



The reform of national judicial geography and access to local financial resources: unintended territorial effects of a central policy in Italy

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Abstract

Italy's judicial system has long been marked by inefficiencies that hinder local economic development. To improve judicial productivity—while at the same time seeking to contain public spending—a central policy was enacted to suppress and merge courts across the country. However, municipalities were excluded from the design and selection process, which resulted in unintended territorial consequences. This study investigates the impact of this reform on access to external financial resources for manufacturing SMEs, using a Difference-in-Difference approach between 2014 and 2018. We find that the consolidation of courts—while aimed at enhancing efficiency—altered the institutional landscape in ways that affected firms' access to capital markets. Specifically, SMEs located in municipalities where courts were merged experienced improved financial conditions, suggesting that judicial centralization may inadvertently influence local credit dynamics. More specifically, we observe a beneficial effect on borrowing costs: SMEs experienced a reduction in approximately 3% in their financing costs. Even if the observed effect is positive, the findings highlight the importance of considering territorial governance and the role of local institutional presence when designing national reforms.

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1 Introduction

Italy's judicial system has long been characterized by structural inefficiencies that hinder economic activity, particularly at the local level (Ippoliti et al. 2015; Giacomelli and Menon 2017; Falavigna et al. 2019; Comi et al. 2021). Lengthy proceedings, inconsistent outcomes, and significant territorial disparities in court performance contribute to a climate of legal uncertainty that discourages investment and weakens firms' growth prospects (Lanau et al. 2014; Djankov et al. 2025). In response to these challenges—and with the additional goal of reducing public expenditure—the Italian government enacted a far-reaching reform of judicial geography in 2012 (Mocetti et al. 2026). The reform consolidated smaller courts into larger judicial districts through Legislative Decrees No. 155 and 156 of 2012, resulting in the suppression of 30 courts, 30 public prosecutor's offices, and 220 detached sections (Ippoliti 2015; Giacomelli et al. 2017). Although the reform aimed to increase efficiency through economies of scale and workload rationalization, it was centrally designed and implemented without local involvement, raising the possibility of uneven territorial effects.

This paper investigates one such unintended consequence: the impact of court consolidation on the cost of external financial resources for manufacturing SMEs. While the reform was not designed to influence credit markets, we show that indeed the improvements in judicial performance, at least in certain district, reduced legal uncertainty and thus alter lenders' risk perceptions. More precisely, the increased distance from courts may reduce access to justice and discourage litigation, indirectly lowering caseloads and easing judicial congestion. These neglected mechanisms may in turn affect the local institutional environment in ways that ultimately shape firms' borrowing conditions. The appropriate consideration of these latent effects generally suggests adopting a broader perspective in the design of public policies and reforms, one that allows for sufficient analytical distance to capture various nuances and consequences of the actions undertaken (Ramello 2025).

The contribution of this paper is threefold. First, it extends the literature on legal uncertainty and credit markets by documenting an effect on the price margin of credit within the territories—specifically, on interest rates—where previous studies typically emphasize quantity-margin responses such as credit rationing. Second, it provides new territorial evidence on how national reforms, even when not explicitly designed with territorial or economic objectives, can generate differentiated local impacts. Third, it exploits a unique institutional setting to show that improvements in judicial performance—whether intentional or indirect—can translate into lower borrowing costs for SMEs, thereby affecting local competitiveness.

Using a Difference-in-Differences approach and comparing municipalities affected and unaffected by court mergers over the period 2014–2018, we find that firms located in treated municipalities experienced, on average, a 3% reduction in interest rates. This result suggests that judicial consolidation reshaped the institutional landscape in ways that positively influenced local credit dynamics. Overall, this study shows that national administrative reforms can produce non-negligible territorial and economic spillovers, even when they are not designed with such

objectives in mind. Taking the reduction in the demand of justice into account, these findings underscore the importance of considering territorial governance and institutional presence when implementing centrally planned reforms.

The rest of the manuscript is organized as follows: Sect. 2 reviews the theoretical background on legal uncertainty and access to finance; Sect. 3 presents the reform and its unintended territorial effects; Sect. 4 introduces the data and empirical strategy; Sect. 5 discusses the results; and Sect. 6 concludes with policy implications.

2 Theoretical background

The Italian reform of judicial geography was primarily motivated by budgetary considerations and aimed to streamline the structure of the justice system by consolidating smaller courts (Panzani 2013; Nuti 2013). The policy was not designed with territorial development or credit-market outcomes in mind. Nevertheless, reforms that alter the functioning of judicial institutions may unintentionally affect firms' financial conditions, especially in contexts where judicial efficiency plays a central role in shaping the degree of legal uncertainty. Drawing on existing literature, we hypothesize that one such unintended effect operates through two endogenous channels: (i) a supply-side channel, whereby performance gains arise from economies of scale and greater specialization, and (ii) a demand-side channel, whereby increased distance to courts reduces access and lowers caseloads. These two channels operate simultaneously and generate a trade-off: reduced accessibility (a negative institutional effect) but improved efficiency and reduced congestion (a positive effect on predictability), ultimately producing a positive impact on firms' access to the local capital market. Specifically, we test whether improvements in court efficiency could lead financial institutions to charge lower interest rates to borrowers (the price-margin effect).

2.1 Judicial quality, legal uncertainty, and access to the capital market

A large body of empirical research shows that legal uncertainty has a twofold impact on corporate finance. On the one hand, higher uncertainty increases firms' cost of capital, because lenders internalize the risk that enforcing collateral or recovering credit may be slow, unpredictable, or costly (Jappelli et al. 2005; Laeven and Majnoni 2005; Fabbri 2010; Rodano et al. 2016; Falavigna et al. 2026). Where courts operate inefficiently, financial institutions tend to charge higher interest rates to riskier borrowers (the price-margin effect). This mechanism is particularly relevant for SMEs, which face tighter financial constraints and rely more heavily on external finance (Bottazzi et al. 2014; De Bliek et al. 2024). On the other hand, legal uncertainty restricts access to external financial resources, amplifying financial constraints (Shah et al. 2017; Moro et al. 2018; Falavigna and Ippoliti 2022; Aguiar-Díaz et al. 2024). Accordingly, where courts operate inefficiently, financial institutions tend to restrict credit supply to riskier borrowers, adopting strategies of credit rationing or borrower reallocation (the quantity-margin effect). Thus, any reform that improves judicial performance—even unintentionally—may reduce lenders'

perceived risk, lower borrowing costs, and increase access to external financial resources.

More specifically, focusing on Italy, Jappelli et al. (2005) investigate average judicial performance at the province level, and they observe a positive impact of reducing legal uncertainty on the quantity margin (credit becomes more available), while they report ambiguous results for the interest rate (the rate is lower or higher depending on the judicial indicator considered in the analysis).¹ When considering Italian firms as observation units, results appear more coherent and less ambiguous. Rodano et al. (2016) explore Italian bankruptcy law reforms, disentangling the effects of reorganization and liquidation procedures on bank financing and firm investment. Specifically, they examine a reform introducing reorganization procedures that facilitate loan renegotiation, followed by a successive reform that strengthens creditor rights in liquidation. According to their results, the first reform increases interest rates and reduces investment, whereas the second reform reduces interest rates and stimulates investment. Hence, the authors observe coherent effects on both the quantity margin and the price margin of judicial uncertainty. Finally, Falavigna and Ippoliti (2023) investigate how inefficiency in enforcing credit rights affects the corporate finance of Italian manufacturing SMEs. Their findings suggest that, when SMEs are exposed to judicial inefficiency, access to both capital markets and trade credit is limited by the uncertainty of the legal environment, causing firms to rely on tax arrears to raise internal liquidity. Again, the results are consistent with the expected negative impact of legal uncertainty on the quantity margin.

Turning our attention to the judicial consolidation reform, we argue that it may affect legal uncertainty through two main channels. The former is the supply-side channel, with performance gains due to economies of scale. Merging small courts into larger judicial districts increases the potential for specialization and allows the reallocation of human resources from suppressed offices to larger, better-equipped courts. This tends to increase processing capacity, reduce backlogs, and improve overall judicial performance. A more efficient judicial environment enhances the predictability of contract enforcement, thereby lowering the perceived credit risk for financial institutions and reducing interest rates for firms. The latter is the demand-side channel, involving reduced access and lower caseloads. Specifically, the reform increased the physical distance between citizens and judicial institutions in many territories, raising the implicit cost of filing claims and accessing courts. This change can reduce the demand for justice, especially for marginal or less severe cases, leading to fewer incoming proceedings. A decline in caseloads indirectly reduces congestion and may further improve clearance rates. Although reduced accessibility may be undesirable from an equity perspective, it can still generate perceived improvements in judicial efficiency, which financial intermediaries may interpret as a lower enforcement risk.

¹ Specifically, the empirical analysis conducted by the authors revealed a positive correlation between the duration of trials and bank lending spreads. On the other hand, the study found a negative correlation between the stock of pending trials and bank lending spreads, with each decrease in one unit in the stock of pending trials per thousand inhabitants being associated with a decline in bank lending spreads.

With respect to the impact of a reform on the Italian judiciary, the available empirical literature remains relatively limited and offers a not entirely clear picture. Mocetti et al. (2026) provide an empirical assessment, finding an increased level of resolved cases by approximately 5 percent and reduced trial duration by a similar magnitude. According to them, the consolidation of smaller courts into larger judicial districts might have enhanced specialization and fostered economies of scale. However, these results are subject to some limitations related to model specification.

Peyrache and Zago (2016) adopt a different empirical approach and reach partially different conclusions. Using Data Envelopment Analysis on Italian court data, they document substantial heterogeneity in judicial performance and find that technical efficiency does not systematically increase with court size. Importantly, the authors distinguish between technical inefficiency (i.e., the failure to adopt best practices) and size inefficiency (i.e., courts operating at a scale that deviates from the optimal one), emphasizing that larger courts may still operate away from the optimal scale of production. Their findings suggest that increases in court size do not automatically translate into improvements in performance and that, beyond a certain threshold, congestion and coordination costs may outweigh the potential benefits of consolidation. Overall, the results point to the existence of an optimal scale for judicial units.

Mocetti et al. (2026) provide additional useful evidence highlighting the role of the demand-side channel. Importantly, their analysis distinguishes between incoming cases (demand for justice) and resolved cases (judicial output), allowing them to separately assess changes in workload and court activity following the reform. Their findings suggest that the increase in the distance between citizens and courts reduced the demand for justice, leading to a decline in incoming cases and, consequently, lower caseloads in the affected districts. Consistent with Espinosa et al. (2017), we may therefore expect the demand for justice to decline, particularly for marginal or less severe cases, leading to fewer incoming proceedings. This reduction in caseloads may alleviate congestion and contribute to higher clearance rates.

This resulting decline in incoming cases contributes to faster proceedings and greater predictability, thereby enhancing the perceived legal certainty in the territories involved. Overall, the mechanism we identify aligns closely with the patterns documented in the literature, reinforcing the plausibility of the channels underlying our hypothesis. It is noteworthy that findings not immediately visible from a superficial perspective emerge as the outcome of multilevel interactions, highlighting the role of indirect and latent institutional effects. The analysis underscores that reforms often generate complex outcomes that are not readily observable (Ramello 2025).

2.2 Hypothesis

Based on the mechanisms outlined above, we expect the judicial reform to have generated unintended yet straightforward improvements in local judicial efficiency. Although the reform was designed for administrative and budgetary purposes—and given the ambiguous nature of its supply-side effects (economies of scale, resource reallocation, and specialization)—the simple demand-side effects (reduced

caseloads due to greater distance from courts) may have locally reduced congestion and enhanced the overall performance of specific judicial offices. We therefore hypothesize that:

H1. Manufacturing SMEs located in municipalities affected by court mergers experienced a lower cost of external financial resources—measured by a reduction in interest rates—relative to otherwise similar firms in unaffected municipalities.

This hypothesis reflects the idea that improvements in judicial efficiency translate into lower perceived enforcement risk for lenders, thereby reducing the borrowing costs faced by firms. It extends previous evidence that typically documents judicial effects along the quantity margin of credit (e.g., credit rationing or borrower reallocation) by showing that judicial performance can also affect the price margin across territories.

3 The reform of the Italian judicial geography

To implement the reform of judicial geography, the Italian government—under the Monti administration—developed a set of objective criteria to guide the selection of courts for closure and consolidation.² These parameters were designed to ensure transparency, consistency, and broad institutional acceptance. The following description outlines the technical and procedural foundations of the reform, including the benchmarks used to evaluate judicial offices, the exemptions applied to provincial capitals, and the final decisions made through parliamentary and judicial consultations.

The evaluation considered judicial activity over the five-year period from 2006 to 2010, assessing both the total number of registered and resolved civil and criminal proceedings. These figures were benchmarked against the average performance of the 103 provincial courts, which were legally protected from closure. Additionally, the analysis was based on the statutorily assigned staffing levels for each judicial office, rather than the actual personnel present. At the conclusion of the assessment, the following average reference values were established:

- average population served: 363,769 inhabitants (national average: 345,606);
- average total incoming cases: 18,094 proceedings (national average: 18,623);
- average number of magistrates: 28 units (national average: 31);

² Specifically, the reform was enacted through Legislative Decrees No. 155 and 156 of 2012, implementing the mandate granted by Law No. 148 of 2011 to revise judicial geography. Decree No. 155 reorganized judicial districts and established a new structure for first-instance judicial offices, resulting in the closure of 30 courts, 30 public prosecutor's offices, and 220 detached sections; while Decree No. 156 carried out a similar reorganization concerning the offices of Justices of the Peace, significantly reducing their number. See Ippoliti (2015) and Giacomelli et al. (2017).

- annual workload per magistrate: 638.4 or 647.1 cases, depending on whether incoming or resolved proceedings were used to measure productivity (national averages: 600.6 and 606.9, respectively).

Based on these benchmarks, the government identified courts that failed to meet at least one of the established criteria. It is important to note that first-instance judicial offices located in provincial capitals were exempt from closure, as was the requirement to maintain a minimum number of courts within each Court of Appeal district (i.e., three offices).

During parliamentary and institutional consultations (including Parliamentary Committees and the High Council of the Judiciary), an additional criterion was introduced: territorial extension, again benchmarked against the average of the 103 provincial courts, excluding the five metropolitan districts of Rome, Milan, Naples, Turin, and Palermo.

Following these deliberations, the Justice Committee of the Chamber of Deputies issued a favorable opinion on August 1, 2012, albeit with several conditions. Among these was the preservation of certain courts in areas with a significant presence of organized crime, ultimately reducing the number of courts to be closed to 31. Notably, the Ordinary Courts of Caltagirone, Sciacca, Castrovillari, Lamezia Terme, Paola, and Cassino were excluded from the final list of closures.

The legislative decree entered into force the day after its publication in the Official Gazette, on September 13, 2012. However, its full implementation was not immediate, as a 12-month transition period was required before the new structure became effective (September 13, 2013).

Nevertheless, the reorganization did not proceed as originally planned. The Ministry of Justice issued a series of subsequent administrative measures that postponed the closure of several courts, granting extensions of up to two years (Panzani 2013). These postponements were motivated by the need to ensure an orderly transition and to allow judicial offices to dispose of substantial backlogs before their formal suppression. Importantly, the implementation process was also shaped by direct actions taken by the Presidents of the affected courts, who issued formal internal regulations to govern the progressive winding down of judicial activities (Nuti 2013). These provisions coordinated the management of pending cases, established procedures to redistribute files, and—crucially—further delayed the physical transfer of judges, clerks, and administrative personnel to the receiving courts. In practice, these local administrative decisions worked in parallel with ministerial decrees to extend the operational life of the offices slated for closure.

As a consequence of this combined series of postponements—both ministerial and locally determined—the reform did not produce immediate territorial effects. The actual consolidation of judicial functions, including the closure of suppressed courts and the full relocation of human resources to the new judicial headquarters, effectively materialized only in 2016. This year marks the point at which the territorial structure of the justice system was concretely altered and is therefore considered the true onset of treatment in our empirical analysis. Note that, despite the early legal announcement of the reform, several courts operated at full capacity until 2015. As a

result, the effective judicial performance experienced by firms did not change before 2016, limiting the scope for economically meaningful anticipation effects.

As highlighted in this section, the reform to suppress and merge courts across Italy was designed and implemented centrally, with no involvement from local municipalities. As a result, these local governments were excluded from both the design phase and the selection criteria used to determine which courts would be suppressed. This institutional setting offers an opportunity to examine the reform's unintended effects on local capital markets. Specifically, as stated in the previous theoretical section, two channels operate simultaneously and generate a trade-off: reduced accessibility (a negative institutional effect), but improved efficiency and reduced congestion (a positive effect on predictability), which together reduce legal uncertainty and ultimately lead to lower interest rates for firms.

To document this expected trade-off, we compute descriptive indicators of court efficiency for treated and untreated courts over the period 2014–2018. We focus on two standard measures widely used in the judicial efficiency literature: (i) the clearance rate, defined as the ratio between resolved and incoming cases, and (ii) the number of incoming cases, which captures the demand for justice faced by each court. For ease of comparison, we normalize the value of each measure in 2014 to 1 and plot their evolution relative to this baseline. Figures 1 and 2 report the collected evidence.

The descriptive evidence reveals clear differences between treated and untreated courts after the reform. In treated courts, incoming cases decline steadily after 2016, suggesting a reduction in the demand for justice associated with increased distance and higher access costs generated by the consolidation. At the same time, treated courts exhibit a higher clearance rate relative to untreated courts. Note that, since the clearance rate is a measure of judicial productivity that does not account for inputs (such as staff or judges), this pattern should not be interpreted as evidence of higher technical efficiency.

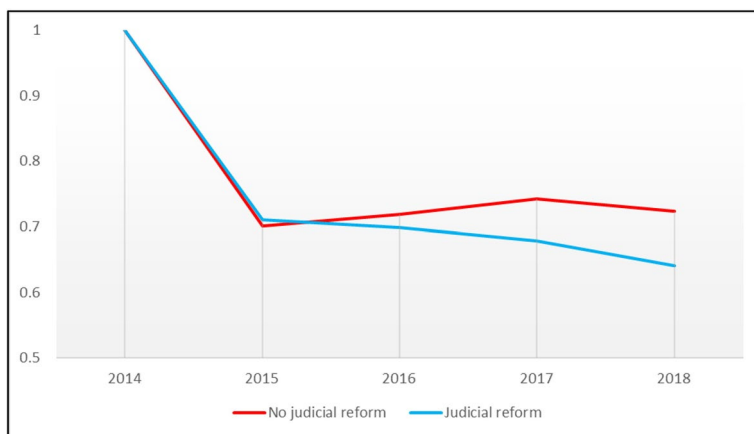


Fig. 1 Incoming cases: treated vs. untreated courts

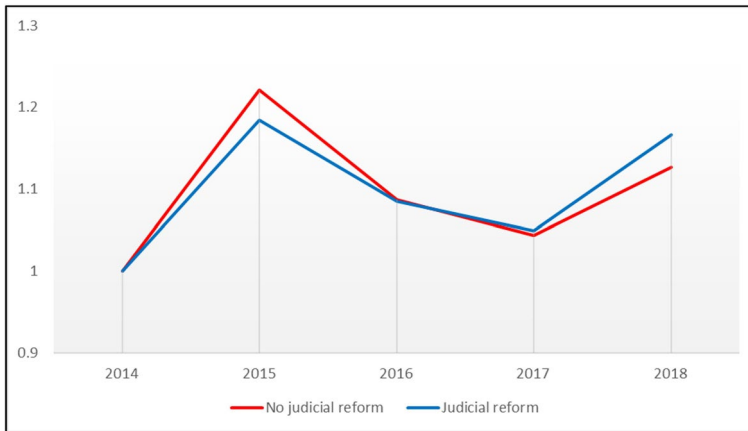


Fig. 2 Clearance Rate: treated vs. untreated courts

Taken together, these descriptive patterns are consistent with the mechanism underlying our empirical analysis: court consolidation appears to have reduced case-load pressure in treated districts, allowing them to dispose of cases at a faster pace relative to incoming demand, even though we cannot ascertain whether underlying efficiency (in terms of inputs and production) actually increased.

In credit markets, what ultimately matters is the expected ability to enforce contracts. If consolidation leads to faster and more predictable proceedings—even at the cost of higher physical distance—lenders may revise downward their assessment of judicial risk. In such cases, the overall effect for borrowing firms is a lower interest rate, even if access to justice becomes more costly for citizens.

While the theoretical argument distinguishes these channels, our empirical design cannot separately identify the contribution of reduced demand and improved supply-side conditions. This is clearly a limit that we acknowledge.

4 Data and method

To investigate the relationship between the reform of judicial geography and the cost of financial resources, we focus on the financial behavior of all active private limited Small and Medium Enterprises (SMEs). The empirical analysis covers the 2014–2018 period, providing two pre-reform years (2014–2015) and two post-reform years (2017–2018). This window is sufficiently large to assess pre-treatment trends while avoiding the confounding effects of the COVID-19 shock beginning in 2020. As previously outlined, although the reform was adopted in 2012, it became effectively operational only in 2016, making this time frame well suited for identifying short- to medium-term effects of the judicial consolidation. Finally, we restrict the analysis to industrial sectors that typically rely heavily on external financial resources to remain competitive, specifically the manufacturing industry (NACE codes 10–33).

The dataset integrates multiple sources. Financial, economic, and administrative data are obtained from AIDA, a Bureau van Dijk database (i); information on local economic conditions is sourced from the Italian Ministry of Economy and Finance (ii); information on judicial geography is collected from the Italian Ministry of Justice (iii); and information on territorial structure is obtained from the Italian National Institute of Statistics (iv).

It is important to note that AIDA covers only limited liability firms (*S.r.l.* and *S.p.A.*) that are legally required to file financial statements with the Italian Chambers of Commerce. As a result, micro-enterprises and non-incorporated firms—typically smaller, less formalized, and less dependent on external finance—are not adequately represented in the data. The “*missing firms*” are therefore not a random subset of the population. Our estimates should thus be interpreted as capturing the effect of judicial reform on the segment of SMEs that regularly access external financial resources and for which accounting data are available. While this may limit external validity for very small firms, it also ensures that the analysis focuses on the portion of the local productive system for which borrowing conditions are most sensitive to institutional changes in judicial performance.

Note that our empirical strategy is constrained by both the institutional setting of insolvency procedures and the nature of the relationship we are investigating. We focus exclusively on SMEs to identify with precision the competent court in the event of insolvency. For small and medium-sized firms, it is reasonable to assume that the firm’s assets are geographically concentrated and that a single local court is competent for the insolvency procedure. In contrast, for larger companies the geographical structure of assets is often more complex, potentially involving multiple establishments across different jurisdictions. In these cases, it becomes difficult to determine with certainty which court would be the relevant competent authority, and this uncertainty would introduce non-trivial misclassification bias into our empirical analysis. For this reason, while we are able to meaningfully explore heterogeneity across territories, we cannot credibly investigate heterogeneity across firm size with the available data and within the institutional framework of Italian insolvency law.

4.1 Empirical strategy

To identify the causal impact of court mergers on the cost of debt, we employ a Difference-in-Differences (DiD) research design. The treatment group consists of municipalities whose court was merged or closed as part of the judicial geography reform, while the control group includes municipalities unaffected by the reorganization. This approach is consistent with the recent empirical literature evaluating the same reform (Canzian and Ferrara 2025).

The DiD estimator is well suited for this context, as it differences out time-invariant heterogeneity across municipalities and common national shocks over time. The identification assumption requires that—absent the reform—the evolution of borrowing costs would have followed parallel trends in treated and untreated municipalities.

Our baseline specification is:

$$Y_{i,t} = \beta(\text{Post}_t * \text{Treated}_i) + \delta_i + \varphi_t + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the average interest rate paid by manufacturing SMEs in municipality i and year t ; $\text{Post}_t * \text{Treated}_i$ is the DiD interaction term capturing the effect of the court merger reform; δ_i and φ_t denote municipality and year fixed effects; and $\varepsilon_{i,t}$ is the error term. Standard errors are clustered at the municipality level. Note that we estimate weighted regressions using analytical weights equal to the number of taxpayers in each municipality, so that economically larger local markets contribute proportionally more to the estimated effect. This approach allows for meaningful comparisons between large urban centers (e.g., Milan, Rome, and Naples) and smaller municipalities, ensuring that the analysis reflects the relative economic relevance of each area.

To account for territorial heterogeneity and local economic conditions that may influence credit market functioning, we augment the baseline model with a set of control variables capturing structural, geographical, and economic characteristics of each municipality. Specifically, we estimate models of the form:

$$Y_{i,t} = \beta(\text{Post}_t * \text{Treated}_i) + \gamma X_{i,t} + \delta_i + \varphi_t + \varepsilon_{i,t} \quad (2)$$

where $X_{i,t}$ includes measures of fiscal and entrepreneurial density, indicators for mountain and coastal municipalities, and alternative income-based proxies (introduced one at a time to avoid multicollinearity), such as income from dependent employment, pension income, real estate income, or income from SME sales.

As robustness checks, we estimate alternative specifications with geographical fixed effects at different territorial levels. Besides the model including municipality fixed effects—our most rigorous specification—we also estimate models with regional and macro-area fixed effects. These weaker forms of spatial fixed effects allow us to verify that the estimated impact of the reform is not driven by unobserved regional shocks or large-scale territorial patterns. Note that models including multiple high-dimensional fixed effects are estimated using the *reghdfe* Stata module (Correia and Constantine 2026), which allows for the absorption of any number of fixed effects and is widely used in applied econometrics.

As an additional robustness check, we estimate a dynamic specification including one- and two-year lags of the dependent variable:

$$Y_{i,t} = \beta(\text{Post}_t * \text{Treated}_i) + \rho_1 Y_{i,t-1} + \rho_2 Y_{i,t-2} + \delta_i + \varphi_t + \varepsilon_{i,t} \quad (3)$$

This approach controls the intrinsic persistence of local credit conditions, ensuring that our results are not driven by pre-existing trends or gradual adjustments in borrowing costs. Note that, when estimating the dynamic specification including one- and two-year lags of the dependent variable, we adopt macro-area fixed effects instead of municipality fixed effects. This choice avoids collinearity and dynamic panel bias that arise when lagged dependent variables are combined with highly granular fixed effects in short panels (Nickell 1981). Macro-area fixed effects still absorb broad territorial shocks while allowing the model to identify the dynamic persistence of local credit conditions.

Finally, to validate the parallel-trend assumption underlying the DiD identification, we estimate a dynamic event study model:

$$Y_{i,t} = \sum_{k \neq 0} \beta_k (\text{EventTime}_k * \text{Treated}_i) + \delta_i + \varphi_t + \varepsilon_{i,t} \quad (4)$$

where EventTime_k indexes the years before and after the reform (with $k=0$ omitted as the reference period). Specifically, if the pre-treatment coefficients ($k = -2, -1$) are statistically indistinguishable from zero, we provide evidence that treated and control municipalities exhibited parallel trends prior to the reform.

4.2 Dependent variable

The dependent variable in our regression models serves as a proxy for the cost incurred by firms when accessing external financial resources from the capital market. Specifically, it is calculated as the ratio of total financial charges to total financial debt for firm (i) at time (t). Financial charges are extracted from the firm's income statement, while financial debt figures are obtained from the balance sheet (source: AIDA, Bureau van Dijk). This measure includes both short-term (less than one year) and long-term (one year or more) financial debt. After calculating the relevant indicators for all manufacturing SMEs and removing outliers, we estimate the average interest rate at the municipality level. Note that, as outlier strategy in this calculation, observations with values exceeding the 10% threshold are excluded. These extreme values often result from financial debts being extinguished before the end of the fiscal year—cases in which the balance sheet no longer reflects the debt, while the income statement still records the associated financial charges. This mismatch can lead to artificially inflated ratios, which are removed to preserve the integrity of the analysis.

Looking at the Second Instance District of Torino (i.e., *Distretto di Corte d'Appello*), Fig. 3 maps the observations along with the estimated average interest rate. As shown, data are not available for all municipalities—either because they are classified as outliers (interest rate > 0.1) or because no manufacturing SMEs are in those areas.

Note that, as robustness test, we adopt the lagged dependent variable to account for the pre-existing financial dynamics of the municipalities. Hence, the dependent variable at time $t-1$ and $t-2$ are also included as a regressor. This allows us to control temporal persistence in local credit conditions, capturing the fact that borrowing costs often exhibit inertia due to financial market frictions, long-term relationships between lenders and firms, or gradual adjustment processes. This can ensure that the estimated effects of the reform are not confounded by prior trends in financial conditions.

Afterward, we implement an additional robustness check to ensure that our results are not sensitive to the specific cutoff used for detecting abnormal debt reimbursement. First, we re-estimate the models in the main analysis using a more conservative threshold of 15% for the interest-rate proxy, instead of the original 10%. This broader cutoff substantially reduces the risk of inadvertently excluding valid

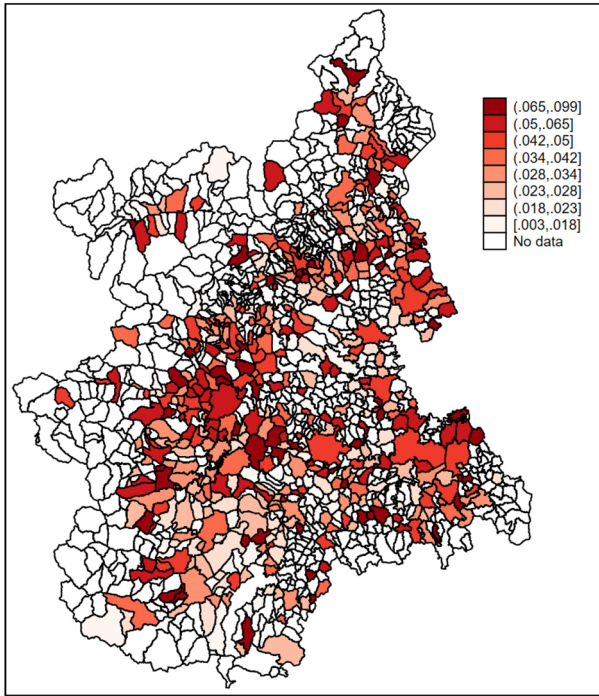


Fig. 3 Second Instance District Court of Torino and its municipalities. Notes: the mapping stratifies municipalities according to the average interest rate observed in 2011

observations. Second, we estimate those models without applying any outlier threshold, retaining the full distribution of the dependent variable and relying exclusively on the fixed-effects structure and cluster-robust standard errors to absorb potential noise.

4.3 Explanatory variable

The identification of municipalities affected by the reform is based on official data obtained directly from the Italian Ministry of Justice, which provides detailed information on the courts involved in the merging process and their jurisdiction (i.e., the municipalities they serve). This allows us to accurately classify observations into treated and non-treated groups, ensuring the validity of our Difference-in-Difference design. It is important to note that we expect the onset of treatment effects starting from 2016, when the transfer of judicial responsibilities and human resources was effectively completed, and a new organization can be adopted in the merged courts.

Figure 4 presents two maps, comparing the judicial geography before the reform (i.e., the territorial organization of courts as it existed prior to the planned implementation in 2015) and after the reform (i.e., the structure as defined by the consolidation plan approved in 2012 and scheduled to come into force after 2015). The

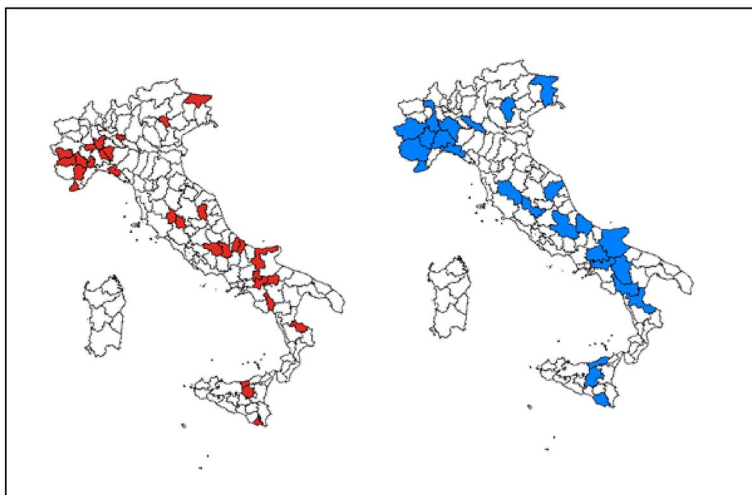


Fig. 4 Judicial geography, before and after the reform. Notes: we highlighted in red the courts that were planned to be suppressed by the reform, and in blue those that were newly merged

figure highlights the courts that were planned to be suppressed (in red) and those that were newly merged (in blue). Note that the maps report the courts identified for closure by the central government in 2012, including the five tribunals that are still formally open (Avezzano, Lanciano, Sulmona, Urbino, and Vasto).

4.4 Control variables

We introduce several controls to ensure that the estimated treatment effects are not driven by structural, demographic, or economic differences across municipalities, but reflect the unintended impact of the judicial geography reform on the cost of external financial resources for SMEs.

First, we introduce variables that concern the territorial structure (source: Italian Institute of Statistics). Specifically, we include two dummy variables to capture morphological features of the local area:

- a dummy identifying mountain municipalities (1 = mountain area; 0 otherwise), which are typically characterized by geographical isolation, higher transportation costs, lower availability of financial services, and overall lower economic density;
- a dummy identifying coastal municipalities (1 = located on the coastline; 0 otherwise), which generally exhibit distinct economic patterns driven by tourism, seasonal activities, and higher levels of market dynamism.

These territorial indicators account for structural constraints and opportunities that may affect SMEs' financing conditions independently of judicial efficiency.

Then, we propose variables that concern territorial economic density (source: Italian Ministry of Economy and Finance, and AIDA). Specifically, we include two continuous variables to control for differences in local economic concentration:

- fiscal density, defined as the number of individual taxpayers per square kilometer, which proxies the intensity of economic activity, the strength of the fiscal base, and the potential demand for financial services in the area;
- entrepreneurial density, computed as the number of manufacturing SMEs per square kilometer, which captures the extent of local business activity, competition, and the overall development of the entrepreneurial ecosystem, typically associated with more competitive and accessible credit markets.

Note that we estimate weighted regressions using analytical weights equal to the total number of individual taxpayers in each municipality. This approach allows municipalities with a larger fiscal base—and therefore a larger underlying credit market—to contribute proportionally more to the estimation. Since the weighting variable (total taxpayers) and the fiscal density control (taxpayers per square kilometer) capture different concepts—market size versus spatial concentration—using both does not generate collinearity or identification issues.

Afterward, focusing on the local income structure, we estimate four alternative proxies (source: Italian Ministry of Economy and Finance, and AIDA). Specifically, we include the following variables for the economic structure of the municipality:

- income from dependent employment, capturing the strength and stability of the local labor market (expressed in thousands of euros);
- pension income, reflecting demographic composition and income stability in areas with older populations (expressed in thousands of euros);
- income from real estate assets, proxying for local wealth, urbanization, and property market conditions (expressed in thousands of euros);
- income generated by SME sales, which captures firm-level economic activity and potentially local demand for external financial resources (expressed in thousands of euros).

Note that, since these components are likely to be correlated, we include them one at a time in separate regressions to avoid multicollinearity. This modeling strategy enables us to disentangle heterogeneous economic dimensions while avoiding mechanical overlaps among highly correlated income indicators.

Finally, we introduce several sets of fixed effects that capture different layers of territorial and temporal heterogeneity. Specifically, we include year fixed effects to control for national-level shocks and macroeconomic trends over time. We include fixed effects for Italian regions and municipalities, which allow us to account for local policy interventions and resource allocations that may influence access to capital markets. Additionally, we also incorporate fixed effects for the five Italian macro-regions (North-West, North-East, Center, South, and Islands), which may reflect broader structural and institutional differences across large geographical areas. While macro-regional effects capture overarching territorial dynamics, regional

Table 1 Descriptive statistics of dependent and independent variables

Variable	Obs	Mean	Std. dev	Min	Max
(Log) Interest rate t	20,804	-2.911	0.391	-9.522	-2.303
Treated i	20,804	0.186	0.389	0.000	1.000
Post t	20,804	0.638	0.481	0.000	1.000
(Log) Pension income t	20,804	18.886	1.952	11.770	23.410
(Log) Income from real estate t	20,804	16.646	2.134	9.700	21.630
(Log) Income from dependent employment t	20,804	19.456	1.933	12.910	24.040
(Log) Income from SMEs' sales t	15,476	12.317	2.520	1.099	17.659
Entrepreneurial density t	20,804	4.500	7.391	0.004	68.174
Fiscal density t	20,804	967.843	1304.267	1.301	6804.646

Number of individual taxpayers in each municipality as analytical weight

Table 2 Descriptive statistics of fixed effects

Variable	Obs	Mean	Std. dev	Min	Max
Year 2014	20,804	0.176	0.381	0.000	1.000
2015	20,804	0.186	0.389	0.000	1.000
2016 (policy reform)	20,804	0.207	0.405	0.000	1.000
2017	20,804	0.216	0.412	0.000	1.000
2018	20,804	0.215	0.411	0.000	1.000
Coastal municipality	20,804	0.253	0.435	0.000	1.000
Mountain municipality	20,804	0.114	0.318	0.000	1.000
North-West	20,804	0.312	0.463	0.000	1.000
North-East	20,804	0.241	0.427	0.000	1.000
Center	20,804	0.191	0.393	0.000	1.000
South	20,804	0.181	0.385	0.000	1.000
Islands	20,804	0.076	0.265	0.000	1.000

Number of individual taxpayers in each municipality as analytical weight

and municipal fixed effects help isolate the role of localized strategies and support mechanisms aimed at improving financial access for SMEs. As part of our empirical strategy, we propose a series of alternative regression models that incorporate these different layers of fixed effects to test the robustness of our results.

Tables 1 and 2 provide descriptive statistics for the sample under investigation. Specifically, Table 1 displays the dependent and independent variables for the municipalities included in the analysis, while Table 2 reports an overview of the fixed effects used in the empirical model.

Table 2 reports descriptive statistics of the key categorical variables used as fixed effects in the empirical analysis—specifically, year, geographical macro-areas, and the morphological classification of municipalities (mountain vs. coastal). These statistics provide an overview of the distribution of observations across time and space, helping the reader to understand how the sample is structured across the Italian

Table 3 Main analysis on the unintended impact of judicial geography reform on municipalities

Variables	(Log) Interest rate t				
	(1)	(2)	(3)	(4)	(5)
Treated t ## Post t	-0.0277* (0.0150)	-0.0311** (0.0152)	-0.0276* (0.0154)	-0.0330** (0.0159)	-0.0328** (0.0158)
Post t	-0.135*** (0.00716)	-0.129*** (0.00775)	-0.131*** (0.00728)	-0.129*** (0.00780)	-0.130*** (0.00872)
Entrepreneurial density t		0.00211 (0.00273)	0.00209 (0.00274)	0.00192 (0.00281)	0.000246 (0.00417)
Fiscal density t		-4.27e-05 (0.000167)	-0.000169 (0.000159)	-0.000141 (0.000198)	5.16e-06 (0.000281)
(Log) Income from dependent employment t		-0.286** (0.143)			
(Log) Income from real estate t			0.102 (0.0889)		
(Log) Pension income t				-0.484*** (0.170)	
(Log) Income from SMEs' sales t					0.00688 (0.0120)
Constant	-2.780*** (0.00441)	2.809 (2.727)	-4.335*** (1.462)	6.494** (3.218)	-2.868*** (0.263)
Year (FE)	Yes	Yes	Yes	Yes	Yes
Municipality (FE)	Yes	Yes	Yes	Yes	Yes
F test	555.74	382.39	368.06	398.80	260.45
Prob > F	0.0000	0.0000	0.0000	0.0000	0.0000
R-squared	0.739	0.739	0.739	0.739	0.741
Observations	20,463	20,463	20,463	20,463	15,150

OLS regression model with multiple fixed effects (i.e., *reghdfe*). Total number of individual taxpayers in each municipality as analytical weight. The “Treated” variable is omitted due to collinearity with the fixed effects. Robust standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

territory. It also shows the extent of municipal heterogeneity that the fixed effects are intended to absorb in the regression models.³

³ For reasons of space and readability, we do not report descriptive statistics for municipalities and regions.

5 Results

Tables 3, 4, 5, and 6 present the results of our investigation. Specifically, Table 3 reports the findings from the main analysis, which employs municipalities as fixed effects. Tables 4 and 5 display the results of the first robustness check, using an

Table 4 First robustness test on the unintended impact of judicial geography reform on municipalities (regions as FE)

Variables	(Log) Interest rate t				
	(1)	(2)	(3)	(4)	(5)
Treated i ## Post t	−0.0490*** (0.0172)	−0.0306* (0.0158)	−0.0312** (0.0158)	−0.0316** (0.0157)	−0.0395** (0.0162)
Treated i	−0.0158 (0.0248)	−0.00576 (0.0164)	−0.00568 (0.0162)	−0.00756 (0.0163)	−0.00205 (0.0204)
Post t	−0.110*** (0.00949)	−0.126*** (0.00738)	−0.122*** (0.00740)	−0.125*** (0.00738)	−0.117*** (0.00920)
Entrepreneurial density t		−0.00410* (0.00223)	−0.00447** (0.00220)	−0.00328 (0.00222)	−0.00280 (0.00337)
Fiscal density t		1.43e-05 (1.13e-05)	2.00e-05* (1.12e-05)	1.18e-05 (1.13e-05)	7.60e-05*** (1.50e-05)
(Log) Income from dependent employment t		0.0667*** (0.00586)			
(Log) Income from real estate t			0.0583*** (0.00537)		
(Log) Pension income t				0.0652*** (0.00577)	
(Log) Income from SMEs' sales t					0.0239*** (0.00329)
Coastal municipality		0.0472*** (0.0173)	0.0405** (0.0179)	0.0440** (0.0174)	0.0877*** (0.0214)
Mountain municipality		−0.0647*** (0.0153)	−0.0730*** (0.0151)	−0.0735*** (0.0150)	−0.0996*** (0.0179)
Constant	−2.791*** (0.0120)	−4.084*** (0.105)	−3.760*** (0.0803)	−4.016*** (0.0997)	−3.145*** (0.0378)
Year (FE)	Yes	Yes	Yes	Yes	Yes
Region (FE)	Yes	Yes	Yes	Yes	Yes
F test	324.45	357.02	356.13	350.55	189.23
Prob > F	0.0000	0.0000	0.0000	0.0000	0.0000
Observations	20,804	20,804	20,804	20,804	15,476
R-squared	0.222	0.334	0.331	0.334	0.317

OLS regression model with multiple fixed effects (i.e., *reghdfe*). Total number of individual taxpayers in each municipality as analytical weight. Robust standard errors clustered at municipality level in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5 First robustness test on the unintended impact of judicial geography reform on municipalities (macro-areas as FE)

Variables	(Log) Interest rate t				
	(1)	(2)	(3)	(4)	(5)
Treated i ## Post t	-0.0683*** (0.0258)	-0.0385** (0.0159)	-0.0400** (0.0159)	-0.0405** (0.0159)	-0.0513*** (0.0168)
Treated i	0.00450 (0.0349)	6.81e-05 (0.0154)	-0.00434 (0.0152)	-0.00553 (0.0152)	0.00916 (0.0209)
Post t	-0.0925*** (0.0225)	-0.119*** (0.00789)	-0.115*** (0.00822)	-0.117*** (0.00818)	-0.107*** (0.0111)
Entrepreneurial density t		-0.00450* (0.00242)	-0.00457* (0.00237)	-0.00360 (0.00244)	-0.00666* (0.00379)
Fiscal density t		2.10e-05* (1.26e-05)	2.58e-05** (1.27e-05)	2.04e-05 (1.28e-05)	8.29e-05*** (1.73e-05)
(Log) Income from dependent employment t		0.0662*** (0.00524)			
(Log) Income from real estate t			0.0573*** (0.00507)		
(Log) Pension income t				0.0629*** (0.00542)	
(Log) Income from SMEs' sales t					0.0284*** (0.00418)
Coastal municipality		0.0512*** (0.0193)	0.0445** (0.0198)	0.0497** (0.0194)	0.0915*** (0.0225)
Mountain municipality		-0.0704*** (0.0135)	-0.0748*** (0.0136)	-0.0775*** (0.0135)	-0.0962*** (0.0177)
Constant	-2.656*** (0.0369)	-3.952*** (0.0973)	-3.622*** (0.0800)	-3.863*** (0.0988)	-3.068*** (0.0523)
Year (FE)	Yes	Yes	Yes	Yes	Yes
Geographical macro-areas (FE)	Yes	Yes	Yes	Yes	Yes
F test	262.36	308.74	294.46	297.90	157.79
Prob > F	0.0000	0.0000	0.0000	0.0000	0.0000
Mean VIF	2.54	3.12	3.12	3.12	2.79
Observations	20,804	20,804	20,804	20,804	15,476
R-squared	0.183	0.320	0.316	0.317	0.298

OLS regression model. Total number of individual taxpayers in each municipality as analytical weight

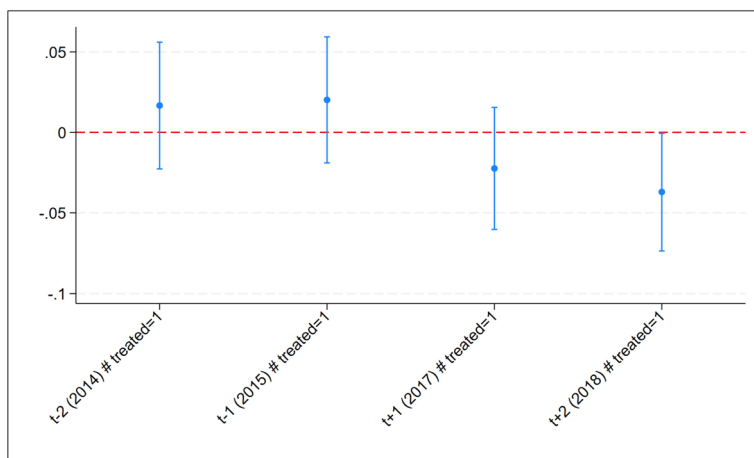
Robust standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

alternative specification with regions and geographical macro-areas as fixed effects. Table 6 shows the results of the second robustness test, focusing on lagged dependent variables. Afterward, Fig. 5 presents the results of the event study that we adopted to verify the parallel-trend assumption. Appendix provides additional evidence, presenting in Tables 7 and 8 the main analysis with alternative restrictions to

Table 6 Second robustness test on the unintended impact of judicial geography reform on municipalities

Variables	(Log) Interest rate t	
	(1)	(2)
Treated $_i$ ## Post $_t$	-0.0259** (0.0122)	-0.0268* (0.0161)
Treated $_i$	-0.00332 (0.0171)	-0.000540 (0.0205)
Post $_t$	-0.0723*** (0.0102)	-0.0752*** (0.00932)
(Log) Interest rate $_{t-1}$	0.548*** (0.0192)	0.454*** (0.0148)
(Log) Interest rate $_{t-2}$		0.203*** (0.0142)
Constant	-1.262*** (0.0483)	-0.984*** (0.0535)
Year (FE)	Yes	Yes
Geographical macro-areas (FE)	Yes	Yes
R-squared	0.524	0.522
Observations	23,489	19,076

OLS regression model. Total number of individual taxpayers in each municipality as analytical weight. Robust standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

**Fig. 5** Event study: treated vs. control (dynamic effects)

the sample (i.e., measure of outliers), and in Table 9 the detailed results of the event study.

Estimations are based on Ordinary Least Squares (OLS) regression using a pooled sample of observations, as well as on OLS with multiple fixed effects (i.e.,

reghdfe). To improve the reliability of the estimates, robust standard errors at the municipality level are applied in the analysis. In the sensitivity analysis, Column 1 presents the baseline model, while Columns 2 through 5 progressively introduce additional control variables related to local economic conditions.

As part of the diagnostic checks, we assess multicollinearity using the mean Variance Inflation Factor (VIF) in the OLS models, and we examine the distribution of residuals. Both diagnostics yield acceptable results, with mean VIF values ranging between 2.54 and 3.12, indicating no serious multicollinearity concerns.

We also test the overall significance of each model using the F test, which evaluates the null hypothesis that all regression coefficients are jointly equal to zero. All models are statistically significant (p value < 0.000), suggesting that the independent variables collectively explain a substantial portion of the variance in the dependent variable.

5.1 Discussion

The results presented in Table 3 provide strong support for the proposed hypothesis: there is a statistically significant and negative relationship between the implementation of the judicial geography reform and the interest rate paid by SMEs (p value < 0.1). Specifically, firms located in municipalities affected by the reform—those involved in the court merging process—experienced, on average, a 2.77% reduction in interest rates (Column 1). Since the dependent variable is expressed in logarithmic form, the coefficient of -0.0277 can be interpreted as a percentage change, indicating that the reform is associated with a 2.77% lower cost of borrowing for treated firms compared to the control group. This finding is consistently confirmed across extended model specifications (Columns 2–5), which incorporate control variables related to territorial structure and economic conditions. In these models, the coefficient remains negative and statistically significant (p value < 0.1), with the estimated impact ranging from 2.76% (Column 3) to 3.30% (Column 4).

To assess the economic relevance of the estimated effects, we quantify the implied interest savings for a representative SME. Based on the AIDA data used in this study, the average manufacturing SME carries approximately €800,000 in bank debt.⁴ Applying our estimated reduction in borrowing costs, a 2–3% decrease in the interest rate corresponds respectively to €16,000 and €24,000 in annual savings. These amounts are far from negligible for manufacturing SMEs, which typically operate with limited liquidity margins and face significant financial constraints. An annual cost reduction of €16,000–€24,000 can support investment in new machinery, workforce expansion, working capital stabilization, or improved creditworthiness. This quantification highlights that the unintended effects of the judicial consolidation reform—although modest in percentage terms—translate into meaningful improvements in firms' financial conditions.

⁴ In this estimation, we consider both the short-term (< 1 year) and the long-term (> 1 year) financial debts, considering the 2018 as reference year.

Considering the parallel-trend assumption, the coefficients for the pre-reform years ($t-2$ and $t-1$) are very small and statistically insignificant (p value equal to 0.406 and 0.312), confirming that treated and control municipalities followed parallel trends prior to the reform (see the results of the event study in Fig. 5 and Table 9). Moreover, the first robustness check presented in Tables 4 and 5, which replaces municipalities' fixed effects with regional and macro-areas fixed effects, further validates the relationship. The estimated effect remains negative and statistically significant across all specifications (p value < 0.1). Finally, considering the second robustness test (Table 6), we include a lagged dependent variable as a regressor and we verify whether results are not driven by pre-existing trends or gradual adjustments in borrowing costs, controlling the intrinsic persistence of local credit conditions. According to results, we confirm a statistically significant and negative relationship between the implementation of the judicial geography reform and the interest rate paid by firms (p -value < 0.1).

These results underscore the role of judicial efficiency in shaping the risk perceptions of financial institutions. Firms operating in municipalities with historically longer judicial delays face higher borrowing costs, as lenders internalize the increased risk of legal uncertainty—particularly in cases of insolvency or contract enforcement. The reform, by consolidating courts and presumably improving judicial performance, reduces this perceived risk. Consequently, firms in treated areas benefit from improved access to capital markets at more favorable rates. The consistency of these findings across multiple specifications highlights their robustness and relevance for policy. They suggest that centrally implemented judicial reforms, even if not designed with local economic outcomes in mind, can have meaningful and positive spillovers on financial conditions for SMEs. This aligns with existing literature in Law and Finance (e.g., Bae and Goyal 2009; Gopalan et al. 2016; Rodano 2021), which emphasizes the importance of legal institutions in facilitating access to finance and reducing refinancing risks.

Considering the heterogeneity across territories, the results in Table 5 confirm the presence of a North–South gradient. SMEs located in the northern regions of Italy exhibit a statistically significant lower cost of debt compared to those located in the Islands (p value < 0.01). At the same time, no statistically significant difference emerges between the South and the Islands. This pattern is consistent with the well-documented territorial divide in Italy in terms of institutional quality and judicial performance, which tend to be higher in northern regions (Ippoliti and Tria 2020; Troisi and Alfano 2023). The North is characterized by faster judicial proceedings, more efficient courts, and a more stable administrative environment, all of which contribute to lower perceived credit risk for lenders. These institutional features translate into more favorable financial conditions for firms operating in northern territories. In contrast, SMEs in southern regions and the Islands face a structural disadvantage due to slower judicial processes, greater uncertainty in contract enforcement, and overall weaker institutional capacity, which are commonly associated with higher borrowing costs.

In summary, the evidence cannot reject the hypothesis that judicial reform significantly influences firms' financial costs, generating an unintended but beneficial effect for municipalities involved in the intervention. These findings reinforce

the broader economic relevance of judicial efficiency and suggest that institutional reforms can indirectly support firm growth and financial resilience.

5.2 Limits

While the findings of this study offer valuable insights into the unintended financial effects of judicial geography reform, several limitations should be acknowledged. First, the use of interest rates as a proxy for access to capital markets, while informative, may not capture the full spectrum of financial constraints faced by SMEs, such as credit availability, loan size, or collateral requirements. Second, the perspective is limited to the SMEs; therefore, we do not observe the broader impact on the financial market or on other economic agents (i.e., the smallest and the largest ones). Third, the exclusion of municipalities without manufacturing SMEs may limit the generalizability of the results, particularly in regions with different industrial compositions or financial ecosystems. Fourth, the empirical strategy cannot separately identify the two channels, i.e., the supply-side channel (economies of scale, specialization, reallocation of human resources) and the demand-side channel (reduced access to courts and lower caseload). Additionally, the reform's impact may vary depending on the specific characteristics of the merged courts (e.g., workload, staffing, digitalization), which are not directly accounted for in the current model.

Future research could address these limitations by: incorporating firm-level data to explore heterogeneous effects across sectors, firm sizes, and credit histories (i); examining long-term impacts on firm performance, investment behavior, and employment (ii); investigating the role of complementary institutional reforms (e.g., digital justice tools, staffing changes) in amplifying or mitigating the observed effects (iii); extending the analysis to other countries undergoing similar judicial restructuring, to assess the external validity of the findings (iv). Such extensions would deepen our understanding of how institutional reforms shape local economic outcomes and inform more inclusive and territorially sensitive policy design.

6 Conclusions

The findings of this study highlight a critical lesson for policymakers: even centrally designed reforms, when implemented without territorial coordination, can produce significant and unexpected local effects—both positive and negative. Actually, the

scholars should always pay attention to the multilevel interaction that may lead to latent institutional consequences. In the case of the reform of national judicial geography, originally aimed at reducing public expenditure and improving judicial productivity, inadvertently enhanced financial access for SMEs in affected municipalities by reducing legal uncertainty. This unintended outcome reflects the interaction between two distinct local mechanisms. While performance gains from economies of scale and increased specialization are not self-evident, the reorganization—by increasing the distance to courts for some citizens—reduced access to justice and lowered caseloads, thereby easing congestion. This resulted in a clear trade-off: reduced accessibility to justice (a negative institutional effect) on the one hand, and improved efficiency and predictability (a positive institutional effect) on the other. Overall, these effects appear to have generated a net positive impact on firms' access to local capital markets.

Overall, these results raise broader concerns about how to balance the intended and unintended consequences of centralized policies. Future reforms should involve local stakeholders to evaluate efficiency gains against potential social costs—such as the equitable access to justice highlighted in this case. Moreover, regional development strategies should explicitly consider the indirect effects of national policies, particularly those that shape institutional quality and influence investor confidence.

Centralized reforms often rely on a linear cause–effect logic, overlooking the systemic complexity and interdependence of institutional settings such as the judiciary. As policy theory reminds us, institutional systems tend to generate second-best equilibria rather than predictable, first-best outcomes (Lipsey and Lancaster 1956). Unintended consequences therefore arise not merely from design shortcomings, but from the endogenous reactions of actors who strategically adjust their behavior to new institutional constraints. In this sense, local variations in court access or credit allocation are not anomalies; they are manifestations of adaptive dynamics within a complex institutional system.

This interpretation resonates with Elinor Ostrom's (2005) insight that uniform, centrally designed policies often overlook contextual diversity, and that sustainable institutional reforms are more likely to succeed when grounded in polycentric and locally informed governance. By shedding light on these dynamics, this paper contributes to the broader discourse on how public policies shape cities and regions in complex and often unforeseen ways.

Appendix

See Tables 7, 8 and 9.

Table 7 Additional robustness test

Variables	(Log) Interest rate t				
	(1)	(2)	(3)	(4)	(5)
Treated t ## Post t	-0.0335** (0.0138)	-0.0359** (0.0140)	-0.0338** (0.0142)	-0.0375** (0.0146)	-0.0324** (0.0156)
Post t	-0.142*** (0.00695)	-0.137*** (0.00753)	-0.139*** (0.00709)	-0.136*** (0.00758)	-0.138*** (0.00847)
Entrepreneurial density t		0.000717 (0.00237)	0.000667 (0.00237)	0.000550 (0.00242)	-0.000700 (0.00309)
Fiscal density t		-5.64e-05 (0.000150)	-0.000157 (0.000143)	-0.000121 (0.000178)	-1.39e-05 (0.000289)
(Log) Income from dependent employment t		-0.218 (0.143)			
(Log) Income from real estate t			0.0754 (0.0867)		
(Log) Pension income t				-0.498*** (0.161)	
(Log) Income from SMEs' sales t					0.00958 (0.0111)
Constant	-2.714*** (0.00475)	1.590 (2.740)	-3.827*** (1.424)	6.830** (3.061)	-2.808*** (0.267)
Year (FE)	Yes	Yes	Yes	Yes	Yes
Municipality (FE)	Yes	Yes	Yes	Yes	Yes
F test	710.04	468.25	464.65	465.58	328.98
Prob > F	0.0000	0.0000	0.0000	0.0000	0.0000
R-squared	0.730	0.731	0.731	0.731	0.738
Observations	22,576	22,576	22,576	22,576	16,767

Analysis on the unintended impact of judicial geography reform on municipalities, relaxing the outliers' criteria (rate < 0.15)

OLS regression model with multiple fixed effects (i.e., *reghdfe*). Total number of individual taxpayers in each municipality as analytical weight. The "Treated" variable is omitted due to collinearity with the fixed effects. Robust standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8 Second additional robustness test

Variables	(Log) Interest rate t				
	(1)	(2)	(3)	(4)	(5)
Treated i ## Post t	-0.0383*** (0.0146)	-0.0405*** (0.0148)	-0.0395*** (0.0149)	-0.0417*** (0.0151)	-0.0409** (0.0169)
Post t	-0.142*** (0.00799)	-0.138*** (0.00850)	-0.141*** (0.00820)	-0.138*** (0.00850)	-0.136*** (0.00981)
Entrepreneurial density t		-0.00207 (0.00215)	-0.00211 (0.00217)	-0.00219 (0.00218)	-0.00249 (0.00291)
Fiscal density t		-3.88e-05 (0.000149)	-0.000124 (0.000141)	-0.000107 (0.000162)	-2.16e-05 (0.000305)
(Log) Income from dependent employment t		-0.221 (0.159)			
(Log) Income from real estate t			0.0154 (0.0995)		
(Log) Pension income t				-0.383** (0.172)	
(Log) Income from SMEs' sales t					0.00599 (0.0113)
Constant	-2.664*** (0.00515)	1.673 (3.031)	-2.796* (1.630)	4.666 (3.250)	-2.702*** (0.274)
Year (FE)	Yes	Yes	Yes	Yes	Yes
Municipality (FE)	Yes	Yes	Yes	Yes	Yes
F test	621.35	411.21	408.68	401.56	275.43
Prob > F	0.0000	0.0000	0.0000	0.0000	0.0000
R-squared	0.679	0.679	0.679	0.679	0.684
Observations	24,342	24,342	24,342	24,342	18,138

Analysis on the unintended impact of judicial geography reform on municipalities, without considering outliers

OLS regression model with multiple fixed effects (i.e., *reghdfe*). Total number of individual taxpayers in each municipality as analytical weight. The “Treated” variable is omitted due to collinearity with the fixed effects. Robust standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9 Event study: treated vs. control (dynamic effects)

Variables	(Log) Interest rate t (1)
t-2 (2014)	0.242*** (0.00932)
T-1 (2015)	0.145*** (0.00831)
T + 1 (2017)	-0.143*** (0.00883)
T + 2 (2018)	-0.145*** (0.00789)
t-2 (2014) # Treated (1)	0.0167 (0.0201)
T-1 (2015) # Treated (1)	0.0202 (0.0200)
T + 1 (2017) # Treated (1)	-0.0224 (0.0193)
T + 2 (2018) # Treated (1)	-0.0370** (0.0186)
Constant	-2.813*** (0.00480)
Municipalities (FE)	Yes
R-squared	0.6793
Observations	24,342

OLS regression model with multiple fixed effects (i.e., *regh-dfe*). Number of active limited SMEs used as analytic weight. The “Treated” variable is omitted due to collinearity with the fixed effects. Robust standard errors clustered at municipality level in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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Data availability Data about SMEs’ financial and economic information have restricted access, which is subordinate to the authorization of Bureau van Dijk (BvD). All other data are public, with open access.

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