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Do Court Delays Distort Capital Formation?

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DO COURT DELAYS DISTORT CAPITAL FORMATION?*

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Abstract

We study how longer bankruptcy trials affect the size, structure, and allocation of corporate capital. Exploiting a legal reform that reorganised the judicial districts in Italy, we provide three novel insights into the real and allocational effects of legal institutions. First, poor enforcement of financial contracts leads firms to under-allocate capital in intangible assets. Second, it aggravates misallocation by preventing the optimal allocation of physical and intangible assets towards firms with high capital return. Third, in addition to court delays, our findings shed light on the importance of distance from the court as another source of organizational frictions in bankruptcy.

JEL Classification: E22, G33, K40, O16.
Keywords: court enforcement, bankruptcy proceedings, corporate intangible capital, misallocation, corporate debt overhang, distance from the court.

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1. INTRODUCTION

The economic performance of Southern European countries has been at the centre of political and economic debate. Since the mid-1990s, these economies have fallen behind and diverged from the global productivity frontier (Schivardi and Schmitz (2020)). The economic slowdown in this region has often been linked to financial frictions and malfunctioning capital markets (Cette et al. (2016); Gopinath et al. (2017)). Economists and policy makers have often surmised that these frictions might stem from the poor quality of legal institutions that hinder creditor rights in bankruptcy, leading to a lower recovery rate for loans and discouraging lending (La Porta et al. (1997, 1998, 2000, 2008); Kumar et al. (1999); Demirgüç-Kunt and Maksimovic (1998); Djankov et al. (2007, 2008)).

The origins of these frictions partly stem from the adoption of the French civil law in this group of countries which include Italy, Spain, Portugal and Greece. The reason for this is that the civil laws with French origin provide lower levels of legal protection to creditors compared to English common law, the German and Scandinavian civil law (Djankov et al. (2003)). To overcome these issues and improve their performance, Southern European countries have implemented major legal reforms to update their bankruptcy codes[1]. Nevertheless, the recovery rate of loans remained low, due to the lack of efficient enforcement. For instance, the typical bankruptcy procedure takes 7.25 years in Italy whereas lenders expect to be repaid after only 1.5 year in the U.S. [2]. However, the bankruptcy trial length is not constant within countries exhibiting significant variations [3]. This suggests that the organization and bureaucracy of the sub-national legal systems plays an eminent role in defining the efficiency of debt contract enforcement in each region.

In this paper we look into the duration of bankruptcy cases across different Italian judicial districts and how they shape firms' financial decisions

[1]For instance, the 2005–2006 Italian bankruptcy reforms introduced legal outlets, similar to US chapter 11, which made the renegotiation of credit contracts easier. Furthermore, it significantly accelerated firms' liquidation procedures Rodano et al. (2016). New bankruptcy laws were also introduced in Spain in 2004 and Greece in 2007.

[2]Both calculation for U.S. and Italy refer to 2012. Estimate for U.S. comes from Ponticelli and Alencar (2016). The estimate for Italy is based on authors' calculation

[3]See Giacomelli and Menon (2016) for Italy and Fabbri (2010) for Spain

and outcomes. To establish the causal link between court delays and (under- or mis) allocation of capital, we exploit the reorganization of 49 judicial districts in Italy.[4] After the passage of the legal decree n. 155 of 7 September 2012, 26 courts and their districts were suppressed and absorbed by the remaining 23 districts to create a new landscape of judicial areas. Starting from September 2013, firms located in the reformed areas were reassigned to the newly created courts. The average trial length of the new courts was often substantially different from that of their predecessors. The key reason is that the merger happened across districts with very different pre-implementation levels of judicial efficiency. The trial lengths in the new courts were close to the average trial length of the merged districts. The reform thereby drastically increased the trial length in more efficient districts and reduced it in less efficient ones. This heterogeneous impact of the reform provides an almost ideal setting to estimate the causal effect of law enforcement.

To measure the impact of the reform we follow the approach suggested by Pezone (2018). We need to estimate the difference in trial lengths between the new court and that of the merged districts. However, the comparison is not straightforward. Ideally we would prefer to compare the trial length in the new and old districts over the same year. This would guarantee that our comparison is not affected by year specific shocks in trial lengths. Nevertheless, the old courts ceased to exist after 2013 and the new courts were only created afterwards. This implies that we don't have comparable data on trial lengths for the same years both for old and new courts. To overcome this problem, we impute the hypothetical length of proceedings in the newly created district based on the case flow information. We then infer the impact of the reform by comparing the hypothetical length of trial case in the new and old courts, during 2011 and 2012, before the enactment of the reform. Using this estimated impact of the reform to instrument the difference in the length of bankruptcy proceedings before and after the reform, we estimate the elasticity of firms' outcomes to the trial length in bankruptcy cases. Our findings provide three novel insights on the importance of judicial enforcement.

[4]The reform covered 49 out of 165 judicial districts

First, we analyze the effect of court delays on firms spending across various types of assets. During the last two decades, the corporate asset structure has seen a drastic shift from physical to intangible capital in an effort to adjust to a high technological change environment. Hall (2001); Borisova and Brown (2013); Corrado et al. (2005, 2009); Corrado and Hulten (2010); Eisfeldt and Papanikolaou (2013). The intangible investment in R&D activities, workforce training, firm-specific know-how, new software, databases, copyrights, designs, trademarks, organisation, and distribution networks are increasingly becoming key contributors to a firm's ability to catch up with the expanding productivity frontier Demmou et al. (2019). Although intangible assets represent a key feature in the transformation of modern corporations in light of technological changes, almost a quarter of Italian firms in our sample report no investment in intangible capital. Using the aforementioned instrumental variable strategy, we show that the interquartile improvements in judicial efficiency [5] not only increase the stock of capital by 13.3% and raise the capital intensity of production by 11.9% but also lead firms to increase their allocation of capital in intangible assets by 2.3%. The effect is sizable considering that the median share of intangible assets for Italian firms is only 4.1%.

Our second contribution is that we examine whether the local court delays in bankruptcy proceedings disrupt the optimal allocation of capital. Our paper highlights an important channel when the financial frictions caused by court delays prevent better allocation of capital across firms. This is aligned with a set of recent empirical and theoretical studies linking misallocation to financial frictions (Midrigan and Xu (2014); Moll (2014); Gopinath et al. (2017); Larrain and Stumpner (2017); Bau and Matray (2020)). Exploiting the impact of the judicial reform, we find that a reduction in bankruptcy trials reduces capital missallocation by increasing capital relatively more in firms with the highest pre-reform level of marginal return product of capital (MRPK). Furthermore, we find that the inflow of capital towards the high MRPK firms is accompanied by a substantial increase in the share of intangible assets among these firms. This finding sheds light on a new channel that links the rising sectoral dispersion in marginal return of capital to aggregate productivity loss.

[5] According to the average length of bankruptcy proceedings between 2008 and 2012.

The third contribution of the paper is that we study the distance from the court as another source of organizational frictions in bankruptcy. The existing literature mostly focuses on court delays as a main cause of judicial inefficiency. However, regardless of how courts function, factors that limit firms' access to the courts can negatively impact their performance. The reorganization of local courts provides us with a unique opportunity to explore the abrupt and substantial change in firms' proximity to their corresponding court. We show that the distance from the courts increased by 18.3 km on average after the reform. To draw a causal inference, we use an identification strategy that exploits the changes in firms' distance from the court within similar pre-reform judicial districts.

Our identification is motivated by the fact that firms in the same pre-reform judicial districts experienced a similar change in court delays after the reform. However, these firms faced different changes in distance from the courts according to their location in the districts. This allows us to neatly isolate the impact of the change in the distance from courts. Our estimates indicate that a 10 km increase in the distance from the court reduces firms' stock of capital by 1.81% and the share of intangible capital by 0.29% . These impacts are roughly comparable to 10% increase in court delays.[6]

The remainder of this paper proceeds as follows: Section 2 reviews the relevant literature and Section 3 describes our estimation strategy covering the institutional details of the reform, data and empirical estimation. Section 4 presents the results and Section 5 concludes.

2. Law enforcement and financial frictions

The quality of legal institutions has been found to positively correlate with financial development and economic performance across countries (La Porta et al. (1997, 1998, 2000, 2008); Kumar et al. (1999); Giannetti (2003); Lerner and Schoar (2005); Qian and Strahan (2007); BAE and Goyal (2009); Haselmann et al. (2009)). Since judicial quality at the national level is also correlated with other aspects of the institutional environment, isolating the causal impact of the legal institutions in a cross-country setup can often

[6] 10% increase in length of bankruptcy trials reduces the firms' stock of capital by 1.89% and the share of intangible capital by 0.35%.

prove challenging. (Acemoglu (2005); Acemoglu and Johnson (2005); Laeven and Woodruff (2007)).

Motivated by this concern, more recent literature exploits the spatial variation of judicial efficiency within countries to design a more reliable empirical identification and better control for potential omitted variables. This process helps unbundle the national and sub-national effects that mediate the mechanism of judicial quality on firm-level outcomes. In particular, this enables researchers to distinguish between the legal content and law enforcement. While the former is usually common across all regions in a country, the latter can vary significantly due to sub-national organisation, the characteristics and workload of the local courts Laeven and Woodruff (2007); Fabbri (2010).

Building on this rationale, several studies look into regional variations in local court enforcement for several countries including Mexico Laeven and Woodruff (2007), Italy (Jappelli et al. (2005); Giacomelli and Menon (2016); Pezone (2018)), Spain Fabbri (2010) and Brazil (Ponticelli and Alencar (2016)). These studies use these local variations to study the impact of judicial efficiency on firms' employment size (Jappelli et al. (2005); Laeven and Woodruff (2007); Giacomelli and Menon (2016); Ponticelli and Alencar (2016); Pezone (2018)), firms' capital (Fabbri (2010)), firms' investment and capital intensity (Ponticelli and Alencar (2016)) and bank loans (Jappelli et al. (2005); Visaria (2009); Schiantarelli et al. (2020)).

In order to draw a causal link some studies have relied on regional historical differences to instrument the variation in the length of civil proceedings. Laeven and Woodruff (2007); Brown et al. (2016) [7]. Another common approach is a discontinuity design that binds municipalities in pairs that are

[7] Laeven and Woodruff (2007) exploit the systematic correlation between the quality of the legal system and the prevalence of the indigenous population in the year 1900 across different states in Mexico. The key hypothesis is that in those places where the share of the indigenous population was higher, European settlers were more likely to adopt institutions designed to exploit local labour. This had a protracted impact, hindering the quality of institutions where indigenous populations had a more pronounced presence. Brown et al. (2016) exploit the historical institutional differences in court enforcement between American Native reservations. The difference arose when the passage of Public Law 280 in 1958 assigned the subset of reservations to state courts while others remained under tribal courts. Studying the credit market activities between 1997 and 2003, they find that the reservations under state courts, with more efficient court enforcement, perform substantially better.

located across the judicial districts' borders Giacomelli and Menon (2016); Brown et al. (2016, 2017); Ponticelli and Alencar (2016). While these approaches have merit, their outcomes are still susceptible to firm behavior, including sorting their options about locating their headquarters. This in turn suggests that a reverse effect might be in place, as the firms with higher growth potential might locate their headquarters in more efficient judicial districts Pezone (2018).

The common caveat that both of these approaches share is that they rely only on spatial variation that is susceptible to the problems such as firm sorting. Recent examples in the literature overcome this issue by exploiting a natural experiment. Visaria (2009) studies the impact of the staggered introduction of Debt Recovery tribunals across Indian states whose aim was reducing long trial delays. Li and Ponticelli (2020) examine the impact of the staggered introduction of specialized bankruptcy courts in different prefecture-level cities across China. Iverson (2018) and Müller (2020) explore the passage of the Bankruptcy Abuse Prevention and Consumer Protection Act in the United States which decreased the congestion of courts.

In this paper we use the reorganization of Italian court districts as a natural experiment that enables us to exploit both the temporal and the spatial variation in law enforcement. In this regard, our identification is very close to Pezone (2018) that exploits this setting to study the impact of average civil trial length on firms' employment size, while we study the impact of court delays on capital allocation. Nevertheless, what distinguishes our paper's setup from previous literature is that the reorganization of judicial districts allows us to disentangle and compare the effect of distance from the court and change in court delays. In this regard, to the best of our knowledge, our paper is the first to shed light on the impacts of organizational frictions caused by the longer distance from the court. This finding contributes more broadly to the literature that study the effect of distance in social and economic interactions such as investment (Giroud (2013)), innovation (Bell et al. (2019)), health care provision (Avdic (2016)), voting turnout (Cantoni (2020)), and lending (Guiso et al. (2004); Petersen and Rajan (2002); Degryse and Ongena (2005); Agarwal and Hauswald (2010); Gilje et al. (2016); Nguyen (2019)).

One of our main results contributes to the growing literature on the impact of financial friction on firms' under-allocation of capital in intangible assets. Studying the impact of the Great Recession, Duval et al. (2019) show that financial frictions could be an important factor in explaining the low share of firms' intangible assets. In fact, they show that intangible corporate spending is a lot more likely to be affected by financial frictions compared to investments in physical assets. As firms adjust their investment depending on their expected risk and return for various types of assets [8], a weak contract enforcement environment can decrease the perceived return of intangible assets. This is because it is harder to use intangible assets as collateral, owing to the difficulty of external valuation of patents and tacit workforce training and skills. Given the complexity of property right cases, it also becomes even harder to secure a return for intangible assets Claessens and Laeven (2003); Braun (2005). Therefore, weak law enforcement can distort the optimal allocation of spending on intangible assets.

The earlier works by Claessens and Laeven (2003); Demirgüç-Kunt and Maksimovic (1998) provide cross-country evidence that highlights the association between a better legal system and the higher share of spending in intangible assets. However, the cross sectional nature of these works makes it hard to establish the causal link. Using a quasi experimental instrument, our paper is the first to provide the micro level evidence and causal links to support this hypothesis. We find that longer bankruptcy trials have led firms to under-allocate capital in intangible assets.

Furthermore, our work contributes to the literature that links misallocation to the financial frictions. Theoretical frameworks, outlined by Midrigan and Xu (2014); Moll (2014), show that financial frictions can increase misallocation by preventing optimal allocation of inputs towards financially constrained firms. One of the main predictions of these models is that the expansion of external finance improves capital allocation. Studying the episode of financial liberalization, Larrain and Stumpner (2017); Bau and Matray (2020) show that the expansion of capital inflows reduced capital misallocation.[9] Reis (2013)Benigno et al. (2015)Borio et al. (2016) and

[8] See (Matsuyama (2007))

[9] Larrain and Stumpner (2017) examine the impact of capital account liberation in Eastern European countries. Bau and Matray (2020) exploit the staggered introduction of automatic approval of foreign direct investments across Indian industries.

Gopinath et al. (2017) study the dynamics of misallocation in South European countries. They show that the misallocation increased despite substantial capital inflows during the pre-crisis period. They argue that the reason for this is that capital inflows were directed towards less productive firms through low interest rates, lax credit conditions and inefficient financial intermediation. Bai et al. (2018) study the impact of interstate bank deregulation in the United States on allocation of resources. They show the improvements in efficiency of local lending environments led to a reallocation of labor toward more productive small firms.

Our paper adds to this literature by studying the impact of financial frictions, induced by judicial inefficiency, on capital allocation. In particular, we focus on two issues. First, the allocation of capital between intangible and physical assets at firm level. Second, the allocation of capital between high and low return firms at sectoral level. In this regard, our results also contribute to the literature that studies the link between institutional frictions in bankruptcy and reallocation of production factors across firms Bernstein et al. (2019); Ma et al. (2020).

3. Estimation strategy

3.1. Reorganization of court districts in Italy

Italy adopted the French civil law that has several institutional features that are particularly significant to our study. Firstly, the content of these laws over credit relations are the same nationwide, but enforcement varies substantially across local court districts. Secondly, financial disputes are assigned to the local courts on a territorial basis. Thereby, the variation in local court efficiency leads to a variation in financial contract enforceability for local firms (Giacomelli and Menon (2016)).

Figure 1 points to these drastic differences in judicial efficiency at the local court level, ranging between two to more than ten years. Although there is a distinct pattern of improving court enforcement as we move to the north of the country, there are often substantial differences between neighbouring districts. Before 2013, the territorial organization of the Italian judicial system was based on 165 court jurisdiction areas. This territorial distribution of courts was mainly determined by historical factors and largely resembled

the one shaped in 1865, immediately after the unification of Italy. This, in turn, was based on the judicial systems of the previous states.

Until 2013, no existing court had ever been removed (Giacomelli and Menon (2016)). The major reform of the court reorganization occurred on September 7, 2012 and became effective on September 13, 2013. The reform reorganised 49 out of 165 pre-reform judicial tribunes. The reorganization of the courts led to the suppression of 26 courts that merged with the adjacent district of a surviving court.

The main aim of the reform was to reduce costs in the judicial system. Although there was no reduction in the number of employees during the reform, the reorganization led to massive running cost savings in court facilities and buildings. Moreover, the reorganization of the courts was based on some administrative criteria that are not related to the initial efficiency of the courts. For example, the reform suppressed the courts that were not located in provincial capitals. This kept the selection of surpassed courts exogenous in the sense of judicial efficiency. Nevertheless, there were still some exceptions made against the suppression of several southern local courts in districts with a high presence of organised crime (Pezone (2018)).

3.1.1. Impact of the reform

Figure 2 highlights the impact of the reform in the Piemonte region. There were twelve judicial districts before the reorganisation, in 2013. After the reform, seven courts were suppressed and joined with their neighbouring districts. For instance, in the Cuneo province, two districts of Saluzzo and Mondoví were surpassed and joined to their neighbouring district, the provincial capital Cuneo. Alba, another pre-reform district in the province of Cuneo, was also suppressed and absorbed by the neighbouring district Asti in the province of Alessandria.

Although the merged districts were nearby and often located in the same province, they exhibited substantial differences in the length of their bankruptcy proceedings. For instance, the average length of bankruptcy proceedings during 2011 and 2012 before the enactment of the reform, was much shorter in Saluzzo (1,527 days) compared to Cuneo (2,596 days) and Mondoví (2,770 days). However, after September 2013, the firms that were located in these

three districts were assigned to the same new court that was created from their merger.

To study the impact of the reform we need to compare the trial length between the new court and these three pre-reform districts. However, the comparison is not straightforward. Ideally we would like to compare the trial length in the new and old districts in the same year. This would guarantee that our comparison is not affected by year specific shocks in trial lengths. Nevertheless, the old courts existed until 2013 and the new courts were created afterwards. This implies that we do not have comparable data on trial lengths for the same year for old and new courts.

To overcome this problem, we impute the hypothetical length of proceedings in the newly created district during 2011 and 2012. We estimate the trial length in the new court according to the aggregate number of pending, initiated and resolved bankruptcy cases in the three districts during 2011 and 2012. We follow the approach, initially used by Pezone (2018) which will be explained in section 3.1.2.

Using this estimate we can quantify the impact of the reform in these three districts by comparing the hypothetical trial length of the new court with the actual trial length in these districts between the 2011 and 2012.

Our calculation indicates that the hypothetical length of proceedings in the newly created court, would be on average 2,248 days during 2011 and 2012. This number lies in the range between the shortest trial period in Saluzzo (1,527 days) and the longest proceeding length in Mondoví (2,770 days). This suggests that the merger had a very different impact on the firms in these three districts. Our estimation implies that the reorganization of court districts increased the trial length by 49.2% for firms that are located in the district of Saluzzo and reduced trial lengths in Cuneo and Mondoví by 12.9% and 15.1% respectively.

Table 2 presents the hypothetical average trial length for all twenty-three new courts between 2011 and 2012 (before the reform). Comparing these with the actual trial lengths for forty-nine pre-reform districts, we find a similar pattern as we do in our example from the Cuneo districts. The districts that merged with more efficient courts benefited from the reform.

However, the reorganization increased the trial length in districts that merged with courts with relatively longer trial periods.

Furthermore, it is important to note that the reorganization of courts resulted in increasing public concerns and discontent. The complaints were more evident in districts where the post-reform judicial efficiency declined. For instance, in 2017, in a petition to the Minister for Justice, the mayors of the district of Crema complained that their territory was "gravely and unjustly penalized by the closure of the Court of Crema in 2013, which was among the best in the country in terms of efficiency. In another example, the mayor of the city of Bossano del Grappa publicly complained about the suppression of the district and merging to the neighbouring Vicenza court. He wrote in a public letter in 2012, to the prime minister, Minster for Justice and the Home Secretary that the length of civil trials is much longer in the Vicenza court compared to the court of Bossano del Grappa. He stressed that the merger could "gravely penalize a community and an economic area of enormous dimension". [10]

3.1.2. Estimating the reform impact using the hypothetical trial length of new courts before the reform

In this section we explain how we impute the hypothetical pre-reform trial length of new districts that were created after the reform. First, it is worth noting that, in Italy, there is no information on the exact length of bankruptcy proceedings at the court level. Nevertheless, we could estimate them through the available case flow data as follows.

$$D_t = \frac{P_t + P_{t+1}}{E_t + F_t} \times 365 \tag{1}$$

where D_t is trial length to resolve matters regarding the bankruptcy proceedings in year t, P_t are pending cases at the beginning of the year t, P_{t+1} are pending cases at the end of the year t, F_t are new cases filed during year t and E_t are cases ending with a judicial decisions or withdrawn by the parties during the year t.

[10] These two examples come from Pezone (2018). He cites the Crema petition that is available at www.cremaonline.it and the letter form mayor of Bossano del Grappa that is available at www.bassanonet.it

We apply the same formula adopted by the Italian Ministry of Justice and the Italian National Institute of Statistics (Istat) to calculate and report the trial length in different districts. This approach is also widely used in the academic works to calculate the average length of civil proceedings as a sound measure of judicial inefficiency (Jappelli et al. (2005); Giacomelli and Menon (2016); Pezone (2018); Schiantarelli et al. (2020)). Furthermore, using the rarely available data from the actual length of civil proceedings in 2016, (Pezone (2018)) shows that above index is remarkably reliable estimate of the actual proceedings.

Here, we use the same concept to estimate the hypothetical pre-reform duration of court proceedings in the new districts. If the new court would have existed before the reform, its pending, new and solved cases would be the sum of the all pending, new and solved cases in the merged districts. Thus we could calculate the trial length of the new court j at time t that were created by the merger of the district of i ϵ 1,..,N with other N-1 courts at time $T > t$ [11].

$$\hat{D}_{jt}^{New} = \frac{\sum_{i=1}^{N} P_{t_i} + \sum_{i=1}^{N} P_{t+1_i}}{\sum_{i=1}^{N} E_{t_i} + \sum_{i=1}^{N} F_{t_i}} * 365 \tag{2}$$

Then the hypothetical impact of the reform on district i at time $t < T$ can be calculated as follows

$$\Delta_{ijt}^{Reform} = log(\hat{D}_{jt}^{New}) - log(D_{it}^{pre-reform}) \tag{3}$$

Where the $\hat{D}_{jt}^{New}$ and $D_{it}^{pre-reform}$ can, respectively, be driven from equation (2) and (1). We then use Δ_{ijt}^{Reform} to predict the impact of the reform on the length of bankruptcy proceeding in each judicial district. Comparing this prediction with real change in trial length, enables us to isolate the impact of the reform from other changes that caused by macroeconomic conditions and contemporaneous shocks.

3.2. Data and definition of variables

3.2.1. Judicial data

Data on the judicial system comes from the Italian Ministry of Justice and includes information for all Italian civil courts between 2008 and 2017. Pub-

[11] This approach proposed by Pezone (2018) to calculate the impact of the reform

licly available data, between 2014 to 2017, reports only the length of civil proceedings and we don't have access to the case-flow data. However, the data for pending, incoming and resolved cases are available between 2007 and 2013. That makes it possible to calculate the hypothetical length of bankruptcy proceedings for the new courts before the reform in 2013.

Data for civil cases includes the bankruptcy cases as well as labour disputes, property and torts. Fortunately, we have disaggregated data for these subjects. That makes our analysis different from Giacomelli and Menon (2016) and Pezone (2018) that use the average length of civil proceedings due to lack of more detailed data. Following Ponticelli and Alencar (2016), we focus on the length of bankruptcy proceeding to measure the quality of financial contract enforcement. It is important to note that bankruptcy proceedings in our data does not include the voluntary bankruptcy cases that the borrowers file themselves. As voluntary bankruptcy cases tend to be much shorter, this increases the average length of bankruptcy trials that we observe in the data.

To combine our judicial and firm level data, we assign each firm to a judicial jurisdiction. To this end, we use the judicial court where the firm is located. This approach is similar to that used by other works (Giacomelli and Menon (2016); Pezone (2018); Schiantarelli et al. (2020)) to study the real impact of judicial inefficiency in Italian civil courts. This choice is motivated by the fact that post-enforcement, however, this requires several steps. First, lenders need an injunction from the court typically located in the province of its headquarters. Having received an injunction, in order to take possession of the collateral the lender must then adjudicate before the court in the location of the collateral that often coincides with the firm's location Schiantarelli et al. (2020).

3.2.2. Firm level data

Our firm level data comes from AIDA, the subset of ORBIS - Europe, covering both listed and unlisted Italian enterprises provided by Bureau van Dijk. The database includes information on firms' value added, output, capital stock and employment. It also includes data on firms' balance sheets and income statements including debt, asset, capital depreciation and share of tangible vs intangible fixed capital.

We focus on the period of 2008 to 2017, where we have a relatively high availability for the main variable of interest. The number of observations shrinks drastically before 2008 and data are sparse before 2005. In Italy, unlike US, there are strict rules for all firms, including unlisted ones, to disclose their balance sheets to the Italian Chamber of Commerce, this being the main source for AIDA Pezone (2018). As a result, our data has a nationally representative coverage of Italian firms in the period of our study.

Similar to Duval et al. (2019), we study firms in non-farm and non-financial businesses. We follow Kalemli-Ozcan et al. (2015) to clean data from duplicates and inconsistent entries. In the case of multi-plant firms, we keep the headquarters and remove all other plants from the same establishment. We also restrict our sample to the firms that have data entries before 2012 and after 2013. This excludes new firms that entered the market after the reform passed into law in 2012. This could eliminate the potential sorting effect, as the introduction of the reform could have led new entries to strategically locate themselves in districts that expected to gain more from the reform.

3.3. Estimation specification

Equation 4 shows our benchmark model to estimate the impact of financial contract enforcement on firms' outcome.

$$y_{fspit} = \beta log(D_{it}) + \delta_f + \delta_{s,t} + \delta_{p,t} + X_{ft} + \varepsilon_{fspit} \tag{4}$$

Where f, s, p, i and t denote the firm, two digit industry, province, pre-reform judicial districts and year respectively; y_{fspit} measures the performance of firm f of industry s at time t that is located in province p and pre-reform judicial district i. In our estimation y_{fspit} includes firms' outcomes such as the logarithms of the stock of capital, capital per employee, MRPK and share of intangible in fixed capital. The specification contains firm fixed effect δ_f to control for time invariant characteristics of the firms, sector-year fixed effect $\delta_{s,t}$ to control for sectoral shocks and and province-year fixed effects $\delta_{p,t}$ to control for time varying regional characteristics. X_{ft} is a vector of four firm's category-year fixed effects. Each category sorts firms into twenty bins according to one of the following firm's attributes measured in 2012: age; number of employees; average wage and asset.

D_{it} is our variable of interest. It denotes the length of bankruptcy proceeding in days in judicial district i at time t [12]. The trial length has been used widely in the literature to capture the efficiency of contract enforcement[13]. In our specification, we use the logarithm of trial lengths and firms' performance variables; with the exception of the share of tangible assets in fixed capital. This helps us correct the right skewness of our dependent and independent variable. Furthermore, this logarithmic transformation enables us to report our coefficient of interest β in elasticises of firms' outcomes to the bankruptcy trial length.

To obtain an unbiased estimate of β we need to make sure our variable of interest is not correlated with the error term ε_{fspit}. There are many potential confounding factors that could violate this condition. For instance, districts with poor judicial efficiency could have less effective institutions in general, which binds the capacity of local firms and markets.

To address this identification problem, we use exogenous variations in trial lengths across districts caused by the reorganization of local courts in 2013. To this end we use the following first stage IV estimation:

$$log(D_{it}) = \beta' Impact\ of\ the\ reform_i \times Post\text{-}reform_t + \delta_f + \delta_{s,t} + \delta_{p,t} + X_{it} + \epsilon_{fspit} \tag{5}$$

Where *Post-reform* is a dummy that takes one for all in the years after 2012 (t$\geq$2013). *Impact of the reform* is our indicator of reform impact that we construct in section 3.1.2. It estimates the impact of the reorganization on the reformed districts and takes zero for all districts that were not impacted by the reform.

Significantly, since our instrument varies at pre-reform judicial districts, we cluster the error terms at the pre-reform districts in all our estimations. This follows the Bertrand et al. (2004) method to address the potential serial correlation and heteroskedasticity issue.

[12] D_{it} shows the trial length for pre-reform district i before the reform in 2013 and the new district j after 2013. However, since the new districts uniquely is defined by pre-reform districts we drop the index j from our equations

[13] See Fabbri (2010); Ponticelli and Alencar (2016); Giacomelli and Menon (2016); Pezone (2018); Schiantarelli et al. (2020)

3.4. *Validity of the instrument and exclusion conditions*

For our identification strategy to be valid, our instrument needs to capture the exogenous variation in trial length. This implies that our instrument should not be correlated to the error terms ε_{fspit} in equation 4. This exclusion condition can be denoted as follows:

$$cov(\textit{Impact of the reform}_i \times \textit{Post-reform}_t, \varepsilon_{fspit} | X_{fspit}) = 0$$

Where X_{fspit} is the vector of control variables that we used in equation 4. This exclusion condition could be violated under two circumstances. First case is that *Impact of the reform*$_i$ and error terms in equation 4 are correlated. This suggests that the selection of the reformed districts and the way they have been impacted by the reorganization was not random.

To test this possibility, we examine the dynamics of the relationship between firms' outcomes and the *Impact of the reform*$_i$. We trace a year-by-year effect during the 10-year window between 2008 to 2017. We run the following regression that includes the same control variables in equation 4.

$$\begin{aligned} log(y_{fspit}) \quad = \quad & \textit{Impact of the reform}_i \times \sum_{t'=2008}^{2017} \beta_{t'} \times \mathbb{1}(t = t')) \\ & + \delta_f + \delta_{s,t} + \delta_{p,t} + X_{it} + \varepsilon'_{fspit} \end{aligned} \tag{6}$$

We could reject the possibility of correlation between *Impact of the reform*$_i$ and error terms in equation 4 if there is no association between the *Impact of the reform*$_i$ and firms outcomes prior to the reform in 2013. This implies that $\beta_{t'} = 0$ for all $t' < 2013$.

The second case is that *Post-reform*$_t$ and the error terms in equation 4 are correlated. This suggests that 2013, the timing of the reform, was not coincidental. To absorb the general year specific shocks we include the sector-year and province-year fixed effects. However, our results still could be contaminated by other shocks that occurred during the same time span when the reorganisation of the courts was executed. The main concern is that the less efficient courts would respond differently to the other existing shocks. This could bias our results as the predicted impact of the reform depends on the ex-ante quality of the courts.

To reject this hypothesis, we simulate a placebo effect of the reform in districts that are not covered by the reform. We randomly select 23 provinces and simulate hypothetical mergers between neighbouring judicial districts. We calculate the impact of these hypothetical mergers in 2013. We then regress the stock of capital on the impact of this hypothetical reform in our baseline reduced form regression with the same range of control variables. We repeat these placebo simulations with randomly sampled provinces and districts 3000 times.

Finally, it is important to note that our instrument belongs to the family of difference in difference based instruments that have been used recently in studies such as Lu et al. (2017); Pezone (2018); Malgouyres et al. (2019). It is constructed based on the event that impacted some districts while others remained unaffected [14]. Our two exclusion conditions reflect the exogeneity of the timing and impact of the event [15]. These are the two important conditions that need to be met for the validity of this group of instruments.

3.5. Misallocation and financial frictions at the firm level

When firms face idiosyncratic frictions to access external funds, these can have an impact on their return of capital. We follow the common practice in the misallocation literature by examining the differences between the firm's marginal revenue product of capital (MRPK) within similar sectors[16]. First, we briefly explain the conceptual framework behind our reduced form estimation strategy. Then we discuss how we develop it based on our firm level data.

We consider the industry s that is populated by a large number N_s of monopolistic competitive firms. The total output of the industry is given by

[14] However, ours differs from typical difference in difference instruments where the treatment variable is a dummy that takes 1 if the observation belongs to the treatment group. In our case, the treatment variable is continuous, reflecting the heterogeneous impact of the reform

[15] In the normal difference in difference instruments the impact of the reform is binary. Thus the exogeneity of the impact of the event implies random selection to the treatment group

[16] often within sectors classified at two digits industry identification

a CES production function as follows:

$$Y_s = \left[\sum_{i=1}^{N_s} D_{is} (y_{is})^{\frac{\epsilon_s - 1}{\epsilon_s}} \right]^{\frac{\epsilon_s}{1-\epsilon_s}} \tag{7}$$

where y_{is} denotes firm i's real output, D_{is} denotes a demand shift for firm i's variety, and ϵ_s denotes the elasticity of substitution between varieties. We denote by p_{is} the price of variety i and by P_s the price of industry output Y_s. Firms face an isoelastic demand for their output given by

$$y_{is} = (\frac{p_{is}}{P_s})^{-\epsilon_s} (D_{is})^{\epsilon_s} Y_s. \tag{8}$$

This leads all firms in sector s to impose a constant and similar markup $\mu_s = \frac{\epsilon_s}{1-\epsilon_s}$ over their marginal cost when they choose their price, capital, and labor to maximize their profits. Moreover, we consider that firms are exposed to different degrees of frictions τ^k in accessing capital. Denoting the inverse demand function by $p(y_{is})$, depreciation rate by δ_s and real interest rate by r, we find the profit maximization problem of firm i in sector s as follows:

$$max_{p_{is}, k_{is}, l_{is}} \Pi_{is} = p(y_{is}) y_{is} - (1 + \tau^k_{is})(r + \delta_s) k_{is} - w_s l_{is} \tag{9}$$

Assuming a Cobb-Douglas production function with elasticity of output to capital α, we derive the marginal revenue product of capital (MRPK) from first order conditions

$$MRPK_{is} = (\frac{\alpha_s}{\mu_s})(\frac{p_{is} y_{is}}{k_{is}}) = (1 + \tau^k_{is})(r + \delta_s) \tag{10}$$

Equation 10 shows that misallocation can arise when some firms face more severe financial friction (higher τ^k_{is}) than others. This binds their borrowing constraints and leads to their under-capitalization (lower k_{is}). The under-capitalization implies these firms have higher marginal return of capital ($MRPK_{is}$) relative to others. However, the financial fictions prevent the optimal reallocation of capital towards them at the equilibrium.

We hypothesise that the court delays exacerbate the misallocation. Using the reduced form predictions produced by equation 10, we examine this hypothesis. The misallocation increases if the wedges in the cost of external funds τ^k rise more for firms facing higher τ^k initially. This has two impor-

tant effects. First, MRPK increases more for high MRPK firms. Second, the stock of capital declines more in high MRPK firms relative to others. Thus, to examine the link between court delays and misallocation we test whether court delays have differential effects on high MRPK firms.

To estimate firms' MRPK and classify them, we take advantage of equation 10. This shows that $MRPK_{is} \propto \frac{p_{is}y_{is}}{k_{is}}$ across firms operating in the industry s. This implies that $\frac{p_{is}y_{is}}{k_{is}}$ provides us with a within-industry measure for MRPK. This is based on a common assumption in the literature that technological parameters such as elasticity of demand, elasticity of output to factors of production and depreciation rates, are constant across firms in similar industry (Hsieh and Klenow (2009); Restuccia and Rogerson (2008); Gopinath et al. (2017); Larrain and Stumpner (2017); Bau and Matray (2020)).

To measure $\frac{p_{is}y_{is}}{k_{is}}$ based on variables that are available in the AIDA dataset, we closely follow Gopinath et al. (2017) and Larrain and Stumpner (2017). We obtain firm nominal value added, $p_{ist}y_{ist}$, by deducting the real gross output (operating revenue) from the cost of materials. We measure real output, y_{ist}, as nominal value added divided by an output price deflator. Given that we do not observe prices at the firm level, we use gross output price deflators from EU KLEMS at the two-digit industry level. We measure the capital stock, k_{ist}, with the book value of fixed assets and deflate this value with the price of investment goods. In fixed assets we include both tangible and intangible assets. We use a two-digit industry specific deflator based on the good investment prices for Italy. For our price deflators, for both consumption and investment goods, we choose 2010 as our reference year.

We conjecture that high MRPK firms are facing more severe financial frictions compared to other firms that operate in the same sector and province. Our firm-level measure enables us to identify the source of financial frictions that firms face. To better illustrate the relationship between marginal return to capital and financial frictions, we stratify firms into 20 bins based on their average MRPK between 2008 and 2012. Figure 8 plots the median share of external finance (panel (a)) and leverage ratio (panel (b)) in each MRPK bin in 2012. Panel (a) suggests that firms with higher MRPK ,particularly the top quartile, had less access to external resources to finance their

investment[17]. Panel (b) shows the low MRPK firm, in the bottom quartile, were less leveraged in 2012.

4. RESULTS

4.1. *Main Results*

We begin by reporting our baseline results for the impact of court delays in bankruptcy proceedings on firm outcomes. Table 4 reports the estimation for our baseline instrumental variable regression. The first and second stage estimations are respectively reported in panel B and A in table 4. Apart from firm fixed effects all regressions include the sector-year and province-year fixed effects to absorb the sectoral or local specific shocks. We also add the interaction of year dummies with firm categorical variables. Each of them sorts firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and assets. This limits the identifying variation to differences among firms with similar age, size and employment[18]. Furthermore, following (Bertrand et al. (2004)), the standard errors are clustered at the pre-reform judicial districts in order to address the potential serial correlation and heteroskedasticity.

Panel B, indicates that our instrument, *Predicted impact of the reform* $\times$ *Post reform* (*year* $\geq$ 2013), has a positive and statistically significant effect on the *Log*(*Length of proceedings*). This confirms that the reform triggered changes in the length of bankruptcy proceedings in the post reform period and therefore satisfies the relevance condition. The main issue with the relevance condition is the weak instrument problem. To address this concern, we further report the Kleibergen-Paap F-test that has a magnitude of 33 in our main regressions.

With respect to our central research question, panel A indicates that, after being instrumented, the longer length of bankruptcy proceedings cast a negative impact on firms' outcomes. These results are aligned with similar findings in the literature (e.g. Kumar et al. (1999); Laeven and Woodruff

[17]We define the share of external finance as the difference between investment and cash flow as a share of investment. The lower value of the index implies the lower use of external funding.

[18]Table A1, in Appendix I, reports the results without including the interaction of year dummies and these firm categorical variables

(2007); Fabbri (2010); Giacomelli and Menon (2016); Brown et al. (2016); Pezone (2018)).

Column (1) in panel A indicates that a 10% rise in the length of bankruptcy proceedings increases the marginal return production of capital by 1.8%. The size of MRPK used in the literature to capture the size of the firm-specific financial friction (i.e., Larrain and Stumpner (2017)). Our result therefore implies the 10% increase in the length of bankruptcy proceedings increase the firms' marginal cost of raising capital by 1.8%. This result is in line with the estimates of Fabbri (2010) that found 10% increases in the average length of proceedings decreases the cost of bank finance by 3.62%.

The estimate, in column (2) of panel A, shows that the 10% longer bankruptcy proceedings leads firms to reduce their capital stock by, almost the same magnitude, 1.89%. column (3) of panel A, demonstrates that 10% longer bankruptcy trials reduce the capital per employee by 1.59%. This suggests that the weak enforcement of financial contracts could distort the optimal choice of inputs in the production process and thus reduce firm level productivity.

Column (4) of panel A shows the results for the impact of law enforcement on firms' intangible capital stock. Intangible capital includes the corporate spending on R&D and workforce training. The estimate in column (4) indicates that the 10% increase in the length of proceedings reduces the share of intangible capital by 0.35-percentage points. Considering that spending on intangible assets is typically much lower than on physical assets, the estimate is also economically significant. The result suggests that, due to financial frictions caused by court delays, firms under-allocate spending in their intangible assets. This is consistent with the recent findings of Duval et al. (2019) that show financial frictions, caused by the Great Depression, led firms to cut their investment in intangibles more than it did on physical capital. Duval et al. (2019) use the share of debt maturing in 2008 to measure the financial vulnerability of the firms. They found that a 10% increase in debt maturing in 2008 is associated with the 0.58-percentage-point decline in the share of intangible assets when firms are were by the financial crisis.

We further report the reduced form estimation when we regress the firm's stock of capital directly on our instrument along with the same set of con-

trols. In column (5) of panel C our estimate is negative and strongly significant which is consistent with the result in column (2) and further confirms the relevance condition. Column (6) in panel C reports the estimate for an OLS regression. The estimated coefficient is negative, but much smaller, and not statistically significant. This suggests that naive OLS estimation could underestimate the impact of the length of bankruptcy proceedings due to the presence of confounding variables (Lu et al. (2017)).

4.1.1. Court delays and employment

Our results suggest that improved enforcement of financial contracts can enhance firms' access to external finance and lead them to use more capital intensive production processes. Table A3 in Appendix I demonstrates that the higher capital intensity does not crowd labour out. It indicates that shorter trials also have a positive, though small and insignificant effect, on firms' employment.

Column 1 in table A3 shows that the 10% longer trails reduce the employment by 0.25%. Focusing on a subsample of firms that had at least one employee in 2012, we find a larger and more robust estimate which is still statistically insignificant. Figure A1 illustrates that the effect on labour takes place after a time-lag compared with the effect on capital. This suggests that the judicial reform impacted employment through financial channels rather than direct effect on the labour market.

Another potential explanation for the timing of the effect is the major labour reform that occurred in 2015 [19]. To test this hypothesis we run our regression over the subsample of firms that did not have more than 15 employees in 2014 and thereby are not affected by the labour reform. We find the same result confirming that the observed effect is not driven by the labour reform.

[19]The Jobs Act is a labor market reform introduced by March 2015. The reform aimed at enhancing the flexibility of labour contracts, raising the labour flows and increasing the hirings. The reform surpassed the reinstatement clause in the case of unfair dismissals for all new hirings on open ended contracts. As the reinstatement clause was not applied to firms with no more than 15 employees, they were not impacted by the reform. The reform also decreased the judicial discretion. The reason is that severance payments were clarified by well-defined rules depending on tenure. Moreover, the reform provided the possibility of out of court settlement

4.1.2. *Economic magnitude*

To highlight the economic magnitude of this result, we perform a hypothetical experiment. Moving from the areas in the bottom quartile of judicial efficiency to the top quartile, according to the average length of bankruptcy proceedings between 2008 and 2012, decreases the marginal cost of capital by 12.66 % and increases the stock of capital by 13.30% [20]. This also boosts the intensity of capital in firm production process by 11.90 %. Finally, the share of intangible capital will expand by 2.46 percentage-points. While the effect may not seem large, one should bear in mind that many Italian firms have no or a very small stock of intangible capital in their assets. For instance, the bottom quartile and median level of the share of intangible capital in Italy were respectively 0.16% and 5.18% between 2008 and 2012.

4.2. *Robustness checks*

4.2.1. *Dynamic effects and exclusion condition*

Next, we examine the dynamics of the relationship between the changes in the length of bankruptcy proceedings induced by the reform on firms' outcomes. We estimate the elasticity of a firm's capital size and share of intangibles to the predicted impact of the reform for each year in the period of five years before and after the law was enacted. Figure 3 shows these estimated effects. It illustrates that the impact of the reform does not predate the reform. This confirms the validity of our instrumentation strategy to exploit the reform as a natural experiment. This suggests that we could use the impact of the reform as an exogenous source of variation in the length of bankruptcy proceedings to study the impact of financial contract enforcement on the firm's capital size and structure. As a result, this reassures us that our instrument satisfies the exclusion condition.

4.2.2. *Placebo test*

We further conduct placebo tests to assure that our results are not driven by the other shocks that occurred simultaneously with the reorganisation of

[20] The bottom and top quartile of Log(Length of proceedings) between 2008 and 2012 respectively are 7.7129 and 8.4167. This implies that interquartile difference is 0.7038. The impact of interquartile improvement on capital stock in percentage can be obtained by $(8.4167 - 7.7129) \times 0.189 \times 100 = 13.30182$. Where 0.189 is the Elasticity of capital to court delay that we measured in column 2 of table 4.

the courts. The key concern is that the less efficient courts would respond differently to the other existing shocks. This could bias our results as the predicted impact of the reform depends on the ex-ante quality of the courts. To reject this hypothesis, we simulate a placebo effect following a method used by Lu et al. (2017); Pezone (2018); Malgouyres et al. (2019).

We randomly select 23 unreformed provinces and perform hypothetical mergers between neighbouring judicial districts. We then calculate the impact of these hypothetical mergers in 2013 and regress the stock of capital on the impact of this hypothetical reform in our baseline reduced form regression with the same range of control variables. We repeat this placebo exercise for 3000 different random province and district selections.

Figure 4 shows the cumulative distribution function of the estimated coefficients.The distribution of these estimates are centered around zero with the mean value of 0.0041 and standard deviation of 0.0194. The real coefficient lies outside the 99% of the 3000 placebo estimates. These results are very strong and show that our estimates are not driven by other simultaneous shocks unrelated to the reform.

4.2.3. Municipality-Pairs

Every judicial district is composed of several municipalities. In this section we restrict our sample to neighboring municipality pairs that are located across judicial borders. In doing so, we adopt a methodology similar to Ponticelli and Alencar (2016); Giacomelli and Menon (2016); Pezone (2018), which enables us to better capture the potential omitted variables with geographic proximity. We exploit the variation within neighboring municipality-pairs that share their longest border but they are located in different judicial districts within the same province. To this end, we estimate the following equations :

$$y_{fspit} = \beta log(D_{it}) + \delta_{Municipality-Pairs,t} + \delta_f + \delta_{s,t} + X_{ft} + \varepsilon_{fspit} \quad (11)$$

$$log(D_{it}) = \beta' Impact\ of\ the\ reform_i \times Post\text{-}reform_t + \delta_{Municipality-Pairs,t} + \delta_f + \delta_{s,t} + X_{it} + \epsilon_{fspit} \quad (12)$$

Equations 11 and 12 respectively denote the second and first stage of the IV regression. This is similar to our baseline IV regression apart from including

a set of fixed effects for each Municipality-pairs-year. These fixed effects are designated by $\delta_{Municipality-Pairs,t}$. D_{it} is our main variable of interest and denotes the length of bankruptcy proceedings in days. Moreover, f, s, i and t respectively indicate the firm, two digit industry, pre-reform judicial district and year; The δ_f and $\delta_{s,t}$ denote firms and sector-year fixed effect. X_{ft} is a vector of four firm's category-year fixed effects.

Table 5 reports the results of estimating the equations 11 and 12 where the outcomes in the second stage of IV regression are capital stock and share of intangible asset. The estimated coefficients on Log(Length of proceedings) are negative and strongly significant for both outcomes. The estimates indicate that a 10% increase in the length of bankruptcy trial, on average, generates a 1.75% reduction in capital stock and a 0.51 percentage points decrease in share of intangible asset in fixed capital. Furthermore, the magnitude of coefficients are rather similar to our baseline estimates in table 4.

4.2.4. Macroeconomic Shocks

One of the main concerns for our analysis is that the studied period coincides with large macroeconomic shocks such as global financial crises and sovereign debt crises. In particular, economic recovery began more or less in the same period as the legal reform took place. The parallel pre-reform trends and finding no effect in placebo test of non reformed areas are reassuring. This implies that our results are not contaminated by these aggregate shocks. However, in this section we try to alleviate this concern by isolating these shocks and examining the robustness of our results more directly. Following an approach inspired by Gentzkow (2006), we narrow our comparison to the judicial districts that demonstrate the same dynamic after each shock.

We begin with the global financial crisis of 2008. We take the fact that some districts were harder hit by the crisis into consideration. We examine the surge in the number of bankruptcy cases filed in each court immediately before and after the shock. To do so, we sort courts into fifteen bins according to the number of bankruptcy cases that were filed in 2008. We do the same for 2009 after the global financial crisis hit the economy and find the difference between the position of courts in 2008 and 2009. According to these differences, we divide courts into seven categories. We construct

a court level time invariant variable "GFC Shock" that captures these seven categories. It takes 0 if the position of the court does not change between 2008 and 2009. It takes 1 (-1) if the court moves to the next (previous) bin that shows the bankruptcy cases increased (decreased) slightly more relative to other courts. It takes 2 (-2) if the court moves two bins forward (backward). This shows the bankruptcy cases moderately increased (decreased). It takes 3 (-3) if the courts moves more than two bins forward (backward) and indicates a sharp increase (decrease) in the bankruptcy cases compared with other courts. In the same spirit, we split courts to seven categories according to the increase in the number of bankruptcy cases between 2011 and 2012, after the sovereign debt Crisis hit the Italian economy. Variable "SDC shock" captures these categories. We then include the GFC Shock-year and SDC shock-year fixed effects in our regressions. This allows for flexibility and control over the different time-path of courts after the shocks. That in turn enables us to narrow our comparison to similar courts in terms of how hard they were hit by these macroeconomic shocks.

Table 6 reports the results. The estimated coefficients on Log(Length of proceedings) remains negative and strongly significant for both capital stock and share of intangible asset. The 10% increase in length of bankruptcy trials reduces capital stock by 2.26% after controlling for the global financial crisis shock, 1.45% after controlling for the sovereign debt crisis shock and, 1.8% when we control for both shocks. Moreover, column 4 to 6 of table 6 shows that the 10% increase in trail length generates a decline in share of intangible capital between 0.36 and 0.40 percentage point. The estimates reported in table 6 indicate that controlling for macroeconomic shocks does not change our main results significantly. These results imply that our findings are not driven by concurrent aggregate shocks.

4.3. Economic channel

Next we examine the mechanism that moderates the estimated effects. We test whether the contract enforcement affects firms' capital formation through the credit channel. Following the standard approach in the literature, we examine whether the impact of law enforcement was different for sectors and firms that are more financially vulnerable. At the sectoral level we construct financial vulnerability according to the external finance dependence and asset intangibility. These two factors make the performance of some sectors

more sensitive to financial development and access to credit (Rajan and Zingales (1998); Braun (2005); Almeida and Campello (2007); Manova (2013)). At the firm level, we focus on the role of debt overhang and ex-ante balance sheet vulnerability. Lamont (1995); Hennessy (2004); Kalemli-Ozcan et al. (2018); Duval et al. (2019).

4.3.1. Sectoral financial vulnerability

Following the approach in Rajan and Zingales (1998), we construct a sectoral proxy for external finance dependence. The index measures the difference between investment and cash flow as a share of the investment. In that sense, the larger value of the index shows the higher dependence on external funding. Since capital expenditures are not directly reported in our data (Aida), we follow the method used by Acharya et al. (2018) and Pezone (2018) to calculate investment. To this end, we compute investment as fixed capital minus lagged fixed capital plus depreciation. We set all negative values to zero. Since investment appears in the denominator, the proxy is calculated only for firms with strictly positive levels of investment. We calculate the average of this index for each firm between 2008 and 2012. Using the average index of each firm, we find the median of each two digit industries. Then, we use this sectoral index to divide sectors into two groups. The sectors that are more dependent on external finance have a lower index than median. Similarly, they depend on internal finance if their index is higher than the median.

Column (1), in panel A of table 7, shows that the length of bankruptcy proceedings does not have any impact on the firms that depend more on internal finance. The estimated coefficient is very small. However, firms are very sensitive to the length of proceedings when they operate in sectors that are dependent on external finance. The estimate suggest that a 10% increase in the length of bankruptcy proceedings led these firms to reduce their stock of capital by 2.82% more. The magnitude is larger compared to our baseline estimation of 1.89%. However, column (1), panel B of table 7, shows that there is no statistically significant difference between sectors, in the elasticity of the share of intangible capital.

In a cross-country study, Claessens and Laeven (2003) show that there is positive association between law enforcement for property rights and

growth. This association is stronger in sectors with a higher share of intangible assets. This implies that intangible intensive sectors should be more vulnerable to weak law enforcement. Nevertheless, some of the recent firm-level empirical evidence shows that improvements in law enforcement tend to benefit more the tangible intensive sectors (Braun (2005); Berkowitz et al. (2015); Pezone (2018)).

One explanation for these contradictory results lies in the fact that intangible intensive sectors might rely more on internal rather than external funds. Thus, in this paper, we study the spread of the law enforcement effect between sectors, that depend on internal and external funding. Then we compare the size of this spread in the subsample of tangible and intangible intensive sectors. Similar to the external funding index, we calculate the average share of intangible capital for each firm between 2008 and 2012. Using the average index of each firm, we compute the median of each two-digit industry. We then use this sectoral index to divide sectors into two groups. The intangible intensive sectors that have a share of intangible capital above the sample median value and intangible sectors that have a share of intangible capital below the median.

Figure 5, panels A, B and C show that the quality of law enforcement has no effect on size of capital in sectors with internal funding across tangible and intangible intensive sectors. However, the weak financial contract enforcement exhibits a strong negative impact on externally funded sectors. This gap, between internally and externally funded sectors, is more substantial and statistically significant amongst intangible intensive sectors than in tangible intensive sectors. Columns (2) and (3) of panel A, in table 7 show that a 10% increase in the length of proceedings led firms to reduce the size of their capital by 1.43 % , which is not statistically significant, in the subsample of tangible intensive sectors. However, a 10% increase in length of proceedings generates a sharp and significant decline in firm capital by 5.32% among intangible intensive sectors. Nevertheless, the results in panel B of table 7 indicate that the impact of court delays on the share of intangible capital is the same across sectors with high and low dependence on external finance. Furthermore, column 2 and 3, show a similar pattern across the sub-samples of tangible and intangible intensive sectors.

4.3.2. *Corporate debt overhang*

The high levels of corporate debt can distort a firm's incentive to invest since it can reduce the profitability of the investment for shareholders (Myers (1977)) and make future financing more costly (Almeida and Campello (2007)). This implies that firms with high levels of debt could face an underinvestment problem that impedes their future growth (Lang et al. (1996)). This situation is often referred to as debt overhang in the corporate finance literature first coined by (Myers (1977)). However, the extent of debt overhang is more severe when cyclical or institutional factors worsen the credit market conditions (Lamont (1995); Occhino and Pescatori (2015); Chen and Manso (2017); Kalemli-Ozcan et al. (2018)). In this section, we examine whether court delays can distort capital formation through intensifying the debt overhang problem.

Following Kalemli-Ozcan et al. (2018), we construct the index for the debt overhang using the ratio of total debt to total assets as a proxy for firm indebtedness. We measure total debt as the sum of long term debt and current liabilities. Then we divide firms in tertiles according to their level of debt overhang in 2012 right before the reform occurred. Following the common practice in this literature, we classify bottom, second and top tertiles as the low, medium and highly leveraged firms.

Columns 1 to 3 in table 8 show that the relationship between court enforcement and capital size is much stronger among highly leveraged firms. The impact of court delays more than doubles when we move from the subsample of firms with low to high leverage. Column 4 indicates that the difference between the estimated coefficients, -0.179 is statistically significant at 5%. Columns 5 to 7 point to a similar pattern in the impact of court delays on capital structure. The coefficient is six times larger for the sub-sample of firms with high leverage compared to the low leveraged firms. Column 8 reports the difference between the estimated coefficient in the sub-sample of firms in the top and bottom tertiles. The difference is substantial and significant at 5%. Figure 7 visualizes the results of table 8. It shows that corporate investment and capital formation in highly leveraged firms is much more sensitive to court delays.

4.4. *Court delays and misallocation*

To measure the presence and magnitude of misallocation, the literature often relies on the observed dispersion of marginal product of capital within sectors. However, this approach brings about two empirical challenges (Bau and Matray (2020)). First, it is prone to measurement error as it attributes all changes in sectoral dispersion to misallocation [21]. Second, it is hard to estimate the causal effect of specific sources of misallocation from aggregate sectoral dispersion. Furthermore, it prevents us from examining whether the misallocation is generated by sub-optimal capital allocation to low capital return firms or under-allocation of capital towards firms with high capital return.

Following Bau and Matray (2020), we use a firm-level analysis to study misallocation. We examine whether the shorter bankruptcy trial reduces misallocation and helps distribute physical and intangible assets towards firms with higher returns to capital. To this end, we study the heterogeneous impact of the reform on firms with different pre-reform levels of MRPK within the same sectors and local credit markets. First we calculate the average level of MRPK between 2008 and 2012. Based on this average, we divide firms within each 2-digit sectors and provinces into three groups; upper quartile, interquartile range and lower quartile. We classify these groups as high, medium and low MRPK firms. Table 9 reports the estimates for the impact of court delays across these three groups.

Columns 1 to 3 in table 9 show that the relationship between court delays and MRPK is much stronger among firms in the upper quartile of pre-reform MRPK distribution. Moving from the top to the bottom quartile, the impact of court delays quadruples. Column 4 indicates that this differential is substantial and statistically significant. A 10% increase in court delays raises the MRPK by 2.84 % among firms in the top MRPK quartile compared to those in the bottom quartile. This illustrates that court delays increase dispersion in the return of capital within the same sectors and provinces. This suggests that court delays exacerbate misallocation at the local credit market.

[21] This can create upward bias in measuring of the misallocation. See Bau and Matray (2020) for a detailed review of different sources of this upward bias

The estimates in columns 5 to 8, show the effect of court delays on firm's capital stock. The results indicate that firms with ex ante high MRPK experience the strongest effect. This confirms that the link between court delays and higher dispersion of MRPK is moderated by preventing the allocation of capital towards firms with ex ante high MRPK. Columns 9 to 12 report the differential impact of court delays on the share of intangible capital. We find that the impact of court delays is more than ten times stronger on the subsample of high MRPK firms. The results show that a shorter bankruptcy trial not only reallocates the physical capital but also disproportionately shifts intangible assets towards high MRPK firms. This implies that the distortionary impact of legislative delays on the share of intangible capital is biased against high MRPK firms that face more severe financial frictions. These results shed light on a novel and important channel that can explain the link between sectoral MRPK dispersion and productivity.

4.5. The reform and the distance from the court

In this section, we study another aspect of the reform. The legal reform surpassed the 26 districts and assigned firms to the new courts. This caused an exogenous change in the proximity between firms and the courts in these areas. Table A5, in Appendix I, shows that the distance from the court increased by 18.3 km on average after the reform. Table A5 also highlights the heterogeneous impact of the reform on the firms. The distance has increased by 11.3 km more for firms on the top quartile of the change in distance, compared with those in the bottom quartile (averaging 15.36km for the bottom quartile and 26.65km for the top quartile).

It is important to note that firms in the same pre-reform judicial districts experienced a similar change in court delays after the reform. However, these firms faced different changes in distance from the court according to their location in the districts. This allows us to neatly isolate the impact of the reform caused by the change in proximity to the courts. We estimate the following equation using the sample of firms that are located in surpassed districts and thus experienced the non-zero change of distance from the courts:

$$y_{fspit} = \beta\, Change\ in\ distance\ from\ court_f \times Post\text{-}reform_t + \delta_{i,t} + \delta_f + \delta_{s,t} + X_{it} + \epsilon_{fspit} \tag{13}$$

In equation 13, the main variable of interest is the change in distance from the court in kilometers. Where, f, s, i, and t respectively indicate the firm, two-digit industry, pre-reform judicial district, and year; The δ_f, $\delta_{i,t}$ and $\delta_{s,t}$ denote firm, pre-reform district-year, and sector-year fixed effects. X_{ft} is a vector of four firm's category-year fixed effects. Each category sorts firms into twenty bins according to one of the following firm's attributes measured in 2012: age; number of employees; average wage and asset. Moreover, the standard errors are clustered at pre-reform judicial districts.

Figure A2, in Appendix I, shows that there is a negative relation between distance from the court and the change in distance after the reform. This implies that remote places might be impacted differently by the reform compared to urban places near the courts. This is concerning, since our results might be partially driven by the structural differences between urban and rural areas. To address this concern we also add the interaction between year fixed effects and the firm's proximity to the court before the reform.

Our estimates in table 10 indicate that longer distances from the court have a significant negative effect on firm's outcomes. A 10 km increase in the firm's distance reduces its stock of capital by 1.81% and shrinks its share of intangible capital by 0.29%. Furthermore, it increases the firm's marginal return product of capital (MRPK) by 1.68%. This suggests that a longer distance from the court raises the financial frictions and the cost of external finance.

5. Conclusion

In this paper, we look into the causal link between judicial organizational frictions in bankruptcy and firm's under-allocation of capital. We show that court delays in bankruptcy trials increases financial frictions which is indicated by the changes in firms' marginal return productivity of capital (MRPK). This affects both the size and the structure of firms' capital, limiting firms' ability to accumulate capital and under-allocate resources to riskier forms of capital such as intangible assets. We show that longer bankruptcy trials also increase the sectoral capital misallocation by exacerbating existing financial frictions. Moreover, we highlight the importance of distance from the court as an additional source of organizational frictions in bankruptcy.

Our findings has important policy implications. It highlights the channels through which financial frictions in South European countries affect firms' input allocation. The key source of financial frictions is related to the low recovery rate of secured loans in this region which in turn discourages lending. To address these implications, governments across the region have implemented several legal reforms to limit the extend of these frictions through nation-wide legislation aimed to improve creditor's rights in cases of bankruptcy. These interventions had limited impact, as the recovery rate remained low. The key contribution of this paper is to highlight the significant impact of local civil court inefficiencies for judicial enforcement. As policy-makers often suggest that the economic slowdown in southern Europe stems from the poor institutional quality of contract enforcement, we aim to provide robust empirical evidence and a clear mechanism to explain this conjecture.

Figure 1

Average length of bankruptcy proceedings across Italian civil courts

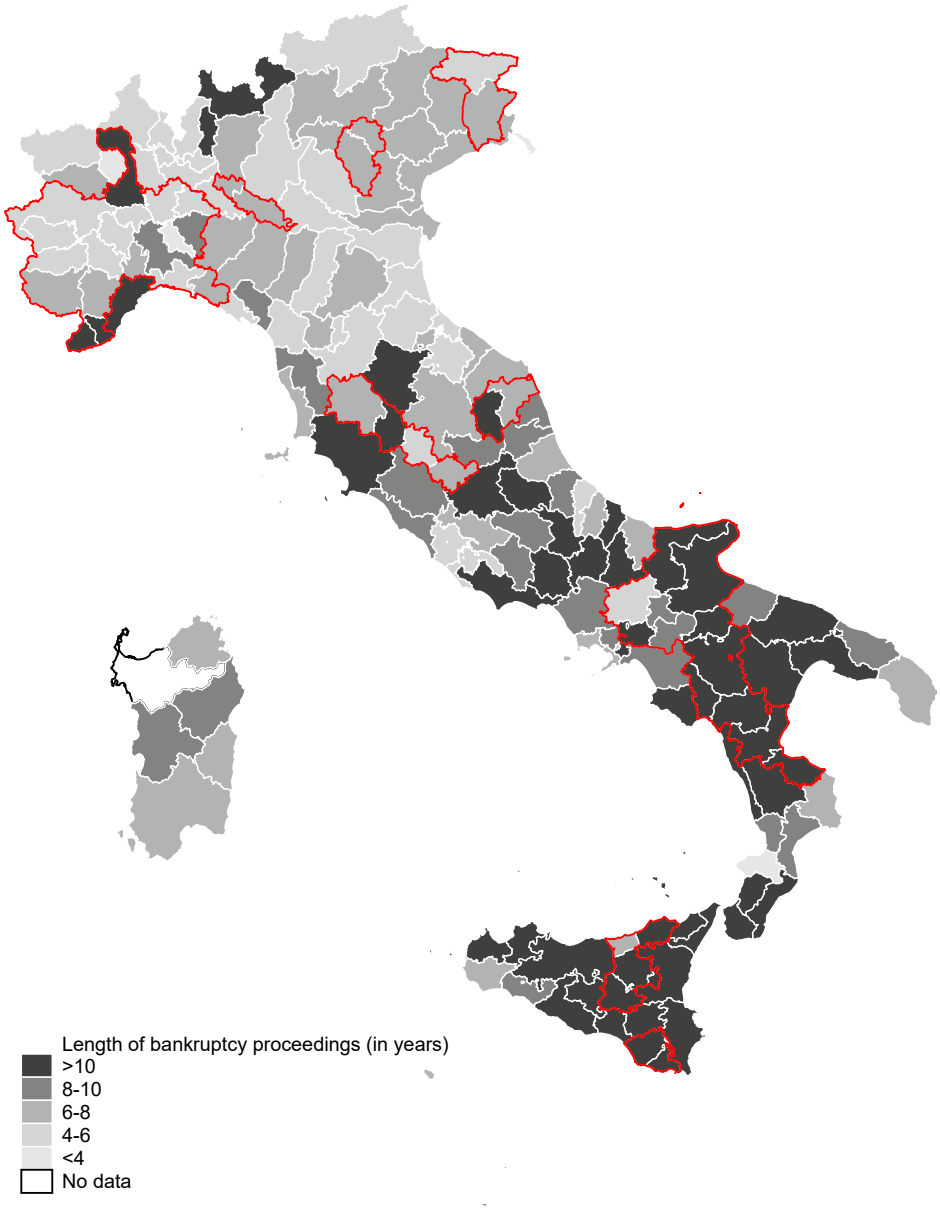


Figure indicates the average length of proceedings (in years) during the 2011 and 2012 before the enactment of the reform in 2013. The red line indicates the area covered by the reform.

Figure 2

Impact of the reform in the northern region of Piemonte

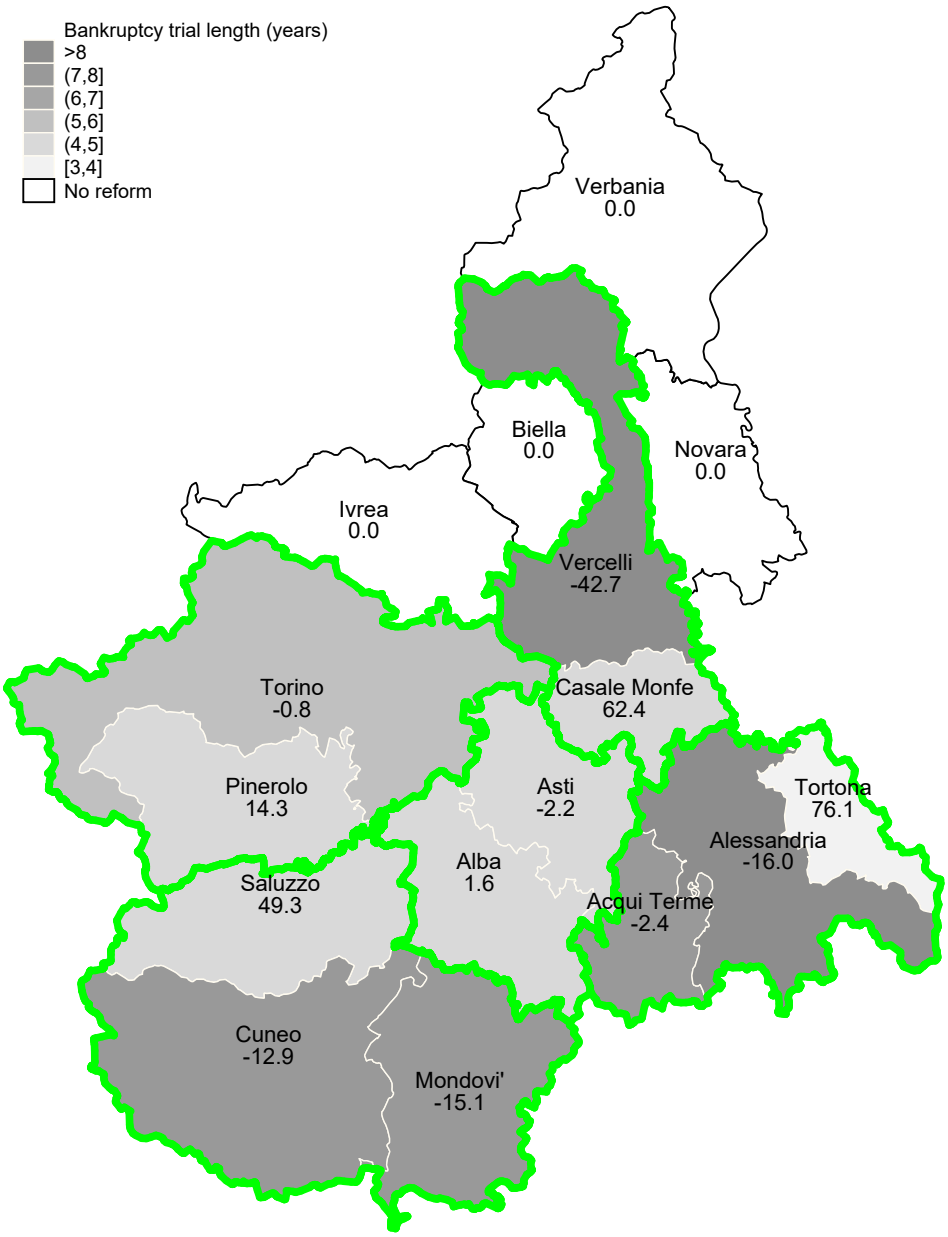


Figure shows the impact of the reform in the northern region of Piemonte. The variation in greyscale indicates the average length of bankruptcy proceedings between 2011 and 2012 before enactment of the reform. The thick green line indicates the border of the new judicial districts generated by the merging of smaller districts. The lines within the encircled green zones indicate the border of judicial districts before the reorganization. The number, below the name of each prereform judicial districts, indicates the predicted percent change in length of bankruptcy proceedings due to merging.

Figure 3

Response to the court delays induced by the reform

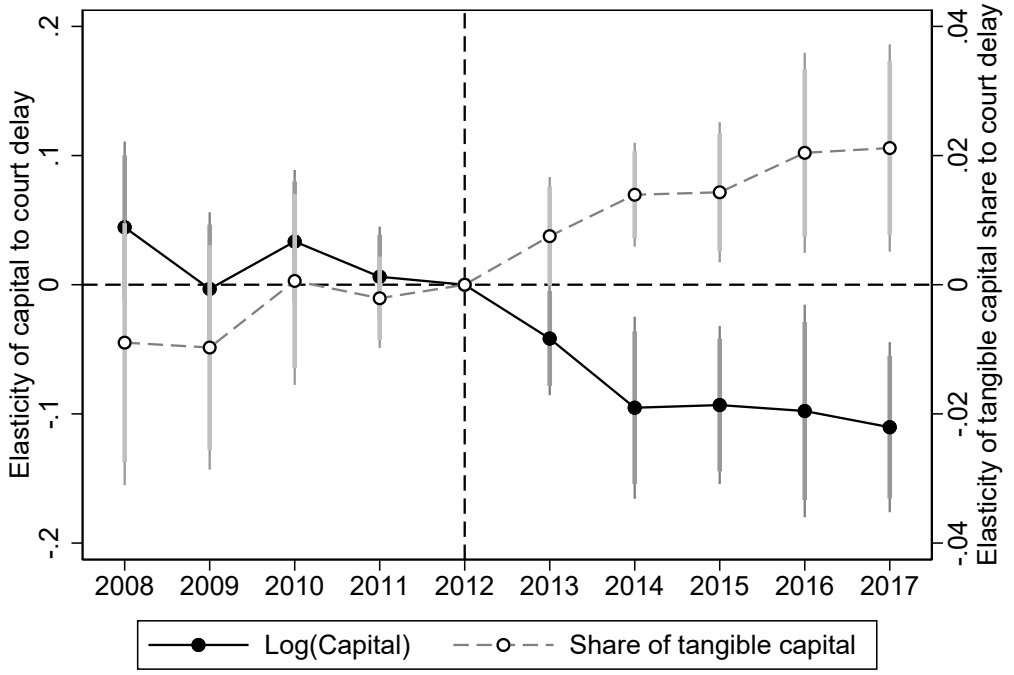


Figure shows that the court delays induced by the reform reduce the size of firms' capital and lead them to under-allocate capital in their intangible assets. We use the impact of the reform to instrument trial length in our benchmark IV regression. The impact of the reform measures the difference between actual trial length of reformed districts and hypothetical trial length of new districts between 2011 and 2012, before the enactment of the reform. The year-by-year court delay elasticity is captured by the coefficients $\beta_{t'}$ in following reduced form regression

$$log(y_{fspit}) = Impact\ of\ the\ reform_i \times \sum_{t'=2008}^{2017} \beta_{t'} \times \mathbb{1}(t = t')) + \delta_f + \delta_{s,t} + \delta_{p,t} + \varepsilon'_{fspit}$$

Where f, s, p, i and t denote the firm, two digit industry, province, pre-reform judicial districts and year respectively and y refers to firm's capital size and share of tangible capital.

Figure 4

Placebo test

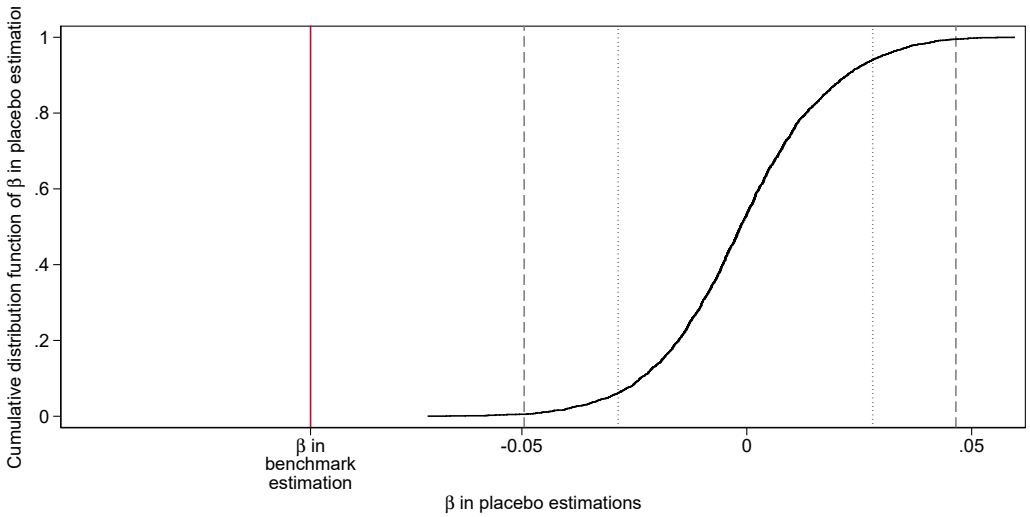


Figure plots the empirical cumulative distribution function of estimated placebo coefficients from 3000 regressions. Each time, we simulate a placebo reform in 2013 that creates 23 new courts out of the merger between neighbouring districts that are randomly selected. The draws exclude areas that were covered by the reform. The solid red line corresponds to the actual estimate for the elasticity of the capital in the reduced form specification in table 4 column (5). It lies outside of the 95% and 99% confidence interval that are delineated respectively by the doted and dashed lines in gray.

Figure 5: Response to Court delays: Internal vs external finance dependence

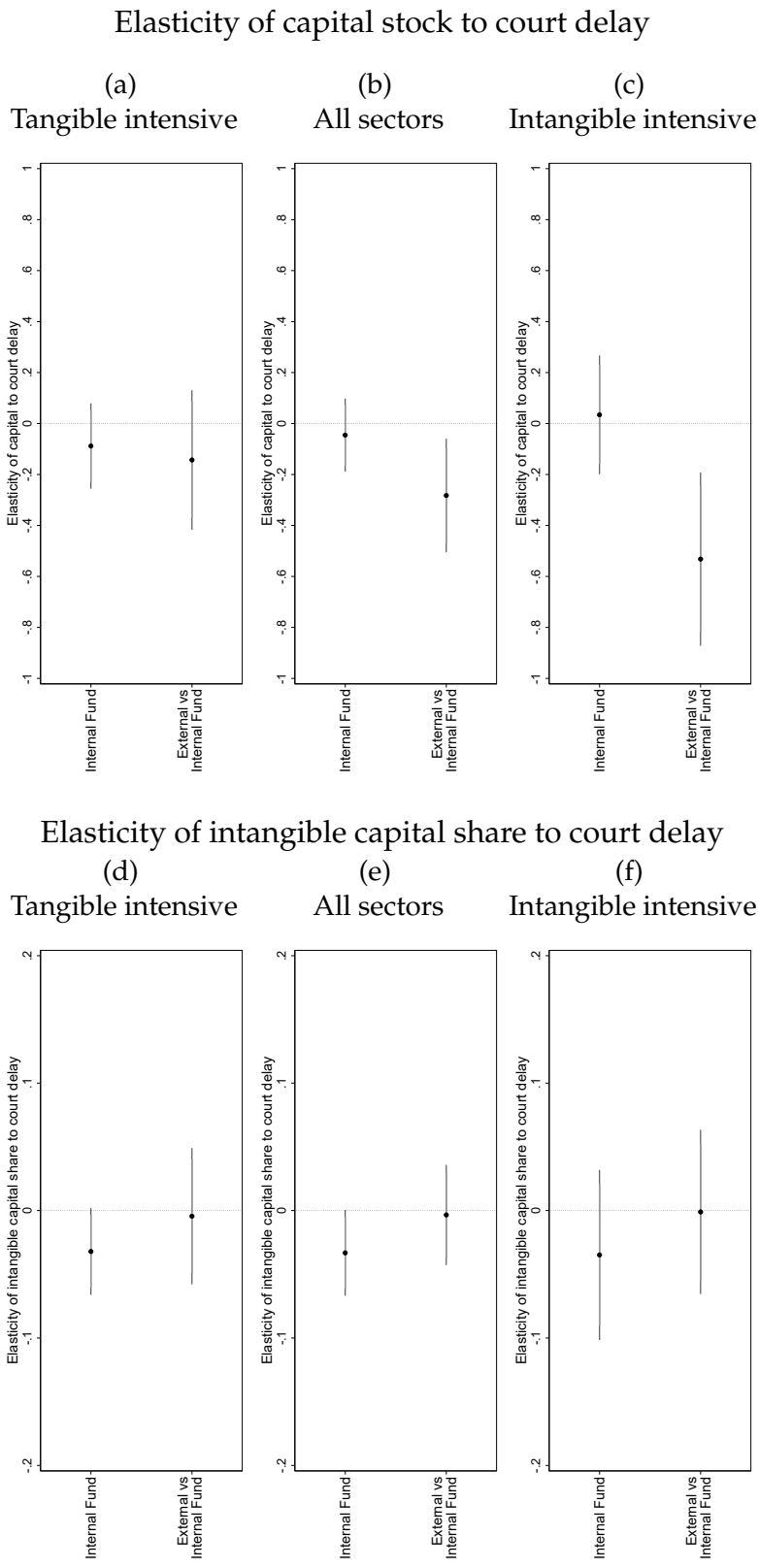
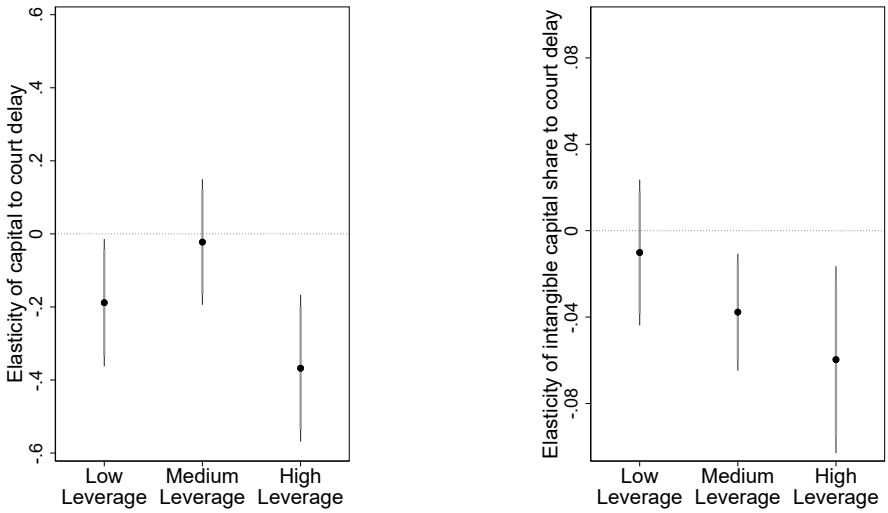


Figure depicts the differential impact of court delays on sectors that depend on internal finance(sectors that has lower than median dependency on external finance) and external finance (above median dependency on external finance). It depicts he estimates for three different samples: All sectors, sectors with low share of intangible capital and sectors with high share of intangible capital. The figure visualize the results of the table 7.

Figure 6

Court delays and firm's debt overhang



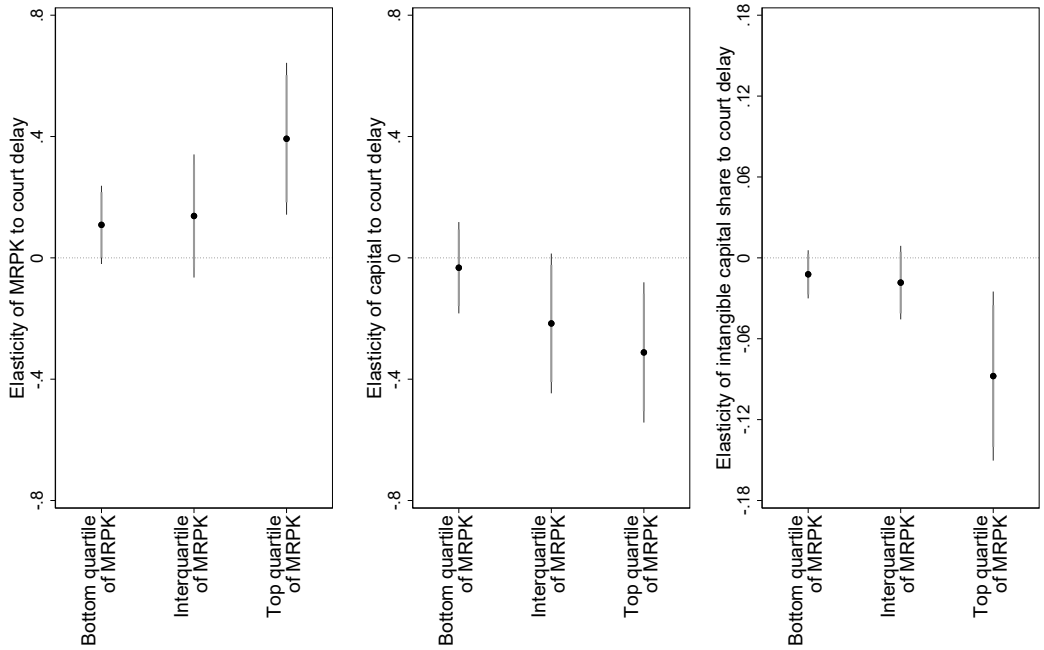
(a) Log(Capital)

(b) Share of intangible capital

Figure depicts the differential impact of court delays across distribution of leverage ratio . It depicts he estimates for subsamples of firms in top middle and bottom tertiles of leverage ratio in 2012 before the reform took place. The figure visualize the results of the table 8.

Figure 7

Court delays and misallocation



(a) Log(MRPK)

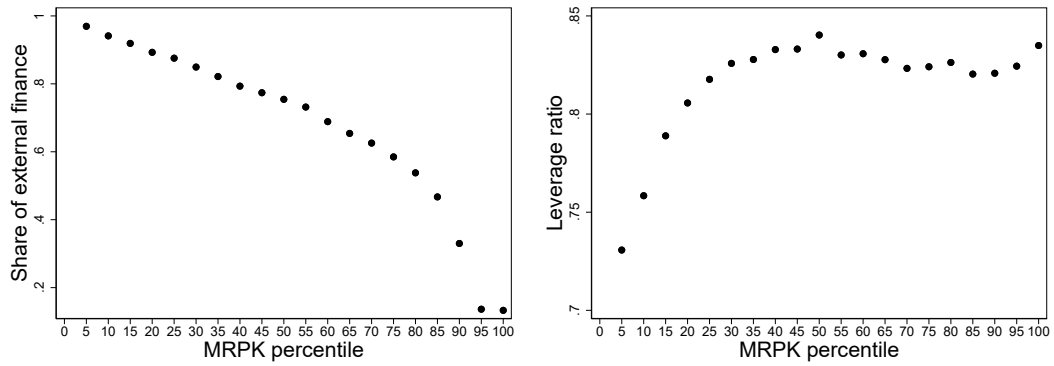
(b) Log(Capital)

(c) Share of intangible capital

Figure depicts the differential impact of court delays across distribution of prereform MRPK. It depicts he estimates for subsamples of firms in top quartile, interquartile range and bottom quartile of MRPK between 2008 and 2012 within each province and 2 digit industry classification. The figure visualize the results of the table 9.

Figure 8

MRPK and source of financial frictions



(a) Access to external finance

(b) Leverage ratio

Figure indicates the median share of external finance (panel a) and leverage ratio (panel b) in 2012 for each MRPK bin. We sort firms into twenty bins according to their average MRPK between 2008 and 2012.

Table 1

Summary statistics.

	Obs	Mean	Median	Std.Dev.	Min	Max
Panel A : ***Firm level variables***						
Log(MRPK)	2072767	6.087	6.066	1.716	1.822	10.581
Log(Capital)	2200169	7.149	7.092	2.294	1.611	12.893
Log(Capital Intensity)	2200169	5.324	5.299	1.961	0.275	10.697
Share of tangible capital	2200492	0.187	0.041	0.271	0.000	1.000
Asset (€ million)	2200492	4.406	0.844	12.899	0.034	98.871
Age	2200485	17.841	14.000	13.168	1.000	162.000
Employment	2200492	15.942	6.000	35.665	1.000	264.000
Wage (€ thousands)	2200492	31.576	29.401	18.889	0.257	113.534
Leverage	2200492	0.756	0.801	0.269	0.098	1.835
Panel B : ***Court level variables***						
Log(Length of proceedings)	2190750	7.814	7.735	0.377	4.875	10.728
Log(Length of proceedings) in reformed area	312541	7.812	7.701	0.402	6.553	9.575
Log(Length of proceedings) in non-reformed area	1878209	7.814	7.739	0.373	4.875	10.728
Reformed	2190894	0.143	0.000	0.350	0.000	1.000
Predicted impact of the reform $\times Postreform(year \geq 2013)$	2190894	0.000	0.000	0.043	-0.696	1.215

Note: log(MRPK) is the logarithm of marginal return product of capital, log(Capital) is the logarithm of capital stock deflated at 2010 prices of two digit industry, Log(Capital Intensity) is the capital per employee. Wage is the total cost of labour divided by the number of employees. "Log(Length of proceedings) "is a continuous variable that estimates the logarithm of bankruptcy proceedings in days. Reformed is a dummy variable that takes 1 if firm located in the reformed judicial districts and zero otherwise.Finally, "Impact of the reform" estimates the change in bankruptcy trial length that caused by the reform t. It is calculated as follows

$$\begin{aligned} \text{Impact of the reform} &= log(\text{Length of proceedings})^{2012,2011}_{\text{Old district(Actual)}} \\ &- log(\text{Length of proceedings})^{2012,2011}_{\text{New district(Hypothetical)}} \end{aligned}$$

Note that Log(MRPK), Log(Capital), Log(Capital Intensity), Asset, Employment, Wage and Leverage. are winsorized at the 1st and 99th percentile.

Table 2

The reformed judicial districts: Actual length of proceedings in preformed courts and hypothetical length of proceedings in new courts.

Region	Province	Pre reform judicial district	Pos treform judicial district	2011-2012 Actual Prereform courts	2011-2012 Hypothetical New courts
		Nord Ovest			
Liguria	Genova	Chiavari	Genova	2159	1825
Liguria	Genova	Genova	Genova	1787	1825
Liguria	Imperia	Imperia	Imperia	5707	4986
Liguria	Imperia	Sanremo	Imperia	4782	4986
Lombardia	Cremona	Crema	Cremona	2138	2555
Lombardia	Cremona	Cremona	Cremona	2767	2555
Lombardia	Pavia	Pavia	Pavia	2096	2388
Lombardia	Pavia	Vigevano	Pavia	2132	2388
Lombardia	Pavia	Voghera	Pavia	3486	2388
Piemonte	ssandria	Acqui Terme	Alessandria	2476	2417
Piemonte	ssandria	Alessandria	Alessandria	2877	2417
Piemonte	ssandria	Tortona	Alessandria	1373	2417
Piemonte	Asti	Asti	Asti	1802	1763
Piemonte	Cuneo	Alba	Asti	1736	1763
Piemonte	Cuneo	Cuneo	Cuneo	2581	2247
Piemonte	Cuneo	Mondovi'	Cuneo	2646	2247
Piemonte	Cuneo	Saluzzo	Cuneo	1506	2247
Piemonte	Torino	Pinerolo	Torino	1641	1876
Piemonte	Torino	Torino	Torino	1892	1876
Piemonte	ssandria	Casale Monferrato	Vercelli	1732	2813
Piemonte	Vercelli	Vercelli	Vercelli	4906	2813
		Nord Est			
Friuli-Venezia Giulia	Udine	Tolmezzo	Udine	1734	2167
Friuli-Venezia Giulia	Udine	Udine	Udine	2226	2167
Veneto	Vicenza	Bassano del Grappa	Vicenza	2371	2774
Veneto	Vicenza	Vicenza	Vicenza	2901	2774
		Centro			
Marche	Macerata	Camerino	Macerata	4628	2688
Marche	Macerata	Macerata	Macerata	2498	2688
Toscana	Siena	Montepulciano	Siena	4502	3198
Toscana	Siena	Siena	Siena	2852	3198
Umbria	Terni	Orvieto	Terni	1806	2347
Umbria	Terni	Terni	Terni	2430	2347
		Sud			
Basilicata	Potenza	Lagonegro	Lagonegro	6448	4398
Basilicata	Potenza	Melfi	Potenza	6094	6984
Basilicata	Potenza	Potenza	Potenza	7325	6984
Campania	Avellino	Avellino	Avellino	5113	4745
Campania	Avellino	Sant'Angelo dei Lombardi	Avellino	3145	4745
Campania	Avellino	Ariano Irpino	Benevento	3206	2364
Campania	enevento	Benevento	Benevento	1959	2364
Campania	Salerno	Sala Consilina	Lagonegro	3692	4398
Calabria	Cosenza	Castrovillari	Castrovillari	5916	7550
Calabria	Cosenza	Rossano	Castrovillari	9678	7550
Puglia	Foggia	Foggia	Foggia	3638	3771
Puglia	Foggia	Lucera	Foggia	4205	3771
		Insular			
Sicilia	Enna	Enna	Enna	3784	4498
Sicilia	Enna	Nicosia	Enna	6892	4498
Sicilia	Messina	Patti	Patti	8900	7821
Sicilia	Ragusa	Modica	Ragusa	8578	4670
Sicilia	Ragusa	Ragusa	Ragusa	3871	4670

Table 3

The pre-reform balance

	Judicial districts that gained from the reform			Judicial districts that lost from the reform			
	Obs	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Diff
Log(MRPK)	48013	5.98	1.61	86670	5.99	1.65	0.004
Log(Capital)	50716	7.27	2.20	90723	7.36	2.24	0.091
Log(Capital Intensity)	50716	5.39	1.83	90723	5.45	1.89	0.067
Share of tangible capital	50727	0.19	0.26	90739	0.18	0.26	-0.004
Asset (€ million)	50727	4.24	12.54	90739	4.64	13.07	0.398
Age	50727	16.66	13.56	90739	16.49	13.07	-0.167
Employment	50727	16.29	35.21	90739	17.58	38.16	1.293
Wage (€ thousands)	50727	30.64	17.43	90739	31.49	16.97	0.846
Leverage	50727	0.75	0.24	90739	0.75	0.24	-0.004

Note: According to the impact of the reform, we divide the judicial districts to the two groups. First, districts that would benefit from the reform; the length of their bankruptcy trial declines. Second, courts that lose from the reform; the length of their bankruptcy trial increases. The (expected) impact of the reform is calculated as following

$$\begin{aligned} \text{Impact of the reform} &= log(\text{Length of proceedings})^{2012,2011}_{\text{Old district(Actual)}} \\ &- log(\text{Length of proceedings})^{2012,2011}_{\text{New district(Hypothetical)}} \end{aligned}$$

The table present the statistics and estimated differences between the two groups. It shows that the differences are small and never significant. Note that the data only include the reformed districts and the time period before the reform. The standard errors are clustered at prereform (old) judicial districts. log(MRPK) is the logarithm of marginal return product of capital, log(Capital) is the logarithm of capital stock deflated at 2010 prices of two digit industry, Wage is the total cost of labour divided by the number of employees. Note that Log(MRPK), Log(Capital), Log(Capital Intensity), Asset, Employment, Wage and Leverage. are winsorized at the 1st and 99th percentile.

Table 4

The main results: Quality of local court enforcement and its impact on firm's outcome

	IV				Reduced form	OLS
	(1) Log(MRPK) b/se	(2) Log(Capital) b/se	(3) Log(Capital Intensity) b/se	(4) Share of intangible capital b/se	(5) Log(Capital) b/se	(6) Log(Capital) b/se
Panel A : Second-stage estimation						
Log(Length of proceedings)	0.180***	-0.189**	-0.159**	-0.035**		
	(0.066)	(0.074)	(0.062)	(0.014)		
Panel B: First-stage estimation, Dependent variable: Log(Length of proceedings)						
Impact of the reform $\times Postreform(year \geq 2013)$	0.512***	0.514***	0.514***	0.514***		
	(0.089)	(0.088)	(0.088)	(0.088)		
Panel C: OLS and Reduced form estimation						
Impact of the reform $\times Postreform(year \geq 2013)$					-0.097***	
					(0.030)	
Log(Length of proceedings)						-0.009
						(0.009)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector $\times Year$	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times Year$	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls $\times Year$	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2015224	2138595	2138595	2138857	2138736	2138595
Ftest	33.40	34.03	34.03	34.05		

Note: The dependent variable, log(MRPK) , in column (1), in panel A , is a continuous variable that estimates the logarithm of marginal return product of capital at each year for each firm. The dependent variable, log(Capital), in columns (2), (5) and (6) of panel A and C is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. The dependent variable, Log(Capital Intensity), in column (3) measures the capital per employee at each year for each firm. The dependent variable, share of intangible capital , in column (4), is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "Log(Length of proceedings)"is a continuous variable that estimates the logarithm of bankruptcy proceedings in days at each year. "Impact of the reform" estimates the the change in bankruptcy trial length that caused by the reform t. It is calculated as follows

$$\text{Impact of the reform} = log(\text{Length of proceedings})^{2012,2011}_{\text{Old district(Actual)}} - log(\text{Length of proceedings})^{2012,2011}_{\text{New district(Hypothetical)}}$$

Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. Table A1, in Appendix I, reports main results without Firm controls $\times$ Year. Table A2, in Appendix I, reports main results with observation only from reformed areas.The standard errors are clustered at prereform (old) judicial districts. *** and ** denote statistical significance at the 1, 5 percent levels respectively.

Table 5

Municipality-Pairs across judicial boundaries

	IV: Second Stage		IV: first Stage
	(1) Log(Capital) b/se	(2) Share of intangible capital b/se	(3) Log(Length of proceedings) b/se
Log(Length of proceedings)	-0.175***	-0.051***	
	(0.031)	(0.013)	
Impact of the reform $\times Postreform(year \geq 2013)$			0.576***
			(0.040)
Firm FE	Yes	Yes	Yes
Sector $\times Year$	Yes	Yes	Yes
Municipality-Pairs $\times Year$	Yes	Yes	Yes
Firm controls $\times Year$	Yes	Yes	Yes
Observations	684554	684636	684554
Ftest	211.63	211.63	

Note: The table reports the results when we restrict our sample to neighboring municipality pairs that are located across judicial districts within the same province. Municipality-Pairs is a set of fixed effect dummies for pairs of neighbouring municipalities that share the longest border but are located in different judicial districts within the same province. The dependent variable, log(Capital), in columns (1) is a continuous variable that estimates the logarithm of capital stock deflated at 2010 prices of two digit industries at each year for each firm. The dependent variable, "share of intangible capital" , in column (2) , is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. The dependent variable, "Log(Length of proceedings)", in column (3)is a continuous variable that estimates the logarithm of bankruptcy proceedings in days for each year. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. The standard errors are clustered at prereform (old) judicial districts. *** and ** denote statistical significance at the 1, 5 percent levels respectively.

Table 6

Controlling for the impact of macroeconomic shocks

	Log(Capital)			Share of intangible capital		
	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Log(Length of proceedings)	-0.226*** (0.062)	-0.145** (0.071)	-0.185*** (0.069)	-0.040*** (0.014)	-0.036** (0.015)	-0.039** (0.016)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
Province ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
GFC Shock ×*Year*	Yes	No	Yes	Yes	No	Yes
SDC Shock ×*Year*	No	Yes	Yes	No	Yes	Yes
Observations	1890752	2127872	1880932	1890999	2128131	1881176
Ftest	34.4	25.4	23.8	34.4	25.4	23.8

Note: The table reports the results when we control for the impact of macroeconomic shocks. To construct the "GFC Shock" we sort courts into fifteen bins according to the number of bankruptcy cases that were filed in 2008. We do the same for 2009 after the global financial crisis hit the Italian economy and find the difference between the position of courts in 2009 compared to 2008. "GFC Shock" is a court level time invariant categorical variable that takes 0 if the position of court does not change between 2008 and 2009. It takes 1 (-1) if the court moves to the next (previous) bin in 2009. This shows the bankruptcy cases increased (decreased) slightly more relative to other courts. It takes 2 (-2) if the court moves two bins forward (backward). This shows the bankruptcy cases moderately increased (decreased) relative to the other courts. It takes 3 (-3) if the courts moves more than two bins forward (backward). This indicates that bankruptcy cases increased (decreased) sharply relative to other courts. Similarly, SDC Shock is a court level time invariant categorical variable that takes values between -3 and 3 and is constructed according to the increase in the number of bankruptcies between 2011 and 2012 after the "Sovereign Debt Crisis" hit the Italian economy. Table A4 indicates the number of observations for each category of SDC shock and GFC shock. Please note the table shows the estimates for the second stage of IV regression. In columns (1-3) , the dependent variable is log(Capital). It is a continuous variable that estimates the logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. In columns (4-6), the dependent variable is "share of intangible capital". It is a continuous variable that estimates the share of intangible assets in total fixed capital at each year for each firm. "Log(Length proceedings) "is a continuous variable that estimates the logarithm of the length of bankruptcy proceedings in days at each year. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm attributes measured in 2012: age; number of employees; average wage and asset. High , Medium and Low show the subsamples of firms that are in the top, second and bottom tertiles respectively of the leverage ratio in 2012 before the reform. The standard errors are clustered at prereform (old) judicial districts. *, ** and *** denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 7

External finance dependence and the role of sectoral intangibility

Panel A:Dependent variable:Log(Capital)	Sorting on Intangible capital intensity of the sector		
	(1)	(2)	(3)
	All sectors	Low	High
	b/se	b/se	b/se
Log(Length of proceedings)	-0.045	-0.088	0.034
	(0.072)	(0.085)	(0.118)
High external finance dependence × Log(Length of proceedings)	-0.282**	-0.143	-0.532***
	(0.113)	(0.139)	(0.172)
Firm FE	Yes	Yes	Yes
Sector ×*Year*	Yes	Yes	Yes
Province ×*Year*	Yes	Yes	Yes
Firm controls ×*Year*	Yes	Yes	Yes
Observations	2138595	1312227	826368
F - test	9.71	9.26	10.17
Panel B: Dependent variable: Share of intangible capital	Sorting on Intangible capital intensity of the sector		
	(1)	(2)	(3)
	All sectors	Low	High
	b/se	b/se	b/se
Log(Length of proceedings)	-0.033*	-0.032*	-0.035
	(0.017)	(0.017)	(0.034)
High External Finance dependence × Log(Length of proceedings)	-0.003	-0.004	-0.001
	(0.020)	(0.027)	(0.033)
Firm FE	Yes	Yes	Yes
Sector ×*Year*	Yes	Yes	Yes
Province ×*Year*	Yes	Yes	Yes
Firm controls ×*Year*	Yes	Yes	Yes
Observations	2138857	1312393	826464
F - test	9.71	9.26	10.17

Note: The table shows the estimates for the second stage of IV regression. In all columns of panel A, the dependent variable is log(Capital). It is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. In all columns of panel B, the dependent variable is "share of intangible capital". It is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "High external finance dependence" is a dummy variable that takes 1 if firms operates in sectors that has higher than median dependency on external finance. Low (High) refers to the subsample of firms that operate in sectors that are less (more) intangible intensive than median level. "Log(Length proceedings) "is a continuous variable that estimates the logarithm of the length of bankruptcy proceedings in days at each year. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. The standard errors are clustered at prereform (old) judicial districts. *, ** and *** denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 8

Court enforcement and debt overhang

	Sorting on Leverage Ratio							
	Log(Capital)				Share of intangible capital			
	(1) Low b/se	(2) Medium b/se	(3) High b/se	(4) b/se	(5) Low b/se	(6) Medium b/se	(7) High b/se	(8) b/se
Log(Length of proceedings)	-0.188**	-0.022	-0.368***		-0.010	-0.038***	-0.060***	
	(0.088)	(0.087)	(0.102)		(0.017)	(0.014)	(0.022)	
$\beta^{High} - \beta^{Low}$				-0.179**				-0.050**
				(0.088)				(0.022)
Firm FE	Yes	Yes	Yes		Yes	Yes	Yes	
Sector $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes	
Province $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes	
Firm controls $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes	
Observations	733004	715761	689818		733081	715848	689916	
F - test	25.6	33.7	47.1		25.6	33.7	47.2	

Note: The table shows the estimates for the second stage of IV regression. In columns (1-3) , the dependent variable is log(Capital). It is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. In columns (5-7), the dependent variable is "share of intangible capital". It is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "Log(Length proceedings) "is a continuous variable that estimates the logarithm of the length of bankruptcy proceedings in days at each year. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. High , Medium and Low shows respectively the subsamples of firms that are in the top, second and bottom tertiles of the leverage ratio in 2012 before the reform. The columns (4) and (8) reports the difference between estimated coefficients for top and bottom tertils. The standard errors are clustered at prereform (old) judicial districts. *, ** and *** denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 9: Court delays and misallocation

	Sorting on Pre-reform MRPK											
	Log(MRPK)				Log(Capital)				Share of intangible capital)			
	(1) Lower 25% b/se	(2) Mid 50% b/se	(3) Upper 25% b/se	(4) b/se	(5) Lower 25% b/se	(6) Mid 50% b/se	(7) Upper 25% b/se	(8) b/se	(9) Lower 25% b/se	(10) Mid 50% b/se	(11) Upper 25% b/se	(12) b/se
Log(Length of proceedings)	0.109*	0.138	0.393***		-0.033	-0.216*	-0.312***		-0.012	-0.018	-0.088***	
	(0.065)	(0.103)	(0.127)		(0.076)	(0.117)	(0.117)		(0.009)	(0.014)	(0.032)	
$\beta^{Upper\ 25\%} - \beta^{Lower\ 25\%}$				0.284**				-0.279**				-0.075***
				(0.123)				(0.132)				(0.028)
Firm FE	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Sector $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Province $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Firm controls $\times Year$	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Observations	482130	1013566	511232		503743	1039183	527693		503892	1039204	527776	
F - test	30.9	32.1	38.2		32.4	32.8	36.6		32.4	32.8	36.6	

Note: The table shows the estimates for the second stage of IV regression. In columns (1-3), the dependent variable is log(MRPK). It is a continuous variable that estimates logarithm of marginal return product of capital at each year for each firm. In columns (5-7) , the dependent variable is log(Capital). It is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. In columns (9-11), the dependent variable is "share of intangible capital". It is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "Log(Length of proceedings) "is a continuous variable that estimates the logarithm of the length of bankruptcy proceedings in days at each year. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. Upper 25%, Mid 50%, and lower 25% shows respectively the subsamples of firms that are in the top quartile, inter-quartile range and bottom quartile of MRPK between 2008 and 2012 within each region and 2 digit industry classification. The columns (4), (8) and (12) reports the difference between estimated coefficients for top and bottom quartiles as well as top 25% and bottom 75%. The standard errors are clustered at prereform (old) judicial districts. *, ** and *** denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 10: Change in the distance from the court and firm's performance

	(1) Log(Capital) b/se	(2) Share of intangible capital b/se	(3) Log(MRPK) b/se	(4) Log(Capital) b/se	(5) Share of intangible capital b/se	(6) Log(MRPK) b/se
Change in distance from the court × Post reform (year≥2013)	-0.00181*** (0.00040)	-0.00029*** (0.00010)	0.00168** (0.00065)	-0.00116*** (0.00028)	-0.00023** (0.00009)	0.00140** (0.00054)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
Pre reform district ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls ×*Year*	Yes	Yes	Yes	Yes	Yes	Yes
Pre reform distance from court ×*Year*	Yes	Yes	Yes	No	No	No
Observations	63623	63629	61271	63623	63629	61271

Note: This table describes the heterogeneous impact of the reform within the prereform judicial districts caused by change in firms' distance from the court. "Change in distance from the court" measures the difference between firm's distance to new and old courts. The OLS regressions in all columns only include firms that located in surpassed districts and thus experienced the non-zero change in distance from the court. Columns 1-3 include the interaction between year fixed effects and firm's distance from the court before the reform. Figure A2, in Appendix I, depicts the relation between firm's distance from old and new courts. Table A5, in Appendix I, provides the summary of statistics for the changes in distance from the court. In columns (1) and (4) The dependent variable is log(Capital. It is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. In columns (2) and (5), the dependent variable is "share of intangible capital". It is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. In columns (3) and (6), log(MRPK) is a continuous variable that estimates the logarithm of marginal return product of capital at each year for each firm. Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. High , Medium and Low shows respectively the subsamples of firms that are in the top, second and bottom tertiles of the leverage ratio in 2012 before the reform. The standard errors are clustered at prereform (old) judicial districts. *, ** and *** denote statistical significance at the 10, 5 and 1 percent level respectively.

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APPENDIX I

Table A1

The main results without Firm controls

	IV				Reduced form	OLS
	(1) Log(MRPK) b/se	(2) Log(Capital) b/se	(3) Log(Capital Intensity) b/se	(4) Share of intangible capital b/se	(5) Log(Capital) b/se	(6) Log(Capital) b/se
Panel A : Second-stage estimation						
Log(Length of proceedings)	0.163**	-0.163**	-0.138**	-0.030**		
	(0.065)	(0.067)	(0.057)	(0.013)		
Panel B: First-stage estimation, Dependent variable: Log(Length of proceedings)						
Predicted impact of the reform × Post reform (year≥2013)	0.512***	0.514***	0.514***	0.514***		
	(0.089)	(0.089)	(0.089)	(0.089)		
Panel C : OLS and Reduced form estimation						
Impact of the reform × $Postreform(year \geq 2013)$					-0.084***	
					(0.028)	
Log(Length of proceedings)						-0.012
						(0.009)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector × *Year*	Yes	Yes	Yes	Yes	Yes	Yes
Province × *Year*	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2059529	2189827	2189827	2190142	2189971	2189827
Ftest	32.98	33.57	33.57	33.57		

Note: The dependent variable, log(MRPK) , in column (1), in panel A , is a continuous variable that estimates the logarithm of marginal return product of capital at each year for each firm. The dependent variable, log(Capital), in columns (2), (5) and (6) of panel A and C is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. The dependent variable,Log(Capital Intensity), in column (3) measures the capital per employee at each year for each firm. The dependent variable, share of intangible capital , in column (4), is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "Log(Length of proceedings)"is a continuous variable that estimates the logarithm of bankruptcy proceedings in days at each year. "Impact of the reform" estimates the the change in bankruptcy trial length that caused by the reform t. It is calculated as follows

$$\text{Impact of the reform} = log(\text{Length of proceedings})^{2012,2011}_{\text{Old district(Actual)}} - log(\text{Length of proceedings})^{2012,2011}_{\text{New district(Hypothetical)}}$$

The standard errors are clustered at prereform (old) judicial districts. *** and ** denote statistical significance at the 1, 5 percent levels respectively.

Table A2

The main results with observations only from reformed areas

	IV				Reduced form	OLS
	(1) Log(MRPK) b/se	(2) Log(Capital) b/se	(3) Log(Capital Intensity) b/se	(4) Share of intangible capital b/se	(5) Log(Capital) b/se	(6) Log(Capital) b/se
Panel A : Second-stage estimation						
Log(Length of proceedings)	0.156***	-0.157**	-0.123**	-0.029**		
	(0.057)	(0.069)	(0.049)	(0.011)		
Panel B: First-stage estimation, Dependent variable: Log(Length of proceedings)						
Predicted impact of the reform × Post reform (year≥2013)	0.512***	0.514***	0.514***	0.514***		
	(0.089)	(0.088)	(0.088)	(0.088)		
Panel C : OLS and Reduced form estimation						
Impact of the reform × $Postreform(year \geq 2013)$					-0.085**	
					(0.032)	
Log(Length of proceedings)						-0.020
						(0.016)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector × *Year*	Yes	Yes	Yes	Yes	Yes	Yes
Province × *Year*	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls × *Year*	Yes	Yes	Yes	Yes	Yes	Yes
Observations	290968	305893	305893	305928	305893	305893
Ftest	41.40	41.59	41.59	41.59		

Note: The dependent variable, log(MRPK) , in column (1), in panel A , is a continuous variable that estimates the logarithm of marginal return product of capital at each year for each firm. The dependent variable, log(Capital), in columns (2), (5) and (6) of panel A and C is a continuous variable that estimates logarithm of capital stock deflated at 2010 prices of two digit industry at each year for each firm. The dependent variable,Log(Capital Intensity), in column (3) measures the capital per employee at each year for each firm. The dependent variable, share of intangible capital , in column (4), is a continuous variable that estimates the share of intangible asset in total fixed capital at each year for each firm. "Log(Length of proceedings)"is a continuous variable that estimates the logarithm of bankruptcy proceedings in days at each year. "Impact of the reform" estimates the the change in bankruptcy trial length that caused by the reform t. It is calculated as follows

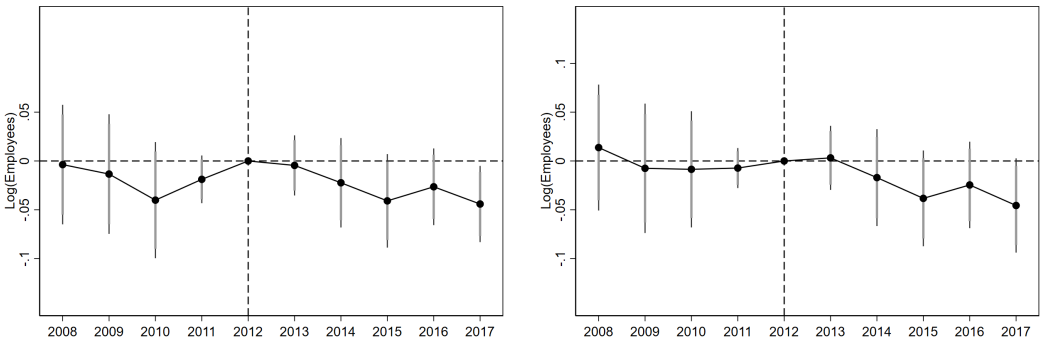
$$\text{Impact of the reform} = log(\text{Length of proceedings})^{2012,2011}_{\text{Old district(Actual)}} - log(\text{Length of proceedings})^{2012,2011}_{\text{New district(Hypothetical)}}$$

Firm controls include four time-invariant categorical variables. They each sort firms into twenty bins according to one of the following firm' attributes measured in 2012: age; number of employees; average wage and asset. The standard errors are clustered at prereform (old) judicial districts. *** and ** denote statistical significance at the 1, 5 percent levels respectively.

Table A3: Court delays and employment

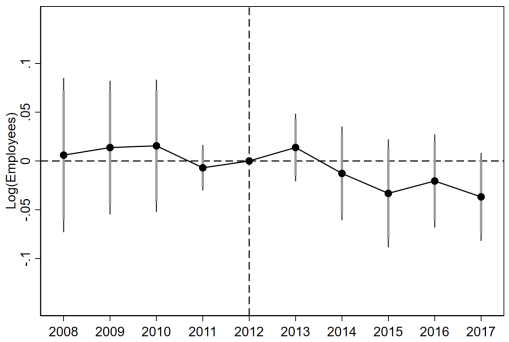
	(1) All firms b/se	(2) Employees>1 in 2012 b/se	(3) Employees≤15 in 2014 b/se
Log(Length of proceedings)	-0.025	-0.037	-0.036
	(0.040)	(0.044)	(0.046)
Firm FE	Yes	Yes	Yes
Sector $\times$*Year*	Yes	Yes	Yes
Province $\times$*Year*	Yes	Yes	Yes
Firm controls $\times$*Year*	Yes	Yes	Yes
Observations	2138857	1880231	1423886
F - test	33.9	32.7	33.56

Figure A1: Court delays and employment



(a) All firms

(b) Employees>1 in 2012



(c) Employees≤15 in 2014

Table and Figure report the impact of court delays on employment. The column (2) in table A3 and subplot (b) in figure A1 report the result for the subsample of firms with more than one employees in 2012. The column (3) in table A3 and subplot (c) in figure A1 report the result for the subsample of firms do not have more than 15 employees and thereby are not affected by the labour reform in 2014.

Table A4

Number of observations for each category of SDC shock and GFC shock

	-3	-2	-1	0	1	2	3
SDC Shock (Observations)	28775	84074	194890	1287818	240480	48764	41972
GFC Shock (Observations)	26418	122566	385776	1090754	175543	82549	43167

Note: The table reports the results when we control for the impact of macroeconomic shocks. To construct the "GFC Shock" we sort courts to fifteen bins according to the number of bankruptcy cases that are filed in 2008. We do the same for 2009 after global financial crisis hit the Italian economy and find the difference between the position of courts in 2009 compared to 2008. "GFC Shock" is a court level time invariant categorical variable that takes 0 if the position of court does not change between 2008 and 2009. It takes 1 (-1) if the court moves to next (previous) bin in 2009. That shows the bankruptcy cases increased (decreased) slightly more relative to other courts. It takes 2 (-2) if the court moves two bins forward (backward). This shows the bankruptcy cases increased (decreased) moderately more relative to the other courts. It takes 3 (-3) if the courts moves more than two bins forward (backward). That indicates the bankruptcy cases increased (decreased) sharply relative to other courts. Similarly, SDC Shock is a court level time invariant categorical variable that takes values between -3 and 3 and constructed according to the increase in number of bankruptcy between 2011 and 2012 after "Sovereign Debt Crisis" hit the Italian economy.

Table A5

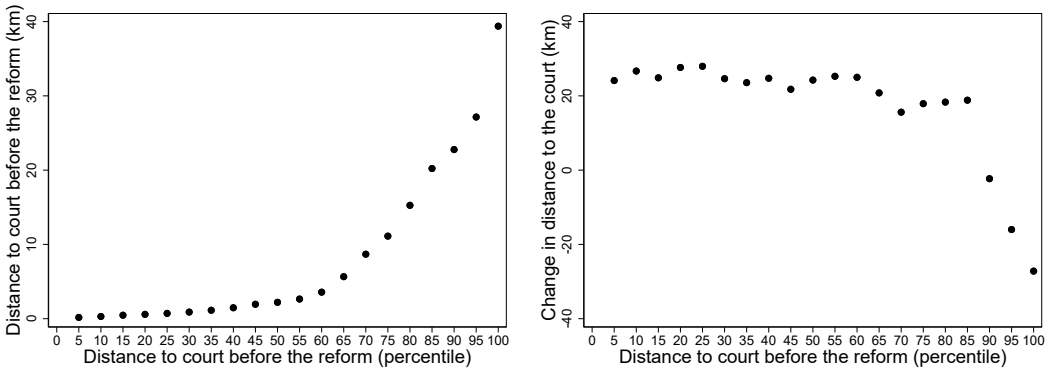
Summary statistics: The distance from the court

	Obs	Mean	Median	Std.Dev.	25%	75%
Distance from the court before the reform (km)	7842	9.12	2.38	23.95	0.80	13.07
Distance from the court after the reform (km)	7842	27.39	26.10	23.28	21.20	32.15
Change in distance from the court (km)	7842	18.27	21.60	15.73	15.36	26.65

Note: Table shows the summary of statistics for distance from the courts before and after the reform.The statistics are based on the sample of firms that located in surpassed districts and thus experienced the non-zero change in distance from the court.

Figure A2

The impact of the reform on the distance from the court



(a) Distance before the reform (b) Change in distance after the reform

Figure indicates the median firm's distance from the court before the reform (panel a) and median change in distance from the court after the reform (panel b) in 2012 for each prereform distance bin. We sort firms into twenty distance bins according to their distance from the court before the reform in 2012. The figure is based on the sample of firms that located in surpassed districts and thus experienced the non-zero change in distance from the court.