic mechanism at work, and to the interaction between legal institutions and financial development.

This paper is organized as follows. Section 2 summarizes the hypotheses to be tested and the relevant 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 Hypothesis Development and Related Literature

2.1 Theoretical Framework and Hypotheses

The basic argument guiding the empirical tests in this paper can be summarized in two steps. First, non-functioning legal institutions are unable to protect outside investors from managerial moral hazard or asymmetric information, increasing the cost of financing. Second, to the extent the employment is, at least in part, funded through external funds, financially constrained firms will reduce employment. If both links are true, employment should rise together with the quality of the judiciary, measured in this paper by the average duration of civil proceedings.[2] Hence, the first hypothesis is as follows:

H1. A reduction in average trial length boosts firms' employment.

Based on the second step of the argument just sketched, legal institutions should matter the most for firms depending on external financing. Moreover, the reduction in the cost of external financing should be more pronounced for firms in which negative cash-flow shocks are more likely to constrain employment, such as those with high levels of debt. Thus, we can formulate a second hypothesis:

H2. The effect of a reduction of the average trial length on firms' employment will be stronger in firms that are highly dependent on external funds or in firms with high leverage.

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. 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. Therefore, the third hypothesis is:

H3. Lower trial length will boost employment more in financially developed areas if financial development and judicial productivity are complements. Alternatively, its effect on employ-

[2] This measure is used in a number of papers, many of which are surveyed in Section 2.2. Moreover, It is often referenced by policy organizations, such as the OECD, the World Bank or the CEPEJ (European Commission for the Efficiency of Justice).

ment will be stronger in less financially developed areas if financial development and judicial productivity are substitutes.

2.2 Literature Review

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 the Law and Finance field. 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. Haselmann, Pistor, and Vig (2009) study a sample of banks in twelve transition economies of Central and Eastern Europe using a difference-in-difference strategy, and find that collateral law appears to matter the most for promoting lending than bankruptcy law. Lerner and Schoar (2005) instead study the effect of law enforcement on private equity investments in a cross-section of countries; Bae and Goyal (2009) and Qian and Strahan (2007) also perform a cross-country analysis on a sample of corporate loans and show that effective law enforcement is associated with lower interest spread and higher maturity.

More recently, some studies have looked at quantitative measures of the quality of the judiciary, typically the average duration of civil proceedings, and, like this paper, use single-country data. 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. Bonetti (2016) uses a similar design and analyzes the passage of a reform in Italy aimed at facilitating the renegotiation of assets of troubled firms, finding that it reduces earnings management in firms located in districts with more efficient courts. Rodano, Serrano-Velarde, and Tarantino (2016) study the same reform, and show how its effects on corporate loans vary with the judicial productivity across courts.

Unlike this work, none of these papers focuses on the judiciary per se; rather, they study how the legal system interacts with other provisions.

Papers that focus explicitly on the effects of average trial duration are Giacomelli and Menon (2016) and Laeven and Woodruff (2007), who find that court productivity exhibits a positive association with firm size, using data from Italy and Mexico, respectively. Jappelli, Pagano, and Bianco (2005) show theoretically and empirically that slower courts may induce borrowers to behave strategically and reduce credit availability. This paper shares the spatial discontinuity-design with Ponticelli and Alencar (2016), Giacomelli and Menon (2016) and Bonetti (2016). Despite the advantages of this strategy, they 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 exogenous variation in trial duration *over time*. Therefore, I can include in my regression firm dummies that will absorb all the time invariant characteristics that may determine a firm's sorting. In addition, my rich firm-level dataset allows me to analyze a number of different outcomes, and explore the heterogeneity of the results based on a number of characteristics.

A more recent and active literature has the intersection of labor and finance in 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 cashflow 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 crucial in determining employment growth.

Finally, this paper 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 6.2 I find evidence of substitutability between the two.

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 et al. (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. During the sovereign debt crisis, the incumbent Experts' Cabinet put forward a number of measures both to reassure investors and to regain credibility towards foreign partners. 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 small courts. The district of each suppressed court was then merged with an adjacent district of a surviving court,

[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)

with these changes becoming effective in September 2013.[4]

No judges or other employees were fired; however, the reorganization planned to obtain savings thanks to the reutilization of the courts' facilities (often large, historical buildings). More importantly, the legislator planned to exploit economies of scale due to the increased specialization of the judges: small courts may force each judge to work on cases requiring very different kinds of expertise.

Importantly, 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, the main ones 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] In other words, no reference was made to the actual productivity (or trends in productivity) of the pre-existing courts when deciding which were to be suppressed.

Figure 1, Panels A and B, provides 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 legal environment.

[4] See Legislative Decree 155/2012.

[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).

3.2 Predicting Average Trial Length Following 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:

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

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.

Going back to Figure 1, suppose that, in 2012, we had to predict the productivity 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 the 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

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

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

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

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 legal environments may affect 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 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 equation 2 above is 353, which is quite close.[7]

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

[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.

recently in the 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.

Before moving to the data, it is important to validate the proxy for trial length defined in equation 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 to to employ a log-transformation of trial length, which will make the scale of the regressor irrelevant.[8]

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,

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

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 court data are in Appendix A.1

Additional control variables, at the provincial level, are obtained by combining data from 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 68,928 company-years and 13,456 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), so that the number of firm employees is capped at 241.

The trial length goes from 150 to 1,674 days, with a mean of 368 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 65% 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 trial length, which has a standard deviation of 0.14, comparable in terms of magnitude to the standard deviation of Log(Length) (0.33).

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), and return-on-assets (ROA, defined as earnings before interest, debt and amortization divided by 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.A detailed definition and source of each variable can be found 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:

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

$\text{Length}_{k,i,b,t}$ 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. Finally, $\epsilon_{k,i,b,t}$ is an error term. Based on the discussion of Section 3.2, I expect the sample coefficient $\hat{\alpha}$ to be greater than zero.

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:

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

The estimate of the coefficient $\hat{\beta}$ is the main object of interest in the analysis. Under the assumption that the instrument $\Delta \times \mathbb{1}(t > 2013)$ is valid, it will provide the elasticity of firms' employment to the trial length, which is expected to be lower than zero.

In order to corroborate the validity of my 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. In column 1, 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 about 726,000 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 and *positive* (0.024), albeit significant only at the 10% level.

In column 2, I adopt the spatial discontinuity-design described in Section 3.2. Thus, I include border-year dummies in the regression 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 (0.013) 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 coefficient switches to negative (-0.098) 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 0.72% rise in actual length of proceedings, on average. The t-statistic is 7.42, suggesting that the instrument is definitely strong.

Column 5 includes 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 -0.29, with a t-statistic equal to -4.40. This number is much larger in absolute value than any of the coefficients estimated in the OLS regressions, and large 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 when $\tau > 0$. The three pre-reform coefficients are all very close to zero for $\tau = -2, 1, 0$ and fall to -0.21, -0.25, and -0.20 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, I only report the second-stage regressions and the F-statistics. Column 2 non-parametrically controls for time-varying industry trends by adding time-industry dummies. (Here and in what follows I will be using the 3-digits SIC code classification, the most refined available in Amadeus.) The point estimate drops a bit in magnitude, from -0.292 to -0.244, 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 estimated more precisely.

In column 4, I control for variables which 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 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 firms, 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 headquartered 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) is identical to the one estimated on the full sample.

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.[9] Column 5 includes firms with at most 15 employees, and column 6 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 the estimates suggest, if anything, a larger effect in larger firms (-0.249 versus -0.397). 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 so it is relevant from a macroeconomic perspective.

[9] 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. 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 (2016). Section 5.3 addresses the concern that the impact of the reform studied here may be confounded by the 2014 labor market reform.

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 less effective. 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 the 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 -0.206); more precisely, the fraction of coefficients lower than the true coefficient is only 0.36%. Finally, the average coefficient estimated from these "placebo reforms" is -0.01, which is very close to zero and much smaller in absolute vakye than the coefficient estimated for the courts affected by the reform, suggesting that the results I find are unlikely to be due 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.[10]

Suppose that all the Southern courts had a duration of civil proceedings equal to the median Northern court. Using the baseline estimate of -0.292 found in Table 5.1, the Southern regions would have about 662,000 additional workers employed, corresponding to a 4.85% increase in the employment rate. Given that 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 almost a quarter of the gap in employment outcomes. The estimate smallest in absolute value from Table 5.1, -0.244, would still produce a 4.05% 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.[11] 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 6.2, 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 (2004)). Overall, while these numbers should be taken with a grain of salt, it is reassuring that they are neither economically marginal nor implausibly large.

6 The Economic Mechanism

6.1 Leverage and Financial Dependence

Benmelech et al. (2011) show that there is a stronger relationship between employment and

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

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

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.[12] 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.

Following standard practice (Farre-Mensa and Ljungqvist (2016), Chaney, Sraer, and Thesmar (2012)), I split firms in terciles for each of the sorting variables, leverage and financial dependence, 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 t-statistics.

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, -0.241, is significant at the 10% level. The difference between the coefficients is larger when we look at columns 3 and 4, where the sorting variable is financial dependence. Here the difference is -0.332, larger in magnitude and significant at the 1% level.

[12]Amadeus does not report capital expenditures, so I follow Acharya, Eisert, Eufinger, and Hirsch (2016) 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 positive levels of investment are kept.

6.2 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 given 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 5 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 × 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.[13] 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)).

[13]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 31st 2012, and then, for each province, construct the weighted average as:

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

where $N_{i,j}$ indicates the number of branches belonging to bank i operating in province j. Tier 1 ratios are from Osiris.[14]

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 -0.657, -0.477 and -0.815. 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.[15]

In sum, Table 5 supports a substitution hypothesis, although it is not easy to further refine the economic channel further. Investors other than banks may be more willing to

[14]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.

[15]Notice however that the results that adopt the branches per 100,000 inhabitants proxy should be interpreted with caution due to the low F-statistic in the "Low" subsample.

provide funds 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.

6.3 Revenues, Wages, and Other Outcomes

This section analyzes other outcomes of interest beyond employment. It is natural to imagine that if court productivity spurs employment, overall real activity, and thus revenues, should follow the same pattern. In Table 6 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 -0.093, but is not precisely estimated (standard error=0.058). The elasticity of sales to trial length is about a third than that 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 due 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.

Given the results on employment and revenues, it is natural to ask how wages respond to changes in the quality of law enforcement. In column 3, the dependent variable is Log(wage), which displays a positive relationship with trial length (coefficient=0.13, t-statistic=2.46). This is consistent with a bargaining story. Given that employment reacts to change in judicial productivity more than revenues, output per worker moves in the opposite direction, and so does each workers' share of the total revenues. Figure 8 is coherent with this explanation and with Figures 5 and 7. Suppose a firm is headquartered in a district where average trial duration falls as a result of the reform. Managers hire more workers but production does not immediately reach full capacity. Then, output per worker, and thus wages, fall as a result. However, as output rises and employment remains constant, wages also rise; therefore, the

β_τs in Figure 8 moves towards zero.

Unfortunately, the post-reform period is relatively short, so it is not possible to examine long-term reactions to improvement in the judiciary. In a steady-state equilibrium, wages may rise if more effective law enforcement also causes productivity to grow. Alternatively, wages may fall if managers, thanks to their improved access to financing, are able to reduce turnover, and therefore risk premia included in wages, as predicted by implicit contract models (Baily (1974), Azariadis (1975)). This would also be consistent with the evidence of Table 4, showing that employment effects are stronger in firms with higher leverage, where risk premia should be higher.

Because employment in the short term responds to improvements in the quality of the judiciary more than proportionally than production, profitability will fall if wages do not adjust enough. In column 3 of Table 6 the dependent variable is return-on-assets (ROA); there is no evidence that this is the case.

Table 6 also examines the effects of average trial duration on capital structure. Jappelli et al. (2005) suggest that access to credit should be constrained by imperfect law enforcement because borrowers may act opportunistically. At the firm-level, however, Fabbri (2001) finds no association between corporate leverage and the length of trials in a sample of Spanish and Italian firms.

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.

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 the lack of evidence of any effect of the reform on profitability and capital structure.

7 Conclusion

This paper has presented evidence of the real effects of judicial productivity, by showing that trial duration has a first order impact on firm employment and wages. Effects on sales are more muted. The institutional setting of the reform I have studied, as well as a number of robustness tests, provide reassuring evidence on the causal nature of such results.

The effects on employment are more pronounced in highly levered and more financially dependent firms. Moreover, lowering trial length appears to be particularly effective in areas with poor financial development. The stark differences between 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 countries or 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 be able to enforce contracts with low costs, making soft-information less important. 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 performance and firms' outcomes.

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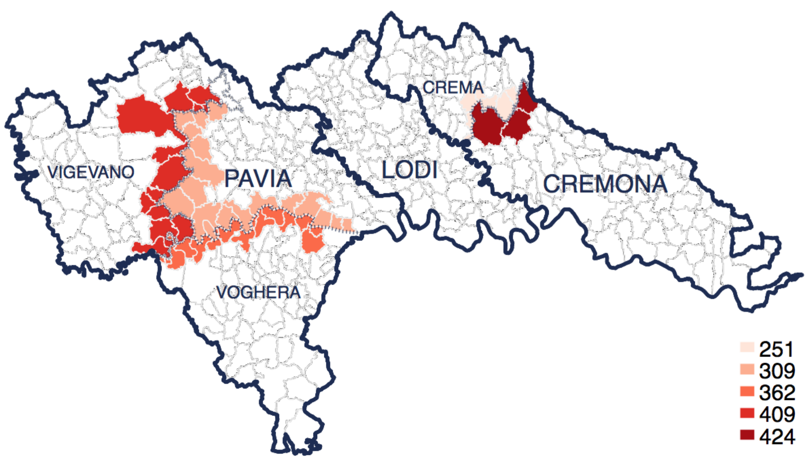
8 Figures and Tables

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 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

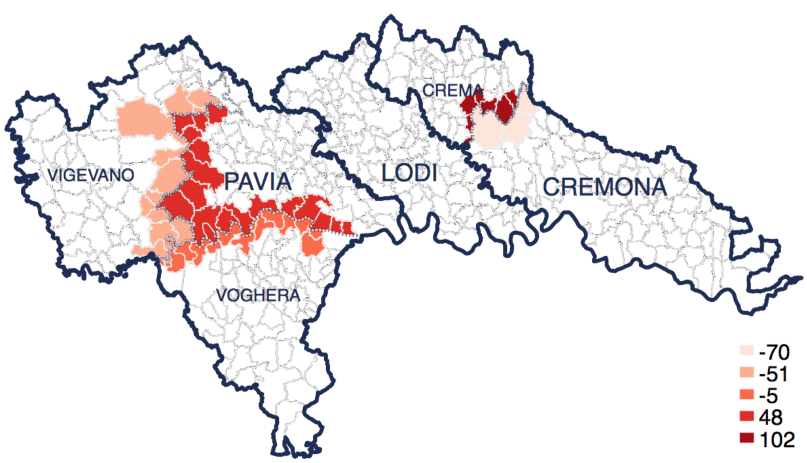


Figure 2
Actual Versus Estimated Trial Length

Figure 2 shows a scatter plot of actual versus estimated trial lengths across Italian districts in 2016. Estimated length is obtained using the equation $\text{Length}_{\text{t}} = (\text{Pending}_{\text{t-1}} + \text{Pending}_{\text{t}})/(\text{Incoming}_{\text{t}} + \text{Resolved}_{\text{t}}) \times 365$. The red line is obtained by regressing the actual length on the estimated length.

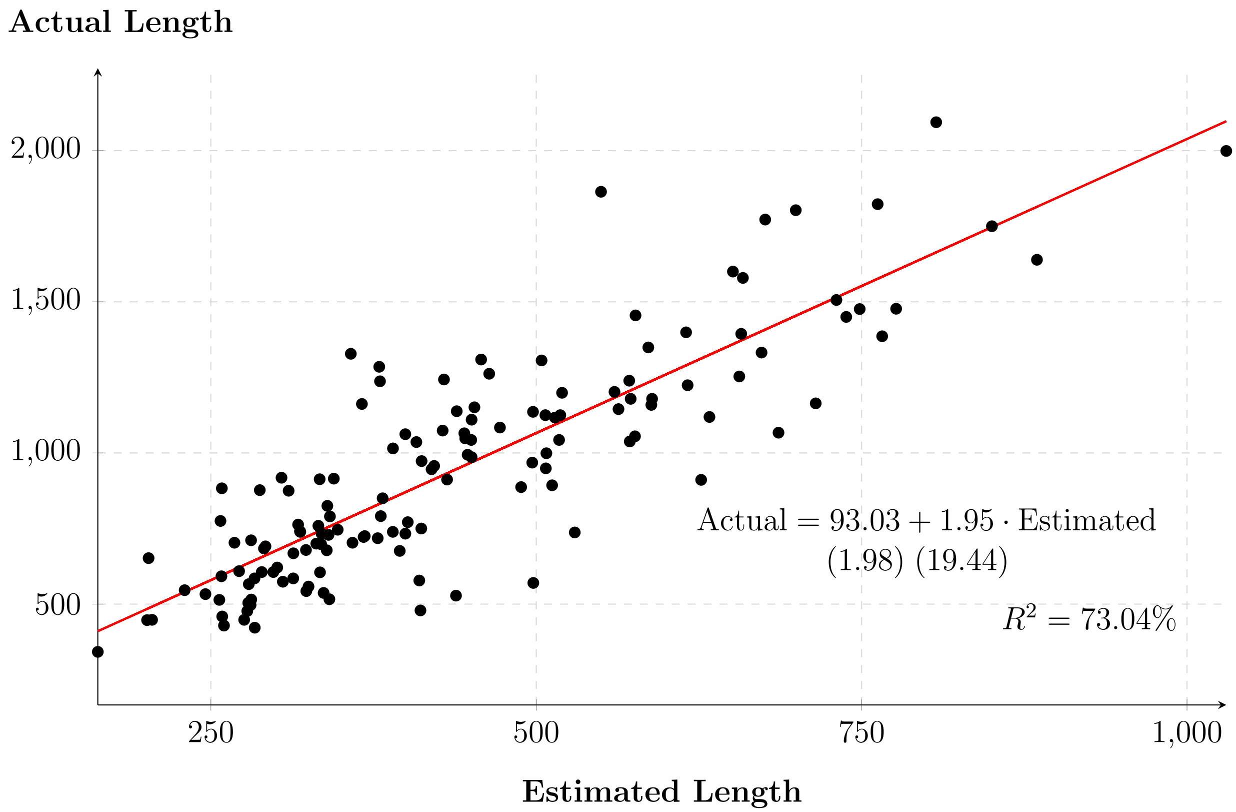


Figure 3
Trial Length across Italian Districts

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

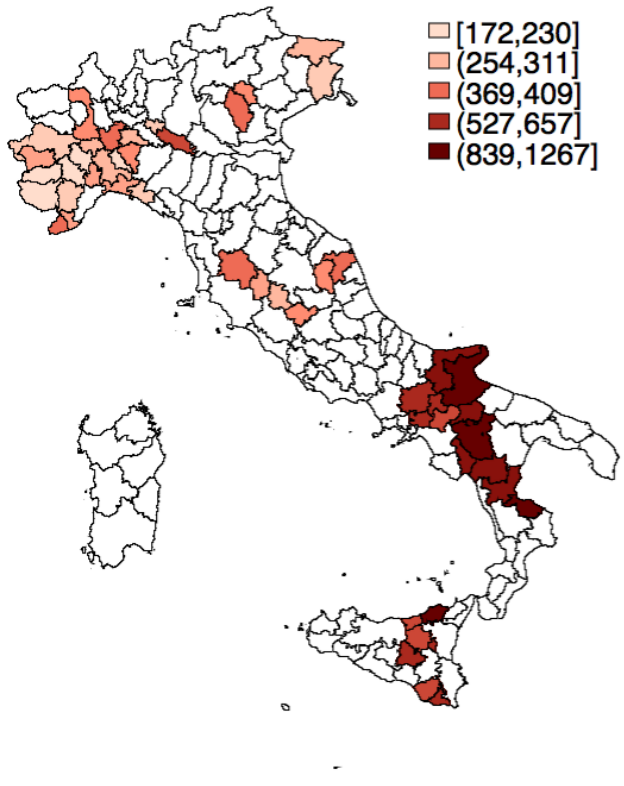


Figure 4

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.

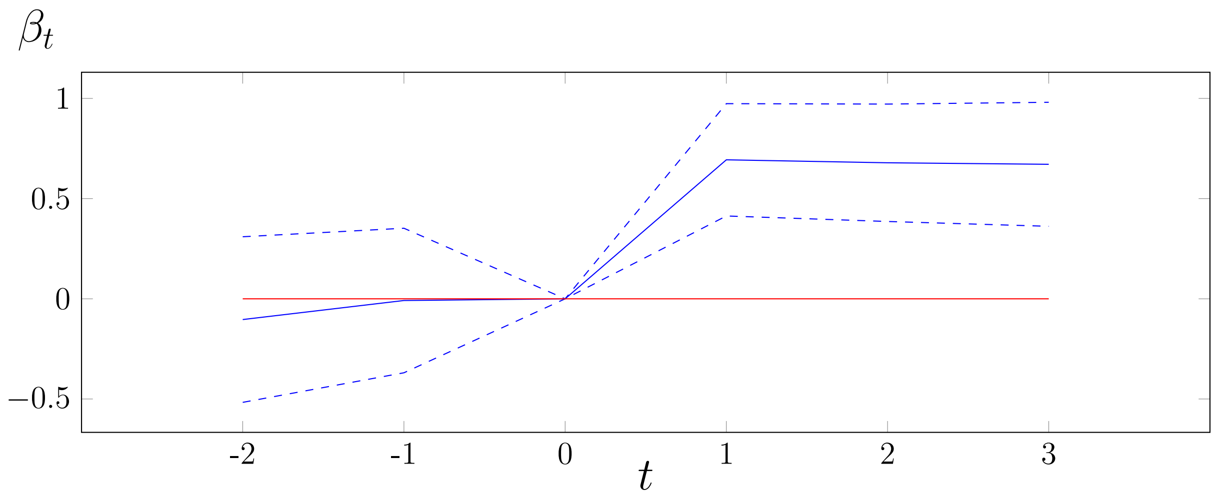


Figure 5

Event Study with Log(Employees) as Dependent Variable

Figure 7 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.

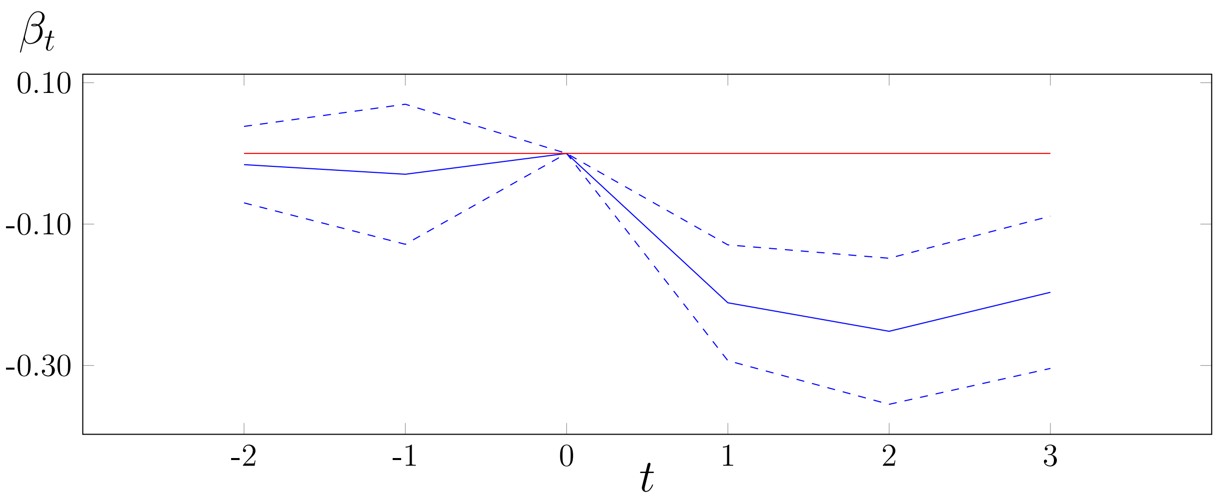


Figure 6
Placebo Test

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.

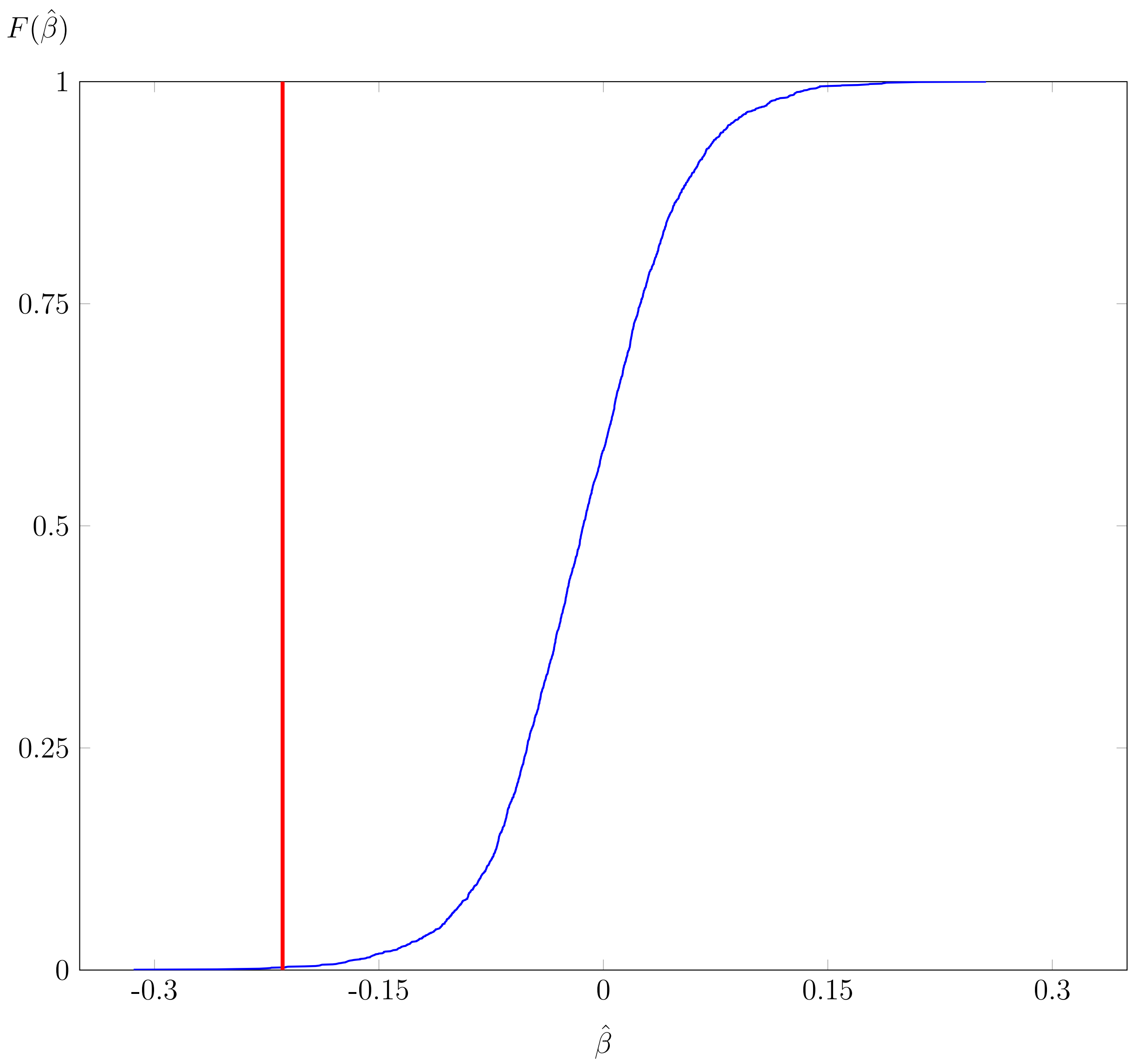


Figure 7
Event Study with Log(Sales) as Dependent Variable

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.

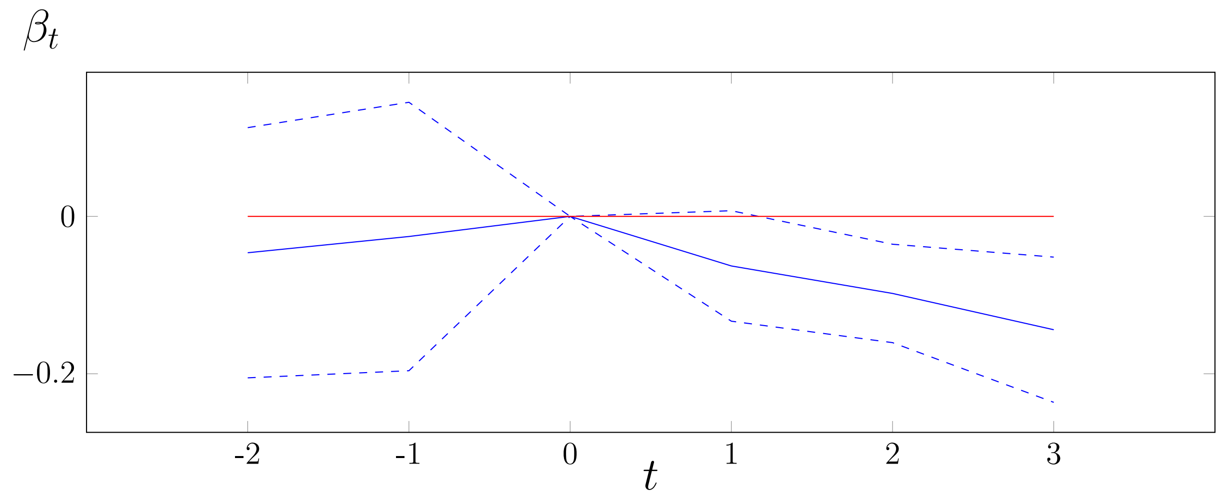


Figure 8
Event Study with Log(Wage) as Dependent Variable

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.

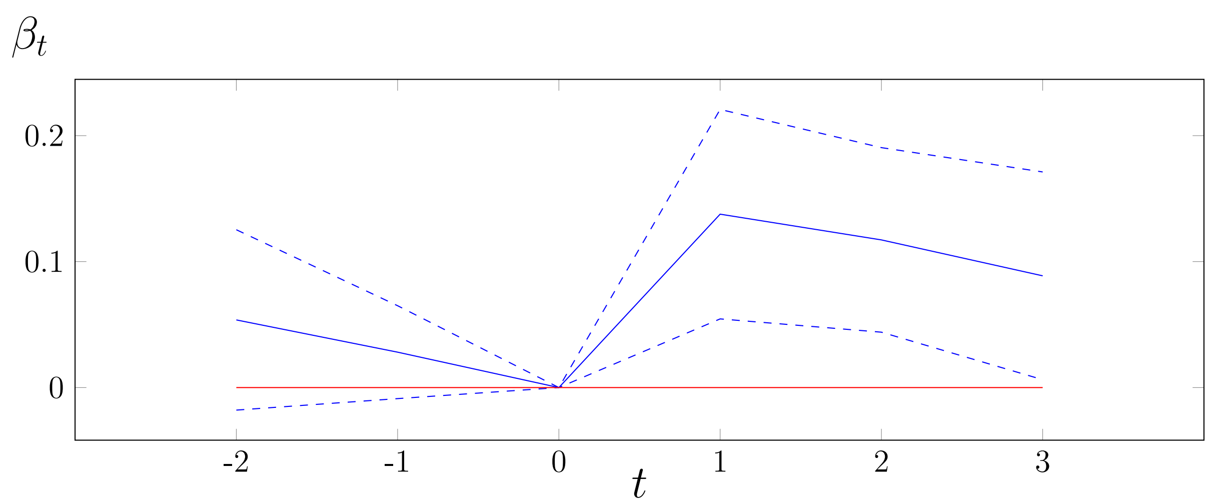


Table 1
Descriptive Statistics

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 equal to leverage, but with cash subtracted from the denominator. 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.	2.5 Perc.	97.5 Perc.
Employees	68,928	31.17	15.00	46.76	1.00	241.00
Sales	68,924	8.08	3.28	13.33	0.63	69.17
Wage	68,861	38.23	36.56	14.62	11.86	81.56
Leverage	68,864	0.73	0.78	0.22	0.20	1.06
Net Leverage	68,617	0.65	0.72	0.27	-0.05	1.01
Return-on-Assets	68,259	0.07	0.06	0.08	-0.11	0.29
Court Variables						
Log(Length)	68,928	5.83	5.73	0.33	5.33	6.72
Δ	68,928	0.06	0.00	0.14	-0.18	0.34
$\Delta \times \mathbb{1}(t > 2013)$	68,928	0.03	0.00	0.10	-0.18	0.34
Post	68,928	0.44	0.00	0.50	0.00	1.00
Geographic Variables						
Branches_{2012}	68,928	61.27	59.44	16.06	30.58	88.95
$\text{Unemployment}_{2012}$	68,928	9.49	8.08	3.30	6.23	20.36
Employment_{2012}	68,857	71.00	71.69	5.07	54.94	78.32
Value Added_{2012}	68,928	24.85	26.76	5.00	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 (1)	OLS (2)	OLS (3)	1^{st} Stage (4)	IV (5)
Log(Length)	0.024*	0.013	-0.098**		-0.292***
	(0.013)	(0.010)	(0.038)		(0.066)
$\Delta \times \mathbb{1}(t > 2013)$				0.718***	
				(0.097)	
Observations	726,437	433,012	68,817	68,817	68,817
R^2	0.931	0.931	0.938	0.992	0.938
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: 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 measured at the end of the last year predating the reform (2012). Column 5 and 6 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 $\times$ *Post* Dummy	Excluding Large Cities	w/Number of Employees...	
						≤ 15	> 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(Length)	-0.292***	-0.244***	-0.268***	-0.248***	-0.291***	-0.249***	-0.397**
	(0.066)	(0.065)	(0.064)	(0.062)	(0.080)	(0.065)	(0.148)
Observations	68,817	68,645	53,752	68,364	42,526	35,710	32,696
R^2	0.938	0.941	0.950	0.939	0.933	0.846	0.892
F-Stat	55.114	57.840	44.585	60.295	72.806	53.192	53.655
Firm FE	X	X	X	X	X	X	X
Year-Border FE	X	X		X	X	X	X

Table 4
Heterogeneity in Firms and Industry Characteristics

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. 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 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)	-0.265***	-0.506***	-0.133**	-0.465***
	(0.075)	(0.116)	(0.062)	(0.109)
Observations	22,737	22,693	23,299	22,581
R^2	0.958	0.911	0.950	0.924
F-Stat	65.456	52.270	55.897	40.033
$\beta_{Low} - \beta_{High}$	-0.241*		-0.332***	
S.E.	(0.137)		(0.109)	
Firm FE	X	X	X	X
Year-Border FE	X	X	X	X

Table 5
Heterogeneity in Financial Development

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. Firms are sorted according to three measures of financial development measured at the province level at the end of 2012. Credit to GDP is the ratio of medium and long term bank loans divided by value added and 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)	-0.553*	0.033	-0.441***	-0.128	-0.847**	0.014
	(0.290)	(0.074)	(0.133)	(0.077)	(0.331)	(0.080)
Observations	26,879	20,740	33,402	20,797	26,503	18,867
R^2	0.923	0.948	0.928	0.950	0.931	0.936
F-Stat	5.329	81.955	20.058	64.238	36.555	107.061
$\beta_{Low} - \beta_{High}$	0.585*		0.313**		0.861**	
S.E.	(0.296)		(0.152)		(0.338)	
Firm FE	X	X	X	X	X	X
Year-Border FE	X	X	X	X	X	X

Table 6
Additional Outcomes

Table 6 has regressions with Log(Length) instrumented by the predicted post-reform change in Log(Length) multiplied by the post-reform dummy as main regressor. All regressions include firm and border-year fixed effects. The dependent variable is indicated in each column's header. Log(sales) is the logarithm of total revenues. Log(wage) is the logarithm of the firm's average wage, equal to total labor cost divided by the number of employees. Leverage is total liabilities divided by total assets. Net leverage is total liabilities minus cash, all divided by total assets. Standard errors, in parentheses, are clustered at the pre-reform district level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Dependent Variable:				
	Log(Sales)	Log(Wage)	ROA	Leverage	Net Leverage
	(1)	(2)	(3)	(4)	(5)
Log(Length)	-0.093	0.130**	-0.004	0.014	0.014
	(0.059)	(0.054)	(0.012)	(0.015)	(0.019)
Observations	68,665	68,747	68,124	68,752	68,497
R^2	0.919	0.763	0.717	0.924	0.911
F-Stat	54.871	55.151	55.710	55.123	55.222
Firm FE	X	X	X	X	X
Year-Border FE	X	X	X	X	X

A Appendix

A.1 Details regarding the Data Cleaning Process

As explained in the main text (Section 3.1), the pre-reform allocation of municipalities 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. I then obtain, for each municipality, the list of bordering cities from the National Institute for Statistics (ISTAT) website.[16]

The two lists of municipalities are linked using a fuzzy matching algorithm to account for different spellings of the names.[17] The accuracy of each match is then verified manually. During my sample period, in a few cases groups of small municipalities merged to give rise to larger administrative units. (Since 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 throughout the full sample period. None of these fusions 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

[16] `https://www.istat.it/it/archivio/137333`.

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

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.

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 (*ebtda*) divided by total assets (*toas*)	Amadeus
Length	Estimated length of a civil court proceeding, defined as $(\text{Pending}_{t-1} + \text{Pending}_t)/(\text{Incoming}_t + \text{Resolved}_t)$	Italian Minister of Justice
Δ	Predicted change in Log(Length) due to the reform.	Italian Minister of Justice

Continued on next page

Table A2 – *Continued from previous page*

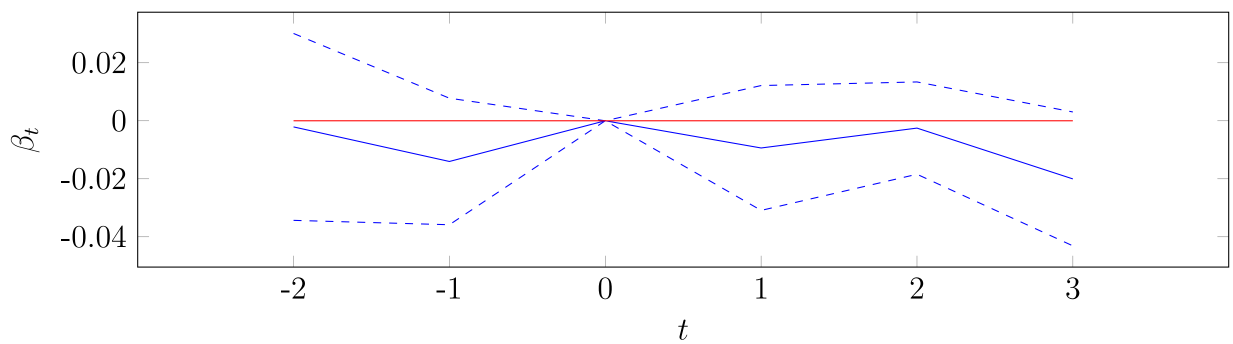
Variable	**Definition**	**Source**
Branches_{2012}	Number of bank branches per 100,000 inhabitants, measured at the province level	Bank of Italy
Average Tier 1 Ratio	Given by: $\frac{\sum_i N_{i,j} \times Tier\,1_i}{\sum_i N_{i,j}}$, where $N_{i,j}$ indicates the number of branches belonging to bank 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

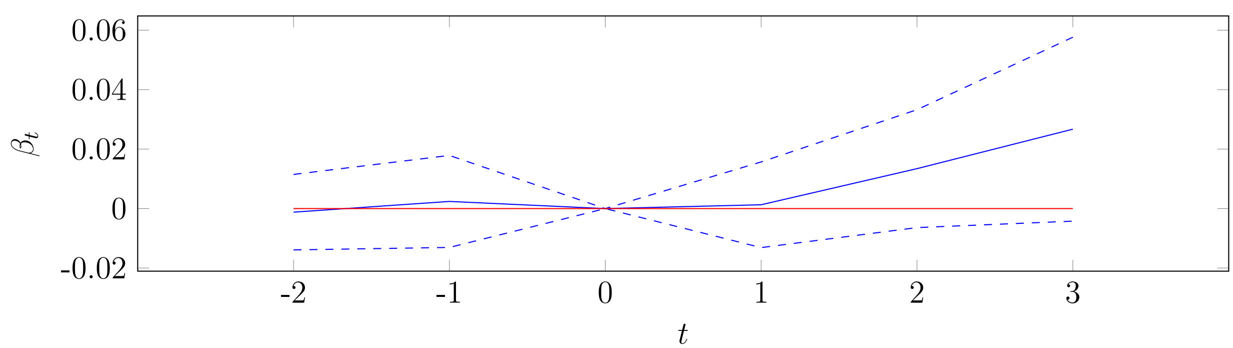
Figure A1
Additional Event Studies

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.

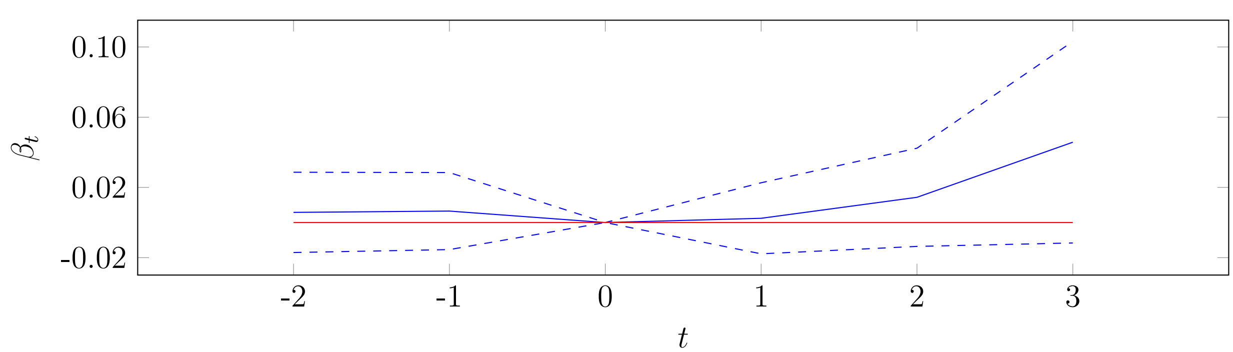
Panel A. Dependent Variable: ROA



Panel B. Dependent Variable: Leverage



Panel C. Dependent Variable: Net Leverage



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